

The Impending Death of Psychoanalysis

From Destructive Obfuscation to Constructive Dialogue

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The disconnection between psychoanalysis and mainstream psychology has reached the point that the long-term health of psychoanalytic theory is in serious jeopardy. “The Impending Death of Psychoanalysis” (Bornstein, 2001) was intended as a wake-up call to the author’s psychoanalytic colleagues who choose not to use relevant research findings from within and outside the discipline in their theoretical and clinical work. However, some of those who responded to the article misperceived it as an attack on psychoanalysis. This article points out factual errors in the responses of these critics, corrects some of the distortions and misrepresentations that characterize their critiques, and places the debate within an appropriate historical context.

Imagine a cardiologist who argued that controlled empirical research was irrelevant to her field. Suppose this cardiologist contended that she did not need to attend to findings from biochemistry and physiology, and that instead of using the most rigorous research methods available, her discipline should be judged by its own self-generated outcome criteria, though

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these criteria are not used in any other area of medicine. Would you go to such a cardiologist for treatment?

Of course not. Such a cardiologist would be ostracized by her peers, shunned by the professional community, and subjected to a variety of formal sanctions to prevent her from harming unsuspecting patients. Let us ask ourselves, then: If such behavior is unacceptable in a cardiologist, why is it acceptable in a psychoanalyst?

“The Impending Death of Psychoanalysis” (Bornstein, 2001) was intended as a wake-up call to my psychoanalytic colleagues, not (as some critics contend) an attack upon a theory that has been the centerpiece of my work for nearly two decades. As the writers in this special section noted, the language of my 2001 paper was direct—even a bit jarring in places. That was intentional. Many of these issues were raised in earlier critiques—both mine (e.g., Bornstein, 1996b) and those of my colleagues (e.g., Fisher & Greenberg, 1996; Masling & Cohen, 1987). For the most part, the earlier critiques were ignored by nonacademic psychoanalysts. Because I believe that the disconnection between psychoanalysis and mainstream psychology has reached the point that the long-term health of psychoanalytic theory is in serious jeopardy, I chose to summarize my views in strong, unambiguous terms.

Of the four writers who contributed to this special section, only Karon (2002) placed “The Impending Death of Psychoanalysis” in its appropriate context: It is the latest in a series of writings that support those aspects of psychoanalysis that are empirically sound and clinically useful (e.g., Bornstein, 1999) and criticize those that are contradicted by the results of laboratory and clinical studies (e.g., Bornstein, 1993). Thus, of the four articles in this series, only Karon’s response represents a constructive dialogue regarding the issues at hand. The other three articles distort my work and that of others, obscure (rather than illuminate) the key issues in this debate, and illustrate (rather than disprove) the importance of an empirically sound psychoanalysis that incorporates relevant research findings from within and outside the discipline. The purpose of this article is to point out factual errors in the responses of my critics, correct some of the distortions and misrepresentations that characterize their critiques, and place the entire debate within an appropriate historical context.

The Denigrate-the-Messenger Strategy

Rather than dealing with the issues at hand, Waiess’s (2002) response consists primarily of a series of ad hominem attacks. She likens my writing

to that of “hate groups” (p. 561), suggests that I am promulgating an “agenda of hatred” (p. 561), and argues that I aim to “manipulate the compliance of group members through fear” (p. 562). Waieess asserts that I have had “a bitter experience at [my] university” (pp. 562–563) and am “blaming someone or something else for [my] despair” (p. 563). In a particularly bizarre passage, Waieess notes that psychoanalysis “cannot spare anyone from eventual death” (p. 562) and attributes my writing this article to some sort of ill-defined death wish.

Waieess’s (2002) assertions notwithstanding, the rumors of my death wish are greatly exaggerated. Leaving aside the fact that my motives for writing this article are irrelevant to the validity of its central premise, the kinds of ad hominem statements that form the core of Waieess’s response are simply unacceptable in professional discourse. They are at best a distraction from the issues at hand, and at worst a deliberate attempt to obscure the facts by denigrating the messenger.

Waieess’s (2002) attempt to psychoanalyze me in print is not only unacceptable from a professional standpoint, it is also a poor use of psychoanalytic methods. A valid psychohistory must draw upon a broad range of data sources, not a single document (see Ihanus, 2001; Simonton, 1998). If “The Impending Death of Psychoanalysis” was the only article I had written, then psychoanalyzing me in print on the basis of its content would be empirically (though not ethically) justifiable. However, Waieess chose to ignore literally dozens of other published writings that illustrate my public views on this topic (and presumably my private motives as well). These include numerous articles from *Psychoanalytic Psychology* (e.g., Bornstein, 1996a, 1999), a series of editorials from the *Bulletin of the Psychoanalytic Research Society* (e.g., Bornstein 1995, 1997), and several introductory chapters from the Empirical Studies of Psychoanalytic Theories book series (e.g., Bornstein & Masling, 1998; Masling & Bornstein, 1996).

Waieess (2002) holds up Sue, Sue, and Sue’s (2000) abnormal psychology text as an example of a book that is “fair to psychoanalysis” (p. 561). I agree. In this context, it is useful to examine Sue et al.’s (2000) assessment of the empirical status of psychoanalytic theory. They write:

The empirical procedures by which Freud validated his hypotheses have grave shortcomings. His observations about human behavior were often made under uncontrolled conditions. For example, he relied heavily on case studies and his own self-analysis as a basis for formulating theory. . . . He seldom submitted the material related by his patients to any form of external corroboration—statements from relatives or friends, test data, documents, or medical records.

Using such private and uncontrolled methods of inquiry as a basis for theory is fraught with hazards. (p. 44)

Sue et al.'s (2000) assessment of the empirical status of psychoanalysis is virtually identical to mine. Why theirs is "fair" (p. 561) while mine "crossed a boundary of decency" (p. 559) is anyone's guess (Waieess, 2002).¹

The All-Must-Have-Prizes Approach

Instead of addressing my contention that certain aspects of psychoanalytic theory and practice are contradicted by empirical findings from within and outside the discipline, Mills (2002) adopts the "Lake Wobegone model" of evaluating research: All methods of inquiry are equally valid, all are above average, all must have prizes. Thus, Mills asserts,

it is simply bias and fallacious to equate truth or an absolute standard with empirical methodology at the expense of other equally viable and philosophically defensible methods such as discursive, dialectical, qualitative, phenomenological, hermeneutic, linguistic, historical, post-structuralist, social constructivist, narrative, deconstructivist, feminist, and logical approaches—each with their respective criteria. (p. 554)

Although many of these perspectives can indeed illuminate important psychological issues, most are not useful methods for generating replicable, generalizable empirical results in the laboratory or clinical setting. Mills's (2002) argument illustrates precisely what is wrong with many psychoanalysts' current approach to theory validation: If one can simply select whichever method of inquiry yields the desired conclusion, one can always find "evidence" to support whatever conclusion one favored in the first place.

Mills (2002) uses similar logic to defend the present status of psychoanalysis in contemporary psychology. Denigrating the "propaganda of scientific idolatry" (p. 557), Mills contends without evidence that "hun-

¹A few other errors in Waieess's (2002) comment are worth mentioning. She contends (p. 560) that the great Western shift toward individual rights occurred in the 19th century. In fact, it took place during the 18th century. She asserts that psychoanalytic treatment "was proven to be effective a very long time ago" (p. 562), but sound evidence in this regard only emerged within the past 20 years (see Blatt & Ford, 1994; Weiss & Sampson, 1986). Finally, Waieess faults me for suggesting that the psychoanalytic corpse should be "dismembered" (p. 560), but I never suggested that. To extract its organs, the corpse must be disemboweled.

dreds of nonempirically oriented subfields in the humanities and social-behavioral sciences . . . are embracing psychoanalytic principles” (p. 557). Then—ignoring data that illustrate the diminished influence of psychoanalytic concepts in American Psychological Association (APA)-approved graduate programs (APA, 1998; Bornstein, 2001)—Mills points to “the proliferation of psychoanalytic institutes and associations that are popping up in every major North American city” (p. 553) as evidence for the long-term health of psychoanalysis.

I turned to a recent copy of the Washington, DC, telephone book to test Mills’s (2002) contention regarding the “proliferation of psychoanalytic institutes and associations that are popping up in every major North American city.” Four psychoanalytic institutes were listed in the Washington, DC, phone book—not bad for a city of this size. Further exploration helped place this number in context. Although there are indeed four psychoanalytic institutes in Washington, DC, there are eight institutes devoted to parapsychology and psychic studies. Clearly, a head count of institutes devoted to a particular theory or technique is not the best way of assessing the validity of that approach.²

Dismissing a wealth of laboratory evidence examining the clinical utility of free association (Erdelyi, 1985) and the parameters of repression (Holmes, 1990), Mills (2002) argues that “empiricism becomes a fundamentally bankrupt criterion when applied to psychoanalytic concepts that by definition cannot be directly observed or measured” (p. 556). This statement is incorrect. Every subfield of psychology—cognitive, social, clinical, developmental, even physiological—examines constructs that cannot be directly observed or measured (e.g., memory, motivation, thought, emotion). The same is true of physics (gravity), chemistry (molecular bonding), and biology (natural selection). Each field has developed innovative methods for operationally defining those “unobservable” constructs that form the foundation of their discipline, often using indirect strategies to measure and quantify those constructs (e.g., by assessing the effect of an unobservable construct on other, observable variables).

If physicists can do it, so can we. Psychoanalysis is no different from

²Mills’s (2002) inattention to the declining influence of psychoanalysis in graduate programs prompted me to revisit this issue and update my data. In 1998, 5% of APA-approved graduate programs described themselves as emphasizing psychoanalysis (vs. 24% that emphasized behavioral training and 71% that emphasized a cognitive-behavioral approach; APA, 1998). Two years later, these numbers had changed: Now 4% of programs emphasized psychoanalysis, 21% were behavioral, and 76% were cognitive-behavioral (APA, 2000).

any field in this regard, and to argue otherwise is to endow psychoanalytic theory with a kind of intellectual uniqueness (and immunity from empirical scrutiny) that cannot be justified philosophically or demonstrated empirically.

Two other errors in fact put forth by Mills (2002) are worth noting, because they illustrate the importance of a rigorous empirical approach to psychoanalytic therapy as well as the theory that underlies it. Mills claims that I said “psychoanalysis must be wrong” (p. 553), because “empirically controlled science unsubstantiates hence negates psychoanalysis” (p. 554). In fact, I said neither of these things. If a psychoanalyst creates false memories of statements from a published article—with benefit of a written record to check these memories—how much greater must be the omissions and distortions that occur during an analytic session, where there is no objective record of the transactions that took place? Psychoanalytic psychologists who choose to remain informed about research on memory construction—and the inescapable distortions that characterize even short-term recall—can take steps to maximize the accuracy of their recollections (Schacter, 1996). Those who ignore such research findings do so at their own risk and that of their patients.³

The Have-It-Both-Ways Technique

The central premise of Lothane’s (2002) comment is that although psychoanalysis is a science, it is a unique science that need not fulfill the criteria for precision and testability to which other scientific fields are held. Thus, Lothane seeks to elevate psychoanalytic theory to scientific status (with all the intellectual prestige that such status brings), while jettisoning those pesky scientific requirements of empirical scrutiny and rigorous hypothesis testing.

Would that it was possible to have it both ways, but sadly it is not. A field is not scientific because its adherents declare it so, nor may devotees of a particular theoretical perspective choose their own standards

³Two other factual errors in Mills’s (2002) comment warrant brief mention. First, he asserts that Exner’s Comprehensive System (CS) “precisely sets out to uncover, measure, and interpret . . . unconscious processes” (p. 554). In fact, the CS is designed to assess implicit motives, cognitive–perceptual style, and aspects of the person’s coping style, but not the sorts of unconscious processes to which Mills refers (see Weiner, 2000). Second, Mills contends that prior to the arrival of William James, “science *was* philosophy” (p. 556). This is, of course, wrong.

for empirical validity. A discipline is scientific to the extent that it yields replicable data, modifies theoretical propositions based on these data, and embeds itself in the broader nomological network of other scientific disciplines (see Proctor & Capaldi, 2001). Just as chemistry must generate data that are consistent with those of biology and physics, psychoanalysis must generate data that are consistent with those of its neighboring fields (e.g., experimental psychology, developmental psychology, neuroscience).

Lothane's (2002) have-it-both-ways strategy relies on a series of false dichotomies and internal inconsistencies that undermine his central tenet. Thus, at one point Lothane asserts that psychoanalysis "colligates individual observations into a science of the particular and of the universal, and is thus counted among the natural sciences" (p. 577), but elsewhere he argues that psychoanalysis "should not be judged by methods that apply to other sciences. It is not an exact science, it cannot quantify everything it studies" (p. 574). These two conflicting statements cannot simultaneously be true.

Another inconsistency emerges in Lothane's (2002) discussion of the relation between theory and method. He writes: "[Bornstein] does not understand the difference between a theory of the method, that is, a way of doing, and a theory of disorder, or an etiological hypothesis formulated for the sake of explaining the disorder" (p. 575). Lothane goes on to suggest that "theories, or hypotheses, of causation of disorder come and go; the method endures" (p. 576).

Lothane's (2002) first statement misrepresents me, and his second statement is factually incorrect. Therapeutic methods can only be effective insofar as they (a) are derived from an etiological model of the disorder in question and (b) include interventions that strive to effect change based on this etiological model. Thus, each variant of cognitive therapy is based on a set of assumptions regarding the pathogenic effects of dysfunctional cognitions and maladaptive thought patterns. Each psychopharmacological intervention seeks to correct a chemical imbalance that is presumed to underlie the disorder being treated. Psychoanalysis is no different, no matter how vociferously one protests otherwise.⁴

⁴Several other errors in Lothane's (2002) article are worth noting. He accuses me of disparaging psychoanalysis using "words and phrases encountered in the popular press" (p. 573). In fact, I did not. He disparages my earlier article because "there is not even one quote from Freud in his article" (p. 573). Lothane does not quote Freud either, and it is unclear why this detracts from my argument but not from his. Finally, Lothane

Karon's Constructive Dialogue

Alone among those who chose to respond formally to my 2001 article, Karon (2002) recognized it for what it is: A call for action designed to alert psychoanalysts to the diminished influence of our discipline in mainstream psychology and increase our efforts to validate and refine the theory before it is too late. Karon's response represents exactly the sort of constructive dialogue that can save psychoanalysis.

Karon (2002) and I agree regarding some important issues. For example, we concur that medication treatments have been oversold, and their purported efficacy exaggerated (see, e.g., Greenberg, Bornstein, Greenberg & Fisher, 1992). In addition, we agree that some research psychologists have unconsciously co-opted and renamed psychoanalytic ideas, reinventing them in the context of their own subfields (see Bornstein, 1996b, in press).

Karon (2002) and I also disagree regarding certain issues, and it is important that we recognize these as well. For example, we disagree regarding the relative importance of clinical and experimental evidence in theory building and hypothesis testing. We disagree regarding extant empirical support for repression, free association, and dream analysis. Karon offers the case of a patient "who could not remember anything before the second year of high school" (p. 565) as evidence for repression and castration anxiety. I believe this case is unconvincing and that it actually illustrates the value of a more critical, research-driven approach to deconstructing a patient's verbalizations.

Finally, Karon (2002) and I disagree regarding Seligman's (1995) *Consumer Reports* data. Although Karon contends that "ninety percent of those who had received psychotherapy said it helped" (p. 567), this is incorrect. In fact, only 4% of the original survey sample returned usable mental health treatment data for this study. True, 90% of the 4% of participants who returned the mental health portion of their surveys said therapy helped, but that figure represents between 3% and 4% of the overall sample, and—given the base rate of outpatient treatment today—almost certainly excludes a sizeable number of therapy receivers. As I have noted elsewhere, a questionnaire-based psychotherapy outcome

infers bias on my part because "there is no mention of any prominent pro-Freudian scholar" (p. 573) in my 2001 article. He fails to acknowledge (and might not realize) that in my other writings related to this topic, I have cited the work of these scholars numerous times.

study with a 4% return rate does not allow any substantive conclusions to be drawn regarding the variables investigated (see Bornstein, 1997).⁵

Conclusion

If psychoanalysis is to survive for another 100 years, we must find better ways to combine idiographic and nomothetic research methods to advance theory and refine technique. A cohesive, integrated research strategy is needed, based on three principles. These are: (a) use clinical data to generate testable hypotheses regarding psychological development and dynamics; (b) test these hypotheses via a combination of idiographic and nomothetic techniques; and (c) look outside the discipline continuously to be sure that our ideas are consistent with the findings obtained in other, related fields. Where inconsistencies arise between idiographic and nomothetic results, we must explore these inconsistencies rather than ignore them. Where inconsistencies arise between psychoanalytic findings and findings from other fields, we must scrutinize these inconsistencies to ascertain whether the difficulty lies within psychoanalysis, outside psychoanalysis, or both.

Contrary to the assertions of Waiss, Mills, and Lothane, psychoanalytic ideas are amenable to empirical testing using nomothetic research techniques: More than 2,000 published studies attest to this fact (see Barron, Eagle & Wolitzky, 1992; Fisher & Greenberg, 1996; Shapiro & Emde, 1995). Why the resistance? However one may choose to label it, this resistance is not a product of personal values, scientific principles, or treatment philosophy. It originates—as resistance often does—in denial. It reflects an unwillingness on the part of some psychoanalysts to confront troubling truths, and in the end is nothing more than an attempt to reify the status quo.

Nomothetic research is frightening to some psychoanalysts because it compels theoretical refinement and forces us to change our methods as new findings accumulate. In contrast, idiographic data *suggest* change

⁵One other error in Karon's (2002) response warrants brief mention. He claims that I said "psychoanalysis has no impact on the psychology of child development" (p. 569). In fact, I did not say that—Berk (1991) did. I quoted Berk to illustrate my point that "in most introductory, personality, developmental, and abnormal psychology texts, psychoanalysis is described in negative terms" (Bornstein, 2001, p. 6). Psychoanalysis has indeed influenced developmental theory in recent years, but much of this influence has gone unacknowledged (Masling & Bornstein, 1996).

rather than compel it, and allow us to cling to old, familiar ways longer than is necessary or useful. But in this case, comfort has cost: Without a sound nomothetic research base, psychoanalytic theory will not survive in academia, nor will psychoanalytic therapy endure in an increasingly competitive clinical marketplace.

The critiques by Waiss, Mills, and Lothane illustrate several self-destructive behaviors exhibited by psychoanalysts who are unwilling to incorporate into their work empirical findings from within and outside the discipline. Each of these critics offered their rebuttal with the aim of defending a theory that they (and I) care about deeply. In fact, such responses can only hasten the theory's demise.

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