SYSTEMATIC EMPIRICAL RESEARCH VERSUS CLINICAL CASE STUDIES: A VALID ANTAGONISM?

This paper considers the issue of systematic empirical research versus clinical case studies raised by Hoffman (2009). A rebuttal of Hoffman's arguments is offered, followed by an argument that each method addresses itself to different questions and that posing them in opposition is not fruitful. Finally, criteria and requirements of the case study method are proposed that, if met, would enhance its evidential value.

The overarching theme of Irwin Z. Hoffman's lead article in this journal (2009), based on a plenary address given at the 2007 winter meeting of the American Psychoanalytic Association, was to warn of the dangers attendant on granting privileged status to systematic empirical research over clinical case studies in the acquisition of psychoanalytic knowledge. The rhetorical title of his paper, "Doublethinking Our Way to 'Scientific' Legitimacy: The Desiccation of Human Experience," expresses quite clearly Hoffman's position.

Controversy regarding the place of research in psychoanalysis has a long history and, as Thomä (2010) has observed, has been intertwined with the question of the role of the university (versus free-standing institutes) in psychoanalytic training. At the Ninth Psychoanalytic Congress in 1925, Eitingon's argument against a university affiliation (and the demand for research and testing of ideas that it would bring) was "heralded with a powerful round of applause" (Gilman 2009, p. 1103)—not unlike the standing ovation Hoffman received following the delivery of his paper. The debate has not been entirely a one-sided one. Through the years there

---

Morris N. Eagle, Professor Emeritus, Derner Institute for Advanced Psychological Studies, Adelphi University; Distinguished Educator in Residence, California Lutheran University; faculty, New Center for Psychoanalysis, Los Angeles; faculty, Los Angeles Institute and Society for Psychoanalytic Studies. David L. Wolitzky, Associate Professor of Psychology, New York University.

Submitted for publication January 24, 2011.

DOI: 10.1177/0003065111416652

have been voices decrying the lack of accountability and systematic research. As an early example, cited by Thomä, Susan Isaacs wrote in 1939 that "the question of the criteria by which we test the validity of our convictions in analytical work is one of great practical importance in the day to day carrying on of our work. . . . It enters into the discussion of controversial issues between analysts. . . . Lastly it is of central importance in the statement of our theory for the non-analytic public, who have the right to challenge our premises and conclusions and to be shewn our methods of testing and verification" (p. 148).

In 1953, Knight decried the effect of lack of research on the state of psychoanalysis. More recently, Cooper (2008) has lamented the failure of most psychoanalytic institutes to teach candidates how to evaluate research, a failure that in his estimation has contributed to the decline in the intellectual and scientific standing of the field. He also views our contemporary pluralism as a "multiplicity of authoritarian orthodoxies . . . rather than a scientific discourse" (p. 235).

For the most part, at least until recently, these calls for accountability and systematic research have gone unheeded. Although a smattering of psychoanalytic research was carried out over the years, only during the last two decades or so has there emerged a small but significant cadre of researchers who have focused on psychoanalysis and psychoanalytic treatment—virtually all of whom, it should be noted, are associated with universities rather than free-standing psychoanalytic institutes. However, neither the calls for research over the years nor the recent emergence of significant psychoanalytic research has had much impact on psychoanalytic training. And yet Hoffman identifies the calls for research and the epistemological privileging of systematic research as looming dangers that will dominate and dictate psychoanalytic training and practice. If anything, however, the danger—not looming, but very much already present—is that the rejection of calls for accountability and research will result in the increasing marginalization of psychoanalysis. Despite this obvious threat to the future of psychoanalysis, Hoffman views attempts to respond to calls for research as caving in to political pressures, as "doublethinking one's way to 'scientific legitimacy' "—as if meeting demands for accountability through systematic research is not in itself an entirely legitimate aspect of the discipline's moral and professional responsibility, quite apart from outside pressures.

The audience's enthusiastic response to Hoffman's paper is understandable in the light of various threats arising from a variety of sources, such as pressures from HMOs and insurance companies, the homogenization of treatment through the use of manuals, and rigid criteria for so-called empirically valid treatments. The response is understandable also when one considers how unpalatable it must be to be told, in effect, that systematic empirical research carries greater epistemological weight than one's knowledge based on years of clinical experience. In that light, Hoffman's paper, which insists that at least equal epistemological warrant be given to case studies, would be experienced as personal validation. Although we agree with Hoffman that one should be cautious about granting epistemological privilege to systematic empirical research across the board, it should be privileged in relation to general questions of treatment outcome.

There is still another reason that Hoffman's paper would resonate with a psychoanalytic audience. The fact is that the findings of systematic empirical research are not always clear-cut and are often not of immediate and concrete use in clinical work. Green (Green and Stern 2000) is undoubtedly correct in observing that research findings appear meager in comparison to the richness of clinical experience. However, the function of systematic empirical research is not to capture the richness of clinical or any other experience, nor is it to offer specific and detailed prescriptions regarding how to conduct psychotherapy at any given moment. Rather, its main functions include putting our convictions—which indeed may be based on rich clinical experience—to the test, providing general guidelines, and generating general principles.

In any case, Hoffman's paper demands a careful critical evaluation, not only because the issues it raises have important implications for the future of psychoanalysis, but also because his views appear to reflect the attitudes and values of many analysts, as evidenced by that standing ovation. We believe that Hoffman's paper provides an important service in raising issues with which our discipline must grapple. However, because we believe also that Hoffman's views, as well as the rhetorical excesses through which he expresses them ("doublethinking," "desiccation of human experience"), are detrimental to the future of psychoanalysis and yet apparently are endorsed wholeheartedly by many of our colleagues, we think it important to offer a detailed critique of Hoffman's position and to present some general comments on the issues he raises.

In the first half of this paper, we will critically evaluate Hoffman's major arguments. In the second half, we will discuss issues that should, though neglected by Hoffman, be part of any discussion of the roles played by clinical work and research in advancing psychoanalytic knowledge. We hope these comments will be a corrective to Hoffman's unnecessarily polarized view of "empirical research" versus "clinical case studies."

REBUTTAL OF HOFFMAN'S MAIN ARGUMENTS

The "Privileged Status" of Systematic Empirical Research

Hoffman asserts that the "privileged status" accorded "systematic empirical research on psychoanalytic process and outcome. . . . as against in-depth case studies is unwarranted epistemologically and is potentially damaging both to the development of our understanding of the analytic process itself and to the quality of our clinical work" (p. 1043). It should be noted that in fact the majority of practicing analysts largely ignore the research literature and do not accord "privileged status" to systematic empirical research. So it is hard to imagine how or why it could be "potentially damaging . . . to the development of our understanding of the analytic process itself and to the quality of our clinical work." One might reasonably conclude, then, that Hoffman means that research could have potentially deleterious effects if it were taken seriously by practicing analysts.

A general debate pitting the epistemological status of systematic empirical research against that of case studies is fruitless. Each method makes different knowledge claims. If we want to know something about a particular person, we are likely better off turning to an in-depth case study. If, however, we want to determine the general outcome of a therapeutic approach, we should look to systematic empirical research. Such research may, of course, vary in quality, adequacy, and "ecological validity"—just as case studies may. However, as far as the question of means and methodology are concerned, for certain purposes the privileged status accorded systematic empirical research is epistemologically warranted, just as for other purposes (e.g., interpreting a patient's dream), the privileged status accorded the method of case studies is warranted.

It is, of course, true, as Hoffman argues, that group results may not be applicable to a particular patient and that we should therefore tread carefully in employing such results in understanding a particular case. But it does not follow that keeping in mind group results is necessarily irrelevant or harmful in trying to understand a particular patient. Consider a concrete example. There is a good deal of evidence that exposure to feared objects and situations is frequently an important factor in the treatment of phobias, including agoraphobia. We do not believe that Hoffman would suggest that consideration of this finding is irrelevant or harmful in treating a particular patient. Rather, Hoffman would simply maintain that keeping case study material in mind is more likely to be helpful (see Hoffman 1998).

Note that one can be quite critical of the DSM, the PDM, or any other diagnostic system, and nevertheless recognize the clinical importance of exposure in helping someone with agoraphobia. One can also recognize that the psychic role played by agoraphobia in one case might be different, in subtle ways, from its role in another case and nevertheless continue to recognize the potential importance of exposure. By the way, despite recognizing the dynamic significance of the symptom, Freud (1919) wrote that in cases of "severe" agoraphobia, "one succeeds only when one can induce them by the influence of the analysis . . . to go into the street and to struggle with their anxiety while they make the attempt" (p. 166). We do not know how frequently Freud's advice has been followed by analysts who work with agoraphobic patients.

It should be noted that though the case study, methodologically speaking, might appear to be a "privileged" route to understanding the individual, this is not necessarily true, in that case studies are often fraught with serious problems, an issue we will deal with in detail in the latter part of this paper. Although Hoffman is rightly concerned that systematic empirical research might impose ill-fitting data on the individual clinical case, in a somewhat different way a similar risk exists with regard to the case study. We do not believe that Hoffman would disagree with this claim so long as one, so to speak, equalizes the risks for both case studies and systematic empirical research. However, the clinical material included in the case study can be, and often is, strongly influenced by the theoretical orientation of the author. Also, as Spence (1990) has pointed out, the case study presented in the literature is a selected, normalized, smoothed-out version of the actual clinical material. Of all people, we analysts should be aware of the motivational factors that are likely to influence, sometimes unwittingly, the selective nature of the presentation of the clinical material (e.g., our theoretical allegiances).

Over the last few months, one of us has been carrying out supervision with the possibility of referring to audio-video recordings made of the therapy sessions. The experience has been a revelation. It is remarkable what is omitted from the therapist's reports and process notes, material of which the therapist is unaware and perhaps cannot be. It is now difficult to carry out ordinary supervision without thinking about what important material may be omitted. In extolling the strengths of the case study, Hoffman does not adequately address this problem and how it might be dealt with—unless, of course, one wants to argue that veridical recall of the session is not necessary for effective supervision.

In addressing the issue of bias in case studies, Hoffman argues that "the ambiguity of psychoanalytic data leave them relatively unmanipulable in the sense of stacking the cards in favor of one or another point of view." He claims that "the analyst can't force something ambiguous simply to support the view that he or she advocates. The ambiguity in itself ensures the openness of the 'data' to critical review and to multiple interpretations. Such data lend themselves to constructive dialogue among the reporting analyst and others. It's noteworthy that with all the concern about how the reporting analyst, in the interest of supporting his or her point of view, can skew both the course of the analytic work and the way in which it is described to others, in point of fact the data that are customarily presented do not seem to prevent people from mounting critiques of the work, from suggesting alternative formulations of what went on in the process, and from offering suggestions as to better ways the analyst might have intervened and participated" (p. 1052).

In our view, Hoffman's comments here further expose weaknesses in the case study method and defeat his defense of it. As Hoffman notes, the clinical data presented do not seem to prevent "alternative formulations of what went on in the process, and . . . suggestions as to better ways the analyst might have intervened and participated." We are all too familiar with these second-guessing responses at case presentations, characterized by each commentator offering his or her formulation of what went on in the process, what the presenting analyst missed, and so on, each alternative suggestion reflecting the commentator's favored theoretical approach. Are these presumed properties of clinical data and the case study method supposed to be strengths? We view them as serious problems that need to be confronted. We are reminded of Meehl's wonderful article "Why I Do Not Attend Case Conferences" (1973), in which he exposes the many ways in which clinicians engage in and tolerate feeble inferences.

The Issue of Accountability

There are many difficulties with systematic empirical research on psychotherapy process and outcome and the uses to which such research is sometimes put—reflected, for example, in the rigidity and narrowness of thinking that has resulted in the classifications of "empirically validated treatments." But some of the most cogent critiques of that kind of thinking have in fact come those who engage in systematic empirical research (e.g., Beutler 2009). A constructive way of dealing with the shortcomings of such research is to do better, more creative, and more ecologically valid research, not to launch attacks on any systematic research whatever or to accuse researchers of engaging in "doublethink."

Undergirding debates about this or that methodology is the fundamentally moral, as well as scientific, issue of accountability. Are various treatment approaches, including the psychoanalytic approach, effective in helping people according to a set of criteria? Do they do what they claim to do? How do they compare with rival treatments? Fortunately, these questions have been addressed with some degree of success in relation to various therapeutic approaches, including psychoanalysis. Hoffman does not acknowledge the value of such research, nor does he confront the question of accountability at all. Does he believe that individual case studies can successfully address the general issue of accountability? Or does he regard it as a non-issue, or an issue unfairly imposed on us by the scientific establishment and insurance companies? It is unclear how clinical case studies can deal adequately with this matter. Peer supervision and thoughtful reflection alone are not sufficient.

It is true that this issue has been exploited politically by HMOs, insurance companies, and the guardians of "empirically validated" treatments. However, this socioeconomic/political pressure does not make the question of accountability any less legitimate and fundamental. Indeed, the lack of interest in that question by analysts over many years has provided a fertile, and in many respects legitimate, ground for exploitation of the issue by hostile critics. Hoffman suggests that any attempt to respond to demands for accountability amounts to caving in to political pressure or, as Hoffman puts it, a "compromising of ourselves for practical ends" (p. 1057). He characterizes this as Orwellian "doublethinking" our way to "scientific legitimacy." This equivalence is then rhetorically buttressed by the citation of a long passage from Orwell's *1984,* Fonagy's warning (2002) that "objections to research will not win the day . . . no matter . . . the strength of opposing arguments" (p. 58), and Strupp's expression of disillusionment with the "science game" (2001, p. 615)—this last cited outside its original context, namely, opposition to the rigidity of the empirically supported treatment movement. There is an irony that in a paper concerned with deeply moral and human issues, Hoffman leaves no room for the relationship of systematic empirical research to the moral issue of accountability.

In summary, to suggest that research on therapy outcome is merely a capitulation to political pressure overlooks the point that even if society did not demand accountability, our own sense of moral and professional responsibility requires that we back up our assertions. As Gill (1994) observed, "We may be satisfied that our field is advancing, but psychoanalysis is the only significant branch of human knowledge and therapy that refuses to conform to the demand of Western civilization for some kind of systematic demonstration of its contentions" (p. 157).

An alternative to linking accountability to systematic empirical research is to relinquish any claim that psychoanalytic treatment is embedded in institutional and social requirements and privileges. Rather, psychoanalysis would be located entirely in what might be called a free-market economy. That is, psychoanalysis would be understood as operating in a framework in which two adults enter into an arrangement in which they talk to each other and one party pays the other; this goes on as long as both parties are willing to continue the arrangement. Outcome would essentially consist in "customer satisfaction." There would be no question of tax deductions or payment from insurance companies, in that it would not be entirely clear in what ways the transaction constitutes a treatment (see Hyman 1999). It should be clear that we do not endorse that direction.

Systematic Quantitative Research: A "Prescriptive, Authoritarian Objectivism"?

We are not entirely clear what Hoffman means by "objectivism." He has long been concerned with contrasting "objectivism" with "constructivism," as he does in this paper (see Hoffman 1991). Like *positivism, objectivism* has become a buzz word that means different things to different people. The term is vague and unclear but seems to refer to science, research, and interest in discovering objective truths. In any case, we assume that Hoffman is concerned that "systematic quantitative research" (p. 1045) will lead to authoritarian control over the nature of our training, what we will and will not be paid for, and so on. As noted earlier, these dangers indeed exist. However, is learning about what works in therapy entirely a matter of opinion, or does such knowledge have a status that, to some degree at least, transcends individual subjective attitudes?

In rejecting the privileged position of empirical research, one should have equal concern for the danger of what might be called "authoritarian subjectivism," that is, the belief that all that is necessary to justify or validate a therapeutic approach is one's subjective feelings and convictions or one's adherence and loyalty to a particular theoretical school. We know from

Hoffman's previous work that he is, indeed, alert to this danger. However, we believe that he does not adequately recognize the degree to which empirical research can mitigate this danger (more so in fact than clinical reports can). Consider the advice given by Greenberg and Mitchell (1983) that when it comes to embracing a theoretical point of view, adopt whatever theory "speaks to you." Given the plethora of schools and analytic institutes, one is likely to find one that "speaks to you," which will reinforce the conviction that subjective experience is the only (or at least the main) criterion for adopting a theoretical approach. Now, it may be that choosing an approach that generates the greatest emotional resonance contributes to being a better therapist. But note that the degree to which this might be the case can be determined only by systematic empirical research.

Learning What Works and How vs. Persuading the Powers That Be

This is patently a false dichotomy and begs the question by clearly implying that systematic empirical research is entirely in the business of persuasion, whereas case studies are in the business of learning about what works and how. Hoffman obviously is aware that systematic research concerns itself not only with outcome, but also with the process of therapy and the nature of therapeutic action.

Research Does Not Control for Who the Therapist Is

Hoffman complains that research does not control for who the therapist is (p. 1050). We are not entirely clear what he means by this. We assume his point is not that there is a failure to take into account therapist variables, for he knows there are scores of studies that do include that element in the research design. If he means that none of the research findings apply to an individual therapist, including himself, because neither he nor thousands of other therapists were included in the studies, he would once again seem to be emphasizing the "consequential uniqueness" of each analytic dyad. A logical implication of this position is that what he has learned about psychopathology and the analytic process with one patient has little if any application to his work with his next patient. That would mean he has no store of cumulative knowledge on which to draw. We doubt he really means this, but the point should be clarified. Hoffman does make an attempt to deal with the issue of cumulative knowledge (p. 1051), but in our view it is inadequate.

Although randomly controlled studies (RCTs) do not take into account "who the therapist is"—that is one of their deficiencies—as Beutler (2009) has pointed out, in more sophisticated studies one can take into account therapist characteristics, patient characteristics, the match between them, and the fit of a given form of treatment with patient characteristics. Thus, it is simply not true that systematic quantitative studies cannot control for "who the therapist is."

Psychotherapy Research Promotes Denial of the Sociopolitical Context

It is not clear how systematic empirical research, in presumed contrast to case studies, especially promotes "denial of the sociopolitical context of the phenomena being studied" (p. 1063).

Research Does Not Help the Clinician in the Consulting Room

Hoffman is correct in noting that empirical research fails to offer him immediate help when he is working in the consulting room. However, the desirability of conducting psychoanalytic research on the process and outcome of treatment and on basic processes posited by psychoanalytic theory should not rest on whether it offers immediate help to the analyst. For example, if in the midst of a session the analyst is conflicted about offering a countertransference-based self-disclosure, no body of research can serve as a specific guide. However, one can imagine the use of research findings as a general guide regarding the circumstances under which different kinds of deliberate self-disclosures, with different kinds of patients at different stages in treatment, facilitate or impede therapeutic progress. Similarly, if the analyst wishes to encourage the patient to overcome a reluctance to attend four sessions a week rather than three, the analyst can be guided by research findings on the relation between frequency of sessions and outcome. The recent finding that transference-focused interpretations were relatively more effective for patients with poor, rather than good, object relations (Høglend et al. 2007) could serve as a background consideration that might guide the therapist's interventions.

A number of examples attest the fact that research findings may contribute to improved patient care. One such example is the many years it took to depathologize homosexuality (Friedman 1988; Friedman and Downey 2008). Without the empirical research of Evelyn Hooker (1957, 1958, 1993) and others demonstrating that homosexuals showed no more evidence of psychopathology than heterosexuals, it would likely have taken much longer for minds to be changed. Even so, much needless suffering could have been avoided had analysts not clung to their pet theories in the face of controverting evidence. The same can be said regarding theories of autism that emphasized “refrigerator mothers” and theories of schizophrenia focusing on “schizophrenogenic” mothers. It may be that although both clinical/theoretical thinking and research-based scientific thinking can change in response to evidence, the former, for a variety of reasons, is in general far more slowly self-correcting than the latter.

Changes in Psychoanalytic Technique Are Not Based on Systematic Empirical Research

Hoffman observes that “compelling critiques of traditional approaches to psychoanalytic work *have* emerged and taken hold, as have changes in the way many analysts practice” and that these changes “owe little if anything to systematic empirical research” but rather “owe more to case presentations and to clinical experience and theorizing, as well as to changes in attitudes and values in our culture” (p. 1052). One wants to say: of course, that is precisely one of the problems with relying mainly or exclusively on case studies, which are often selectively invoked to support one’s theoretical approach. Hoffman seems to assume that “changes in the way many analysts practice” necessarily lead to “better” practice and are associated with better therapeutic outcome rather than being, at least in part, a reflection of changing fashions. As Hoffman himself notes, these developments are partly a product of “changes in attitudes and values in our culture.” One might add that they are probably influenced also by such factors as socioeconomic conditions, the availability of patients, and the plethora of therapies and therapists. In short, the fact that changes in the way many analysts practice owe little to systematic empirical research and a great deal to case studies tells us little about the value or validity of systematic empirical research or case studies or, for that matter, of changes in the way many analysts practice. It tells us only about the relative influence of systematic empirical research and case studies on the way many analysts practice—hardly a great surprise.

For more than a century, the question has been whether changes brought about by shifts in cultural attitudes, philosophical or political values, charismatic theorists, and so on should be considered accretions to knowledge and thus “progress” (e.g., improved outcomes or more valid theories of mental functioning) or should be regarded as being, to too great an extent, mere shifts in prevailing values and fashions (see Eagle and Wolitzky 1989).

There Are Many “Good” Ways of Being with Patients

Hoffman asserts that there are “multiple good ways to be, in the moment” in trying to help one’s patients (p. 1043). This sounds like a reasonable claim. No one today thinks that there is one singular, correct technique. However, it is not clear that this view requires a "nonobjectivistic hermeneutic paradigm" (p. 1043). After all, it is not as though researchers claim special authority with regard to "What is a good way to be in this moment?" or "What constitutes the good life?" (p. 1049). This is a straw man critique. Further, Hoffman's position leaves aside the researchable question of whether it might turn out to be the case that some ways of being "good" as a therapist are better than others. Here we would have to specify criteria for "good" and gradations of "good," as well as specifying when the encounter is no longer "good" but has turned "bad."

Should this just be a judgment call that Hoffman or others can make by reference only to material garnered by clinical impressions, or would it imperil the sanctity and ecological purity of the analytic situation if transcripts of sessions were rated for various factors (e.g., new memories, degree of affect expressed, quality of the alliance, degree of resistance, quality of self-reflection, etc.)? Would this really constitute a "desiccation" of human experience, or could we say it is an attempt to capture aspects of uniquely human experiences for the ultimate purpose of facilitating the fuller flowering of human potential? As noted earlier, to suggest that to measure aspects of human experience is, by that very act, to destroy the experience does not seem valid. To take just one example, in Luborsky and Auerbach's symptom-context method (1969), based on transcripts of sessions, the surrounding context of a clinical event (e.g., a report of a stomach pain) is compared with control segments in order to get clues about the themes associated with the reported event. It is hard to see how trying to learn something like that in a systematic way detracts at all from the analytic experience of therapist or patient. Such studies might eventually tell us that some ways of being with a patient (e.g., silence versus certain kinds of interventions) *might* be more beneficial than others, without imposing on the patient a view of the "good" life. (Of course, by the very act of being therapists we are at least implicitly saying that the "good" life entails self-reflection.)

Psychoanalysis and Social Consciousness

Hoffman seems to think that psychoanalysis has much to offer with regard to increasing "social consciousness and, ultimately, constructive political action" (p. 1062). It is not clear, however, what the basis is for believing that beyond participation as ordinary citizens, analysts possess special qualifications or privileged status as agents of social change and constructive political action.

Doublethinking Our Way to Scientific Legitimacy

We now have a sophisticated cadre of psychoanalytic researchers who are aware of the limitations of research and decidedly are not engaged in "doublethink," as Hoffman alleges they are. With regard to analytic research, investigators show a clear awareness of both the contributions and the misuse of their work. Analytic researchers recognize (a) the limitations of classification, (b) the limitations of randomized controlled trials (e.g., the limitations on patient selection), (c) the distinction between "efficacy" (high internal validity) and "effectiveness" (low external validity), (d) the distinction between statistical significance and clinical significance, and so on. There are also several features of research design that correct for biases. For example, Luborsky et al. (1999, 2002) note that in research reports there is a positive correlation between reports of positive outcome for a particular therapeutic outcome and the theoretical orientation of the investigators—the "allegiance effect." This obviously suggests a serious bias. However, we would emphasize that it is systematic research itself that has identified the bias, and that attempts can be made to control for and minimize this bias in subsequent studies. There are fewer safeguards for this kind of bias in clinical case studies. Strong, or even modest, claims of therapeutic effectiveness based only on clinical case reports can at least some of the time be dismissed as self-congratulatory testimonials.

The "Desiccation" of Human Experience

The tone and content of Hoffman's paper suggest that he regards virtually any use of categorization in relation to patients a "desiccation" of human experience. For example, although the authors of the PDM clearly are aware of the limitations and inevitable oversimplification of any classification system, of the artificial nature of the high comorbidities in DSM-IV-TR, and of the tendency to "reify complex syndromes" (PDM, quoted by Hoffman 2009, p. 1060), Hoffman views the PDM approach as merely a "nod to humanistic, existential respect for the uniqueness and limitless complexity of any person" (p. 1060) because, like the DSM, the PDM manual provides codes. But, as with the DSM, the vast majority of patients do not meet the full diagnostic criteria for a single disorder but show characteristics of several disorders.

There are a number of statements in the introduction to the PDM that reflect the desire to create a clinically meaningful approach while acknowledging the difficulties of doing so. For example, the authors state that there is "a healthy tension between the goals of capturing the complexity of clinical phenomena (functional understanding) and developing criteria that can be reliably judged and employed in research (descriptive understanding)" (PDM Task Force 2006, pp. 5–6).

In our view it is incorrect to suggest that psychoanalytic researchers are engaged in the "desiccation" of human experience because they are trying to measure aspects of it. Conducted in a clinically meaningful manner, such efforts and the research in which they are embedded are the best protection against authoritarian thought control precisely because this approach involves replicable empirical evidence rather than persuasive, charismatic appeals aimed at striking a resonant emotional chord in others.

Research and the Case Study Method

For what kinds of questions do answers from research deserve to be "privileged" over those offered by case studies? Obviously, not *all* questions about treatment can be answered through research (e.g., Hoffman's ninety-five-year-old patient). However, there are many important questions that we can answer better through systematic research than through clinical cases studies, or at least we can see the extent to what we know from clinical work squares with what we can learn from research. We also need to recognize that some relevant questions can never be answered adequately if we rely exclusively on the case study method.

Here are a few questions that research has made (or could make) valuable contributions toward answering:

- Is the optimal number of sessions per week different for patients with different diagnoses?
- Do transference-focused interpretations (compared to nontransference ones) made to borderline patients result in faster and more stable improvement in relationships and decreased self-destructive behavior? And how do the effects of transference-focused psychotherapy compare with those of rival treatments (e.g., dialectical behavior therapy)?
- What is the relationship between outcome and the quality of the alliance at different points in treatment?
- Do therapists adhering to different theories have different rates of success?
- What kinds of personality changes are more enduring when treated by psychoanalysis compared with other forms of treatment?
- What is the relationship of therapist warmth and empathy to outcome?
- Do therapists who adhere to a treatment manual generally achieve better outcomes than those who do not?
- Under what circumstances does countertransference disclosure reverse a previously stalemated treatment? (The literature is replete with case vignettes that purport to demonstrate this, but we have no base rate data telling us, for

example, what percentage of the time countertransference disclosure makes no difference in treatment progress.)

- Do certain symptoms (e.g., stomach pains) get reported in particular thematic contexts rather than in others? (If so, this would provide some insight into the kinds of conflicts associated with particular symptoms. This is precisely what Luborsky and Auerbach [1969] achieved in devising the "symptom-context" method comparing the material just preceding and just following the report of a stomach symptom compared with a control condition. This is the kind of study that could not be done using informal recollections of what patients said.)

Luborsky's work on "momentary forgetting" (1988) and on the Core Conflictual Relationship Theme (CCRT; Luborsky and Crits-Christoph 1998), Bucci's research on "referential activity" (Bucci and Maskit 2007), and the studies by Safran and Muran (2000) on ruptures and repairs of the therapeutic alliance are examples of systematic empirical research using psychoanalytic data that has yielded valuable information of a kind not possible to extract from clinical case studies.

As a final example, consider the role accorded to transference interpretations, long assumed to be an essential element in psychoanalysis and psychoanalytically oriented psychotherapy. How would we ever know if this assumption is valid or the conditions (e.g., type of patient, level of object relations, quality of the therapeutic alliance) under which it matters whether transference interpretations are part of the treatment? It is hard to imagine that we would ever know the answers to these questions merely on the basis of accumulated clinical experience. If we rely exclusively on the case study method, questions such as the optimal role of transference interpretation will be discussed in the literature a hundred years from now.

THE CASE STUDY METHOD

From its inception and throughout most of the twentieth century, the psychoanalytic case study has enjoyed privileged status, vis-à-vis systematic empirical research, as the means of establishing and advancing psychoanalytic knowledge.

We know that case reports frequently consist of vignettes selected to support an hypothesis rather than being a complete and faithful account of what transpired. Thus, years after Freud expressed his concerns, Anna Freud (1971) implied a similar uneasiness when she noted that "we cannot help being conscious . . . of a conspicuous . . . dearth of . . . complete and adequately documented case histories" (p. ix). As Michels (2000) has noted with regret, a survey of the fifteen most frequently cited psychoanalytic articles from 1969 to 1982 failed to find any extensive case study reports (Klumpner and Frank 1991). Other analysts, however, seem to feel that relying on selected case vignettes is fine, indeed preferable to full-length reports, because they provide a more vivid account of the analytic work (e.g., Stein 1988).

Regarding this issue of selectivity, Michels invites us to pay attention to the analyst's intentions in writing up a case and publishing it. When the intention is to offer evidence for an analytic hypothesis about the meaning of some aspect of the patient's behavior, many observers think it useful to have a tape and a transcript. On, the other hand, as Michels notes, Klumpner and Galatzer-Levy (1991), in a panel report of APsaA's Committee on Scientific Activities, comment that the preference for verbatim data is "scientism . . . the irrational veneration of what appears scientific rather than using scientific methods as tools" (p. 736). They state that "abandoning narratives would deprive us of the richly informative narrator's perspective" (p. 736). This view presents an unnecessary choice. It need not be either/or. Obviously the narrator's perspective can be "richly informative," and would be even more informative if accompanied by a record of the thoughts and feelings the analyst experienced during the sessions on which the narrative is based. At the same time, the "richness" would be enhanced by also having the verbatim material for others to study in a systematic fashion. In fact, comparing the analyst's narrative with what might emerge from a detailed study of the original data by independent observers could be quite illuminating and more "richly informative" than either source of data alone. Such an approach would reduce the common limitations of case studies: (1) distortion of case material and/or facts in the patient's history in the service of presenting a more compelling set of assertions; (2) unwitting distortion or selective memory of facts and/or clinical data in the service of offering a more persuasive case or as a result of countertransference reactions; (3) deliberate disguise of the patient's identity that results in the alteration of clinical data or facts about the patient's history that others might feel renders questionable some of the inferences drawn.

The Problem of Confirmatory Bias

A systematic empirical approach might shed light on the issue of biased weighting of clinical evidence. In this regard, one of us participated in a research project on clinical evidence in which several analysts studied the verbatim transcripts of numerous analytic sessions. The group, organized and led by Benjamin Rubinstein, met regularly. We started by reading the transcripts of the first five sessions. Any time a member of the group had an hypothesis to offer, we stopped and recorded the hypothesis and the observations on which it was based. In subsequent meetings, we read transcripts of randomly selected subsequent sessions. When a group member felt there was evidence in favor of or against the hypothesis, we stopped and rated the strength of the evidence. Two noteworthy findings emerged from this procedure. First, 98 percent of the ratings were in the positive direction, meaning that we rarely regarded an hypothesis to have been disconfirmed by the clinical material. Second, when we compared the strength-of-evidence rating of the person who had us stop to rate the evidence for a given hypothesis and compared that rating to the average rating for the other group members, the group rating was lower, with the exception of one analyst (out of eight in the research group).

What this analysis suggests is that the analyst who felt there was evidence for an hypothesis (which did not necessarily have to be the one he proposed originally) thought the evidence was stronger than did his colleagues. In short, there was an indication of what we might call a "confirmatory bias," expressed in our group by the tendency to give more weight to evidence than other colleagues feel is warranted. Another noteworthy finding is that it was quite rare (less than 5 percent of the time) for anyone to find negative evidence of an hypothesis. This finding is somewhat ambiguous in that it could reflect either confirmatory bias or the extraordinary clinical acumen of the clinicians! Extrapolating from these findings to the clinical situation, it is likely that (1) we rarely regard our initial hypotheses as disconfirmed or as not supported by further clinical observations, and (2) we give greater weight to apparently confirmatory evidence than is warranted. It seems reasonable to regard this as a limitation of the case study method. At the very least, this bias suggests room for improvement in the processing and reporting of case material.

Improving the Evidential Value of Case Studies

Even if one wants to maintain that case studies are all we need in psychoanalysis, one needs to identify the criteria by which case studies are deemed to yield reliable knowledge. Case studies should be accorded more evidential value to the extent that they demonstrate the following characteristics:

1. The ratio of theory to data is reasonable; i.e., there is not an excessive amount or theory superimposed on some fragment of data.
2. Observation is clearly separated from inference in the case report.
3. Alternative and rival hypotheses are considered seriously and the reasons for rejecting them presented.
4. The case report is relatively free of jargon.
5. The case report illuminates a phenomenon, justifies a particular technical approach or innovation, or argues cogently for an improved conceptualization of a familiar phenomenon.
6. Verbatim accounts are included when feasible.
7. The case formulation is internally consistent and coherent.
8. The report is sparse and tentative with respect to etiological claims.
9. Issues of generalizability are considered carefully.
10. The report is not based on a fictionalized case or a composite of several cases.
11. The author's theoretical orientation and preferences are clearly stated.
12. It is demonstrated that the inferences grow out of the material and are not imposed prematurely on the clinical observations, even if the vignette is selected to advance a particular point of view.
13. Caution is evident in cause-and-effect claims regarding the patient's dynamics.
14. Caution is evident in cause-and-effect claims regarding childhood causes of current problems.
15. The report reflects evidence of the author's having read and absorbed the cogent points in Meehl's "Why I Do Not Attend Case Conferences" (1973).
16. There is independent confirmation of some of the claims made.
17. There is follow-up information on the case that bears on some of the assertions put forward.
18. The author recognizes the issue of base rates; e.g., if it is alleged that a stalemate in a lengthy analysis was broken by a countertransference self-disclosure, the author states how often such self-disclosures did not seem to make a difference and perhaps offers some hypotheses in this regard (we also would need to know how often a stalemate is broken in the absence of countertransference-based self-disclosure).
19. The case study allows us to reject or disconfirm a psychoanalytic hypothesis.

We are not suggesting that Hoffman would disagree (or not) with these criteria. We want to demonstrate only that many case studies fail to meet almost all of them. If they met more of them, one could make a stronger case for their evidential value. As Kazdin (2001) notes, a serious commitment to patient care should include both a recognition of the limitations of informal clinical judgment and the use of supplementary methods of evaluation (see, e.g., Clement 2007). Wakefield (2007a,b), for example, provides a convincing example of Freud's distortion in the case of Little Hans, showing that Freud incorrectly claimed that the boy confirmed that the giraffe represented his father. This is a distortion based not on inexact memory but on a misreading of the case record. This famous case highlights the distinction between the accuracy or probative value of case material and the influence it can have on generations of clinicians.

Edelson (1984) mounted a spirited defense against Grünbaum's challenge (1974, 1982a,b, 1984) regarding the evidential merit of the case study method. Grünbaum, it will be recalled, pointed to the fallibility of memory, the selection bias of the analyst, and the factor of suggestion as rendering the data obtained in the analytic situation irrevocably contaminated and unusable as probative evidence for analytic claims. In the face of these difficulties, Edelson noted that many analysts have a sense of futility in meeting the standards for evidence. It was Edelson's impression(1984) that "this sense of inadequacy, and the despair that goes with it, may in some cases at least translate into abandonment of scientific for hermeneutic goals" (p. 157).

Edelson presented more than a dozen specific suggestions for strengthening the probative value of case studies, including "Seek falsification rather than confirmation in case studies"; "Use causal modeling and statistical controls"; "Predict responses by the analysand to an interpretation that have not previously been manifested and that are not suggested in the interpretation" (p. 158). Few clinicians, if any, have done the work urged by Edelson, who in the last sentence of his book noted that doing such work is "one of the responsibilities that goes with being a psychoanalyst."

CONCLUDING COMMENTS

Most observers would probably agree that empirical research and the case study each have certain advantages and certain liabilities and that as sources of data they can complement one another. From this perspective, one can ask what each approach can contribute to our understanding.

The advantages of the case study method are that (1) it enables us to study rare phenomena, (2) it generates insights and hypotheses about personality dynamics that are not readily elicited in other situations, (3) it suggests different kinds of interventions, and (4) it can disconfirm certain hypotheses by finding instances that run counter to a theory. Perhaps the main limitation of the method is that it does not offer a good way of choosing among alternative hypotheses. In addition, the data are often unreliable. Often they are fictionalized, composite, or selectively remembered accounts designed to make a point. Rarely is there an opportunity for others to examine the data on which the clinician's conclusions are based.

Although history has repeatedly shown that any method or procedure (whether research or clinical) can be misused, deliberately or unwittingly, for nefarious purposes (e.g., in the service of Orwellian, authoritarian thought control), the advantage of scientific method with its emphasis on accessibility to observable data, replication, and controlled conditions is, in principle, a better safeguard against being ruled by dogma and blind obedience than the point of view of a charismatic, persuasive clinical theorist. Empirical research enables us to reduce speculative inferences since it is easier to control bias and rule out alternative explanations here than in case studies.

A major disadvantage of much research is that the ecological validity of the phenomenon being studied might be excessively sacrificed in order to ensure the internal validity of the research design. To the extent that this is the case, external validity is limited. One compromise is to use clinical data collected in the naturalistic setting for a more systematic study than is possible for an individual therapist (see Luborsky and Auerbach's symptom-context method [1969]). As to Hoffman's objection that the individual therapist is left out of the equation, that need not be the case. For example, it is perfectly possible to study therapist differences in success rates across a sample of patients. We would then be able to answer such questions as (1) Do certain matches or mismatches in personality styles, values, attachment styles, and the like make a difference with respect to treatment outcome? (2) Do some therapists have consistently better or worse outcomes than others? To pursue the answers to such questions need in no way "desiccate" human experience, as Hoffman would have us believe it does. Nor would this line of investigation gainsay the "consequential uniqueness" of each analytic dyad.

Thus, arguing in terms of clinical case studies versus systematic empirical research is simplistic and fruitless. To decide which kind of approach and which kind of "evidence" to "privilege" we need to know the nature of the question that is being asked. The therapist's information-processing capacity has limits, as does introspection, and is subject to bias. This obvious fact does not denigrate the therapist any more than saying that one can see more through a microscope than with the naked eye.

Without objective measures applied by outside observers, even with the pooling of observations and memories across many therapists, we could not answer questions of etiology or of whether, on average, more frequent sessions, longer treatments, degree and frequency of therapist self-disclosure, and a host of other factors correlate with deeper analytic process and better oucomes

The long-reigning hegemony of the case study has made us vulnerable to some rather harsh reactions by scientifically minded colleagues, despite the fact that we are beginning to offer good answers to our critics (see, e.g., Shedler 2010). For instance, in an editorial preface to a blistering critique of clinical psychology recently published by Baker, McFall, and Shoham (2009), Mischel (2009) states that "the disconnect between much of clinical practice and the advances in psychological science is an unconscionable embarrassment for many reasons, and a case of professional cognitive dissonance with heavy costs" (p. i). He approvingly quotes Paul Meehl, who in one of his last public appearances, memorably noted that most clinical psychologists select their methods "like kids make choices in a candy store: They look around, maybe sample a bit, and choose what they like, whatever feels good to them" (p. i). Although the views of Mischel and of Baker and colleagues are too harsh an indictment of the clinical enterprise, they are not entirely without merit.

Lest Meehl's statement seem a totally unfair caricature, recall the advice (noted above) proffered by Greenberg and Mitchell (1983) in their now classic text. When it comes to embracing a theoretical point of view, these authors advised the practitioner to adopt whatever theory "speaks to you," that is, generates the greatest emotional resonance. This position is not even balanced by a suggestion that one read the relevant research literature to see whether the theory one resonates to has received at least some empirical support. In a similar vein, Mitchell (1998) characterized those who are concerned with the issue of evidence as suffering from what he sarcastically dubbed the "Grünbaum Syndrome," allegedly a pathological state of mind.

It is one thing to say that in the immediacy of the clinical situation, it is probably inevitable that the analyst will process the patient's material through the theoretical lenses that are most meaningful to the analyst. This probably is the only way one can proceed. But to hold up such an attitude as an ideal and to disparage research as basically useless will not advance psychoanalysis. One must appreciate that psychoanalysis is not only a method of treatment, but a theory of human nature, a theory that makes claims about etiology and psychopathology. Surely we cannot base such a theory solely on clinical impressions.

Unfortunately, a message conveyed in Hoffman's paper is that those who favor and privilege systematic research are guilty of a host of evils, including "authoritarian objectivism," lack of regard for the "whole person," dehumanizing diagnostic systems, determinism, doublethink, the inhuman practices of HMOs and insurance companies, damage to understanding the psychoanalytic process, bowing to the powers that be, and favoring a "conformist" psychoanalysis. In contrast, those who favor the case study method are identified and associated with the presumed virtues of a nonobjectivist, hermeneutic paradigm, constructivism, regard for the uniqueness of the individual and the "whole person," free will, the absence of doublethink, a critical psychoanalysis, a belief in human freedom and in the dignity of the individual, the meaningfulness of community, and the sacrosanct integrity of every moment of psychoanalytic experience (pp. 1064–1065). Does Hoffman really believe that those carrying out systematic empirical research or those who endow it with a privileged epistemological status are necessarily any less outraged at the practices of HMOs, insurance companies, and utilization review personnel than those who favor the case study method? That there is no necessary link between systematic empirical outcome research and the practices of HMOs and insurance companies is evident when one considers that such research might well demonstrate that effective treatment requires long-term therapy far beyond the number of sessions allotted by those entities.

Clinicians do have a valid point when they question the "real-life" value and relevance of much psychotherapy research. Indeed, similar skepticism is expressed not only by clinicians, but also by researchers themselves. For example, in a recent paper titled "Arbitrary Metrics," Kazdin (2006), a distinguished researcher, raises important issues that cut across a wide swath of psychotherapy research. Central among these is the "ecological validity" or "real-life" meaning of measures of therapeutic outcome. Take as an example, one that Kazdin cites, a change in ratings on the Beck Depression Inventory (BDI) from pre to post treatment. Let us assume that the change in scores is statistically significant. The question, however, is whether and in what ways it is "ecologically" significant. That is, what does a change in scores tell us about the quality of the individual's life? This is a fundamental question applicable to all psychotherapy research, as well as any area in which measurements are employed.

Clinicians do have a right to question how seriously they should take research findings that show that, compared to a control group, therapeutic approach *X* leads to statistically significant shifts in ratings on the Beck Depression Inventory. They also have the right to weigh and evaluate these findings in the light of their own and their colleagues' clinical experience. What they do not have a right to do is to ignore all research findings on the grounds that none are relevant to clinical work, particularly when they have made no effort to familiarize themselves with those findings. Also, the profession to which clinicians belong does not have the right to ignore questions of accountability—or, more important, the relationship between process and outcome—or to leave these questions to be dealt with in an informal, helter-skelter way. Knowledge and convictions gained from clinical experience should not be ignored and should be taken quite seriously. However, as Meehl (1997) has pointed out, virtually every therapeutic intervention in human history has been accompanied by convictions, testimonials, and presumed knowledge.

Both clinicians and researchers have much work to do. Researchers need to take the problem of "arbitrary metrics" very seriously, not only in order to address the skepticism of clinicians, but primarily because an area such as psychotherapy research stands or falls as a function of its ecological validity. And clinicians need to become intelligent, informed, and critical research consumers. The alternative to scientism or poor or ecologically invalid research is better science and more ecologically valid research, not a Masada-like rejection of research. Instead of this polarized view, what we need at this juncture in our history is not an adversarial relationship between clinicians and researchers, between those who favor and privilege systematic empirical research and those who favor and privilege case studies, but rather a joint effort to find the legitimate and constructive uses of each methodological approach. One of Hoffman's major concerns is that systematic empirical research not be given empistemological privilege automatically and uncritically vis-à-vis case studies. We consider this a legitimate concern. Perhaps agreement could be reached if a systematic effort were made to identify the contexts in which each would merit such privilege.

REFERENCES

Baker, T.B., McFall, R.M., & Shoham, V. (2008). Current status and future prospects of clinical psychology: Toward a scientifically principled approach to mental and behavioral health care. *Psychological Science in the Public Interest* 9:67–103.

Beutler, L.E. (2009). Making science matter in clinical practice: Redefining psychotherapy. *Clinical Psychology: Science & Practice* 16:301–317.

Bucci, W. (1997). *Psychoanalysis and Cognitive Science: A Multiple Code Theory.* New York: Guilford Press.

Bucci, W., & Maskit, B. (2005). Building a weighted dictionary for referetial activity. In *Computing Attitudes and Affects in Texts,* ed. Y. Qu, J.G. Shanahan, & J. Wiebe. Dordrecht: Springer, pp. 49–60.

Clement, P.W. (2007). Story of "Hope": Successful treatment of obsessive-compulsive disorder. *Pragmatic Case Studies in Psychotherapy*, http://pcsp.libraries.rutgers.edu, Vol. 3, Module 4, Article 1, pp. 1–36.

Cooper, A.M. (2008). American psychoanalysis today: A plurality of orthodoxies. *Journal of the American Academy of Psychoanalysis & Dynamic Psychiatry* 36:235–253.

Eagle, M.N., & Wolitzky, D.L. (1989). The idea of progress in psychoanalysis. *Psychoanalysis & Contemporary Thought* 12:27–72.

Edelson, M. (1984). *Hypothesis and Evidence in Psychoanalysis*. Chicago: University of Chicago Press.

Fonagy, P. (2002). Evidence-based medicine and its justifications. In *Outcomes of Psychoanalytic Treatment: Perspectives for Therapists and Researchers,* ed. M. Leuzinger-Bohleber & M. Target. Philadelphia: Whurr Publishers, pp. 53–59.

Freud, A. (1971). Foreword. In *The Wolf Man, by The Wolf-Man.* New York: Basic Books, pp. ix-xii.

Freud, S. (1919). Lines of advance in psychoanalytic therapy. *Standard Edition* 17:157–168.

Friedman, R.C. (1988). *Male Homosexuality: A Contemporary Psychoanalytic Perspective*. New Haven: Yale University Press.

Friedman, R.C., & Downey, J. (2008). Sexual differentiation of behavior: The foundation of a developmental model of psychosexuality. *Journal of the American Psychoanalytic Association* 56:147–175.

Gill, M.M. (1994). *Psychoanalysis in Transition: A Personal View*. Hillsdale, NJ: Analytic Press.

Gilman, S.L. (2009). Psychoanalysis in the university: The clinical dimension. *International Journal of Psychoanalysis* 90:1103–1105.

Green, A., & Stern, D.N. (2000). *Clinical and Observational Psychoanalytic Research: Roots of a Controversy.* London: Karnac Books.

Greenberg, J., & Mitchell, S. (1983). *Object Relations in Psychoanalytic Theory*. Cambridge: Harvard University Press.

Grünbaum, A. (1974). Epistemological liabilities of the clinical appraisal of psychoanalytic theory. *Psychoanalysis & Contemporary Thought* 2:451–526.

Grünbaum, A. (1982a). Can psychoanalytic theory be cogently tested "on the couch"? Part I. *Psychoanalysis & Contemporary Thought* 5:155–255.

Grünbaum, A. (1982b). Can psychoanalytic theory be cogently tested "on the couch"? Part II. *Psychoanalysis & Contemporary Thought* 5:311–436.

Grünbaum, A. (1984). *The Foundations of Psychoanalysis: A Philosophical Critique.* Berkeley: University of California Press.

Hoffman, I.Z. (1991). Discussion: Toward a social-constructivist view of the psychoanalytic situation. *Psychoanalytic Dialogues* 1:74–105.

Hoffman, I.Z. (1998). *Ritual and Spontaneity in the Psychoanalytic Process: A Dialectical-Constructivist View.* Hillsdale, NJ: Analytic Press.

Hoffman, I.Z. (2009). Doublethinking our way to "scientific legitimacy": The desiccation of human experience. *Journal of the American Psychoanalytic Association* 57:1043–1069.

Høglend, P., Johansson, P., Marble, A., Bøgwald, K.-P., & Amlo, S. (2007). Moderators of the effects of transference interpretations in brief dynamic psychotherapy. *Psychotherapy Research* 17:160–171.

Hooker, E. (1957). The adjustment of the male overt homosexual. *Journal of Projective Techniques* 21:18–31.

Hooker, E. (1958). Male homosexuality in the Rorschach. *Journal of Projective Techniques* 22:33–54.

Hooker, E. (1993). Reflections of a 40-year exploration. *American Psychologist* 48:450–453.

Hyman, M. (1999). Why psychoanalysis is not a health care profession. In *Psychoanalytic Therapy as Health Care,* ed. H. Kaley, M.N. Eagle, & D.L. Wolitzky. Hillsdale, NJ: Analytic Press, pp. 53–63.

Isaacs, S. (1939). Criteria for interpretation. *International Journal of Psychoanalysis* 20:148–160.

Kazdin, A.E. (2001). Bridging the enormous gaps of theory with therapy research and practice. *Journal of Clinical & Child Psychology* 30:59–66.

Kazdin, A.E. (2006). Arbitrary metrics: Implications for identifying evidence-based treatments. *American Psychologist* 61:42–49.

Klumpner, G.H., & Frank, A. (1991). On methods of reporting clinical material. *Journal of the American Psychoanalytic Association* 39:537–551.

Klumpner, G.H., & Galatzer-Levy, R.M. (1991). Presentation of clinical experience *Journal of the American Psychoanalytic Association* 39:727–740.

Knight, R.P. (1953). The present status of organized psychoanalysis in the United States. *Journal of the American Psychoanalytic Association* 1:197–201.

Luborsky, L. (1988). Recurrent momentary forgetting: Its content and context. In *Psychodynamics and Cognition,* ed M. Horowitz. Chicago: University of Chicago Press, pp. 223–251.

Luborsky, L., & Auerbach, A.H. (1969). The symptom-context method: Quantitative studies of symptom formation in psychotherapy. *Journal of the American Psychoanalytic Association* 17:68–99.

Luborsky, L., & Crits-Chrisoph, P. (1998). *Understanding Transference: The Core Conflictual Relationship Theme Method*. 2nd ed. Washington, DC: American Psychological Association.

Luborsky, L.,Diguer, L., Seligman, D.A., Rosenthal, R., Krause, E.D., Johnson, S., Halperin, G.,Bishop, M., Berman, J.S., & Schweitzer, E. (1999). The researcher's own therapy allegiances: A "wild card" in comparisons of treatment efficacy. *Clinical Psychology: Science & Practice* 6:95–106.

Luborsky, L., Rosenthal, R., Diguer, L., Andrusyna, T.P., Berman, J.S., Levitt, J.T., Seligman, D.A., & Krause, E.D. (2002). The dodo bird verdict is alive and well— mostly. *Clinical Psychology & Practice* 9:2–12.

Meehl, P.E. (1973). Why I do not attend case conferences. In *Psychodiagnosis: Selected Papers*. Minneapolis: University of Minnesota Press, pp. 225–302.

Meehl, P.E. (1997). Credentialed persons, credentialed knowledge. *Clinical Psychology: Science & Practice* 4:91–98.

Michels, R. (2000). The case history. *Journal of the American Psychoanalytic Association* 48:355–375.

Mischel, W. (2008). Connecting clinical practice to scientific progress. *Psychological Science in the Public Interest* 9(2):i–ii.

Mitchell, S.A. (1998). The analyst's knowledge and authority. *Psychoanalytic Quarterly* 67:1–31.

PDM Task Force (2006). *Psychodynamic Diagnostic Manual (PDM)*. Silver Spring, MD: Alliance of Psychoanalytic Organizations.

Safran. J., & Muran, J.C. (2000). *Negotiating the Therapeutic Alliance: A Relational Treatment Guide.* New York: Guilford Press.

Shedler, J. (2010). The efficacy of psychodynamic psychotherapy. *American Psychologist* 65:98–109.

Spence, D.P. (1990). The rhetorical voice of psychoanalysis. *Journal of the American Psychoanalytic Association* 38:579–603.

Stein, M.H. (1988). Writing about psychoanalysis: I. Analysts who write and those who do not. *Journal of the American Psychoanalytic Association* 36:105–124.

Strupp, H.H. (2001). Implications of the empirically supported treatment movement for psychoanalysis. *Psychoanalytic Dialogues* 11:605–619.

Thomä, H. (2010). Remarks on the first century of the International Psychoanalytic Association (IPA) and a utopian vision of its future. Unpublished manuscript.

Wakefield, J.C. (2007a). Little Hans and attachment theory: Bowlby's hypothesis reconsidered in light of new evidence from the Freud Archives. *Psychoanalytic Study of the Child* 62:61–91.

Wakefield, J.C. (2007b). Max Graf's "Reminiscences of Professor Sigmund Freud" revisited: New evidence from the Freud Archives. *Psychoanalytic Quarterly* 76:149–192.

Morris N. Eagle
4351 Redwood Avenue, #1
Marina Del Rey, CA 90292
E-mail: meagle100@aol.com