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PSYCHOANALYSIS AND ITS CRITICS

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This article discusses the question of the basis of changes in psychoanalytic concepts, theory, and treatment. Illustrative examples discussed include the "widening scope" of the use of "parameters" in psychoanalytic treatment; the rejection of the "Enlightenment Vision" and the concomitant de-emphasis on the role of insight; the concept of "narrative truth"; and the "totalistic" re-conceptualization of the meaning of countertransferase. I then discuss the relationship between research and clinical practice and argue that if it is to grow, psychoanalysis must be open to and attempt to integrate findings from other related disciplines.

I begin with a distinction between criticisms and critics of psychoanalysis from without and from within. Serious criticism should always be thoughtfully considered, wherever the critic is situated. However, there is an understandable tendency for those within psychoanalysis to rush to its defense when it is criticized from without, particularly when the outside critic launches what is perceived as an unfair attack. It is somewhat like finding it acceptable to criticize one's wife or husband but rushing to his or her defense in the face of outside criticism. Thus, although I have written many articles critical of certain aspects of psychoanalysis, I wrote to the New York Review of Books defending its contributions in response to Frederick Crews' wholesale condemnation of psychoanalysis. Our understandable reactions to the unfairness and indiscriminateness of some criticisms from without should not, however, blind us to the need for sustained self-criticisms from within, from those who have a broad sympathy with an overall psychoanalytic perspective but take issue with specific claims or practices. The internal critic is often in a better position to offer challenges and criticisms of specific elements within psychoanalytic theory and practice, because his or her thinking is informed by an intimate knowledge of and commitment to the field. I hope that what follows will be perceived as such.

A frequent—I think the most frequent—response from the psychoanalytic community to cogent criticisms of traditional psychoanalytic theory (e.g., Grünbaum, 1984, 1993) is some variation of "oh, that might have been true years ago, but psychoanalysis has progressed and we don't think that way anymore. The critics are beating a dead or at least

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an outdated horse." This reply tends to be taken as self-evidently correct, but in fact there are at least three questions one can pose in regard to this response: (a) Just how do we now think? (b) On what grounds, including empirical grounds, are the changes in how we think based? and (c) Does how we think now effectively address the criticisms previously made?

I want to address mainly the second question in this article, which is essentially a question regarding how changes in theory and in therapeutic approach come about in psychoanalysis. Some years ago I published a couple of articles on theory change in psychoanalysis, and my presentation today is a continuation of those articles (Eagle 1986, 1993). My main claim is that changes in psychoanalytic theory and practice are primarily based not on empirical evidence, including evidence of greater therapeutic effectiveness—although I have the personal conviction that certain changes, for example, the abandonment of the "blank screen" role, could not help but contribute to greater therapeutic effectiveness— but instead reflect broad developments and shifts in cultural, philosophical, and social–economic conditions.

Consider as a case in point the claim that changes in contemporary psychoanalytic theory and practice have yielded a "widening scope" (Stone, 1954, p. 567) of practice that permits effective psychoanalytic treatment for a wider range of patients. The term *widening scope* implies that new techniques and tools are now available that can reach patients who were not treatable by earlier techniques and tools. It is not at all clear whether these presumably new techniques and tools are more effective with more disturbed patients, and whether the "older" techniques and tools were ineffective with certain kinds of patients. One only knows for sure that there are repeated statements in the literature referring to a widened scope and its presumed applicability to more disturbed patients.

In addition to the motivation to help a wider range of patients, an additional factor in leading to the so-called widened scope might have been the need to enlarge the range of patients who were available for psychoanalytic treatment. The pool of patients who met the idealized criteria of appropriateness for psychoanalysis was shrinking. There were fewer and fewer patients who were able or willing to spend a great deal of money and time and who were likely to be capable of withstanding the rigor of the classic analytic stance. The widened scope suddenly made potentially available a large pool of patients who would not otherwise have been available.

For many of the wider range of patients, the classical analytic situation, including the blank screen role of the analyst, was probably not helpful or manageable. This meant that the analytic situation had to be modified so that it would be more suitable and more likely to be helpful for these patients. However, it is important to note that the modifications of the analytic situation—what Eissler (1953, p. 109) earlier referred to as "parameters"—to make it more manageable for a wider range of patients, including more disturbed patients, does not necessarily mean that the blank screen stance, although more readily tolerated by some, perhaps less disturbed patients, *was ever appropriate or helpful for any patient*. It was not as if there was empirical evidence for the applicability of traditional methods within a certain patient population and their inapplicability for certain others, and then new evidence emerged that a different approach could help those others and thus widen psychoanalysis's scope. No such evidence was presented. In fact, at least some of the modifications of the analytic situation which constituted the so-called widened scope and which presumably were generated by the treatment requirements of more disturbed patients may well have been modifications appropriate for all patients, as Stone (1954) suggests.

This, then, seems not so much a matter of widened scope for a particular class of

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patients but of general correctives of a stance that, too often, had become rigidified and stultified. As Holzman (1976) noted, for some classical analysts who took ideas such as blank screen and analytic neutrality too literally, the analytic stance had become somewhat of a caricature characterized by aloofness, excessive silence, and stodginess. Although it is, of course, an empirical question, it is difficult to believe that these characteristics would be therapeutic for any patient.[1]

Eissler's (1953, p. 109) concept of "parameters" implies that although the ideally desirable analytic stance is a neutral[2] and blank screen one, given, so to speak, extenuating circumstances (e.g., more disturbed patients), one may have to introduce modifications, that is, parameters, to the ideal analytic stance. However, Eissler cautioned the parameters should be reduced as soon and as much as possible, should be interpreted, and one should not permit an unnecessary transference gratification. In other words, one should attempt to return to the ideal analytic stance as soon as possible. But there was little reason to believe that the presumed ideal analytic stance was therapeutically ideal. It was ideal only in the sense that it was presumably dictated by theory and had become accepted dogma.

Later modifications of the analytic stance and of analytic practice included other features that one might sum up as the increasing democratization of the analytic situation. Psychoanalysis became increasingly interactional; transference was no longer simply distortion; the analyst was no longer confidently thought to be in an epistemologically privileged and expert position, and so on. Also, the "participant-observer" sensibility of the quintessentially American Harry Stack Sullivan was suddenly rediscovered, often with no explicit acknowledgment. In some quarters, the process of democratization increasingly blurred distinctions between therapist and patient. I read recently in one of our journals—I can't recall where—that the success of a treatment should be measured by the analyst's transformation, as well as the patient's.

From his or her earlier position as an opaque blank screen, the analyst had now become for many, if not an equal, at least a near-equal democratic partner in self-disclosure. The patient self-disclosed, both overtly and inadvertently, through free association on the couch and the analyst self-disclosed, presumably mainly advertently, from behind the couch, through sharing his or her countertransference reactions, sometimes including very personal reactions. The question of the analyst's self-disclosure is a controversial one. But my purpose here is not to discuss that but rather to note the radical swing of the pendulum and to raise the question of how we got from one place to the other.

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[1] One finds in the history of psychoanalysis a particular pattern of justifying departures from traditional theory and practice by initially limiting claims of their applicability only to a particular class of patients and retaining traditional theory for the other "usual" patients. As Mitchell (1979) argues, the division of domains of applicability represents, in part at least, an attempt to escape the charge of heresy through a strategy of retaining a "domain for orthodox concepts" (p. 182) and designating "a new form of psychopathology to which the formerly heretical lines now apply" (p. 188). Furthermore, as I (Eagle, 1987) have noted, once these innovations have been accepted on this limited basis by the psychoanalytic community, then claims of applicability are extended to all patients. A good example of this pattern is the movement from Kohut's (1971) early restriction of the applicability of self-psychology to narcissistic personality disorders (with traditional drive-theory continuing to be applicable to "structural neuroses") to the more all-encompassing claim that at the "deepest level" of all disturbances is self pathology (Kohut, 1984).

[2] Although a full discussion of this issue is beyond the scope of this article, I believe that, understood in a particular way, a good case can be made for the therapeutic legitimacy and value of maintaining a stance of analytic neutrality. However, I do not believe that a plausible case can be made to expect that neutrality instantiated as aloofness, excessive silence, and other related attitudes and behaviors should have any special therapeutic value.

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On what factors was this swing based? As far as I know, we do not have the foggiest idea whether and when self-disclosure is related to therapeutic outcome. We also have little or no systematic information on different kinds of self-disclosure. We have, instead, testimonials and seemingly endless debate.

Let me turn now to the relation between other changes in psychoanalysis and broad philosophical developments in our culture. How can one characterize these broader developments and in what ways are they reflected in changes in psychoanalytic theory and practice? It seems to me that a central and significant cultural and philosophical shift that marks our era is the repudiation of, or perhaps disillusionment with, what Searle (1998, p. 12) calls the "Enlightenment Vision." Among other things, this repudiation is characterized by a rejection of and deep skepticism toward the legitimacy of any concept of a reality independent of the observer, any notion of universal truths, and, as expressed by Rorty (1979, 1985, 1991), of even the usefulness of the notion of truth. Furthermore, for those who do allow room for a concept of truth it is only a local, not a universal, one. It is a socially constructed truth that is saturated with issues of power, social practices, and social status. Again, as expressed by Rorty (1979), in this view, knowledge is not a matter of internal representations that reflect or mirror an independent reality, but rather a matter of pragmatic usefulness in achieving the practical projects and goals one is pursuing. One may recognize my brief description as social constructivism or postmodernism, but the label does not matter.

It seems to me that paralleling the broader philosophical shift briefly described above, indeed, reflecting it, is the shift from psychoanalysis as a quintessential representation of the Enlightenment Vision to a project in which that vision is repudiated and replaced. As I have argued elsewhere (Eagle, 2003), for Freud, learning the truth about oneself—in the form of lifting repression, for example—was at one and the same time, a Socratic moral imperative to know oneself, and the primary means of therapeutic cure. What a fortunate and wonderful and perhaps too good to be entirely true convergence! Gaining self-knowledge, expanding self-awareness, and being cured were all part of the same liberating project, a project that seemed to be a quintessential expression of the Enlightenment Vision. Soon, however, doubts were voiced regarding the curative power of interpretation and ensuing insight and awareness. An attempt was made to rescue insight by positing a distinction between merely intellectual and truly transformative emotional insight. However, this discussion and literature soon faded and what followed was an increasing de-emphasis of the primary role of insight and awareness and an increasing emphasis on the curative powers of the therapeutic relationship.

The increasing emphasis on the therapeutic relationship and the increasingly interactional conception of the psychoanalytic situation brought other changes in its wake. Among many analysts, the patient's transference reactions were no longer understood as distortions or projections on a blank screen analyst, but rather as plausible interpretations of cues emitted by the analyst (Gill, 1982, 1994). Also, although analysis of the transference had long been a central focus in psychoanalytic treatment, for many the belief that only transference interpretations are therapeutically useful has become virtually axiomatic in contemporary psychoanalysis. And yet, there is remarkably little evidence supporting this claim. It has become an article of faith based on repeated assertions (see Spence, 1992), despite the fact that the picture is a very complicated one and that there are at least some studies that report a negative relationship between frequency of transference interpretation and therapeutic outcome (e.g., Ogrodniczuk, Piper, Joyce, & McCallum, 1999).

Consider also the "totalistic" (Kernberg, 1965) reconceptualization of countertrans-

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ference and its new place of honor. Gabbard (1995, p. 475) recently observed that the use of countertransference as a valuable guide to understanding what is going on in the patient's mind constitutes the "common ground" of contemporary psychoanalysis, despite the existence of theoretical differences. However, it should be noted that there are no systematic investigations and no systematic evidence indicating when one's countertransference reactions serve as a reliable guide to what is going on in the patient's mind and when they do not.

Although the de-emphasis of insight and awareness does seem to reflect a disillusionment with the Enlightenment Vision, the concomitant emphasis on the therapeutic relationship is not, in itself, antithetical to that vision—it is sort of orthogonal to it.[3] It reflects a turning to other factors partly as a consequence of the disillusionment with the curative role of insight and awareness. Nevertheless, if one found, on the basis of systematic and ecologically valid empirical research, that relationship factors contribute more to positive therapeutic outcome than interpretation and insight, there would be no special anti-Enlightenment or antiscientific implications. Emphasis on the therapeutic relationship is not in itself antithetical to the Enlightenment Vision. It would simply be a straightforward empirical finding that would present a challenge to a point of view that places exclusive emphasis on the therapeutic role of insight and awareness. And, indeed, there is some evidence that the quality of the therapeutic alliance is the single factor most highly correlated with positive therapeutic outcome (e.g., Blatt & Zuroff, 2005; Zuroff & Blatt, 2006).

Some contemporary features and developments in psychoanalysis, however, go beyond merely giving special attention to the therapeutic importance of the patient–analyst relationship. And it is these developments that I believe most clearly reflect broad philosophical influences and that are most susceptible to criticisms from within. Perhaps the most far-reaching conceptual changes in psychoanalysis, the ones that perhaps most clearly reflect contemporary philosophical shifts, are those that seem to call into question the very ideas that (a) the patient's mind has an organization and structure that is independent of the analyst and the analytic interaction—an ontological claim; and (b) that one can gain any objective knowledge of such a mind—an epistemological claim. Note the issue here is no longer the question of whether uncovering and discovering truths about the patient's mind are *therapeutically effective*. It is, rather, the deeper and more philosophically sweeping question of whether it is in principle even possible. These ontological and epistemological skepticisms regarding, respectively, the separateness and the knowability of another's mind have been expressed in a number of ways in the psychoanalytic literature.

Consider Spence's (1982) concept of "narrative truth" (which if you read Spence you will find, has nothing to do with truth, narrative or otherwise, but entirely with persuasiveness). In contrasting "narrative truth" and "historical truth," Spence makes a modest and defensible point; namely, that because the analyst (and patient) does not have reliable access to historical events in the patient's life, the best one can do is formulate narratives that are persuasive to the patient. Freud (1937) makes a similar point in his "Constructions in Analysis" paper when he writes that "if the analysis is carried out correctly, we produce [in the patient] an assured conviction of the truth of the construction which achieves the same therapeutic result as a recaptured memory" pp. 255–256). He acknowledges that the

[3] Such an emphasis is, however, more congruent with what Rorty (1985) calls "solidarity" in opposition to "objectivity".

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analyst *constructs* rather than *reconstructs* the patient's history.[4] It is unfortunate and misleading, I believe, to have chosen the term narrative *truth*, insofar as it equates at least one kind of presumed truth with persuasiveness. That is, it implies that there is a special kind of truth—narrative truth—that is to be distinguished from ordinary historical truth by its special mark of persuasiveness. However, the basic idea that, *as far as the historical past is concerned*, the analyst may have little choice but to formulate persuasive narrations is, I repeat, a modest and defensible one. What has happened, however, is that the concept of narrative truth has come to be taken by many to be applicable to all analytic interpretations, including those that refer not only to the patient's *historical* past, but to his or her *current* unconscious mental states. In other words, narrative truth has been assimilated into the general position that the analyst does not and cannot gain objective knowledge of the patient's mind, but can only construct (or coconstruct) new narratives. On this view, all the analyst can offer is persuasive narratives that hopefully make sense to the patient and are useful.

In effect, this view concedes defeat in the battle against suggestion, which Freud viewed as the single most devastating criticism of psychoanalysis. For some, who were openly inclined toward postmodernism, even this was not enough. Thus Geha (1984b) takes Spence to task for being a closet positivist, still, poor man, naïvely believing that there is a historical truth that can be distinguished from constructed narratives and for being concerned with retaining truth of any kind. According to Geha (1984a, p. 268), analysts generate "beautifully wrought" esthetic fictions, nothing more, nothing less. (For a further discussion of this issues, including ontological claims regarding other minds, see Eagle, 1984; Eagle, Wolitzky, and Wakefield, 2001; Altman & Davies, 2003; and Eagle, Wakefield, and Wolitzky, 2003).

Note that these developments in psychoanalysis regarding knowledge of the internal world almost completely parallel a currently fashionable philosophical position regarding knowledge of the external world. For example, as noted earlier, Rorty (1979, 1991) tells us that we should dispense altogether with the concept of truth and the idea that our theories and representations mirror an independent reality and are to be tested against that independent reality. According to Rorty, animistic tribal myths about the nature of the universe have no lesser or weaker epistemological status or claim to truth than, say, Einstein's theory of relativity. One cannot look to an independent reality to adjudicate between different accounts. Each story—and each, according to this view, is nothing but a story—is designed to serve a particular pragmatic project and accomplish certain goals and is to be evaluated only in terms of how well it does that. By the way, most often we do not even take this step in psychoanalysis. That is, we generally do not systematically determine whether this or that coconstructed story or retelling or narrative or esthetic fiction or new perspective accomplishes what it is supposed to accomplish. We merely proclaim and assert and tell anecdotes.

So, where does all this lead to and where do we go from here? I can only give you my own views for whatever they are worth. I believe that psychoanalysis cannot be a self-contained discipline but instead must be open to influence from and integration with

[4] It will be noted that although Freud remarks that the best the analyst may be able to do is construct a convincing account of the patient's past, he does not claim that the constructed account somehow possesses some special truth to be distinguished from ordinary truth. Instead, he focuses on the therapeutic usefulness of the constructed account.

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findings and theory from other disciplines. Drew Westen's work, one example of which is presented on this Panel, serves as a very good exemplar of that kind of endeavor.[5]

I think, however, there is a caution to be sounded. There is the risk that one will look to, say, cognitive science and neuroscience only or mainly to *confirm* preexisting psychoanalytic concepts and formulations. That might be interesting and personally satisfying, but it will not necessarily contribute to the growth and vitality of the field. The greater challenge is to look to findings from other disciplines that may suggest a reconceptualization or even elimination of cherished ideas. For example, there is some fascinating empirical work in psychology on "repressive style," the implications of which both support certain aspects of Freud's concept of repression but, at the same time, point to important modifications of that concept (see Eagle, 1998).

I believe that attachment theory and research have a great deal to offer to psychoanalytic theory and practice. For example, as Parish and Eagle (2003) have shown, particularly in long-term psychoanalytic treatment, the therapist serves as an attachment figure for the patient—although in different ways, depending upon the latter's attachment pattern. I think some interesting implications flow from thinking of the therapist as an attachment figure, as well as some interesting work to be done on how the match between therapist's and patient's attachment patterns influence the course of therapy (see Diamond et al., 2002, for preliminary work in this area).

There are certain concepts that cut across different theoretical schools and that are likely to play a central role in understanding how people function. Two such related concepts in my view are defense and affect regulation. As Drew Westen's article (2004) demonstrates, defense has been an extraordinarily robust concept in psychoanalysis—although how it is understood needs to be fine tuned in the light of clinical work and empirical findings in psychology and neuroscience. The concept of self-regulation is integrally linked to defense insofar as from the very beginning, the primary function of defense has been understood to be the regulation of affect states such as anxiety, guilt, depression, and shame. By the way, work on attachment styles shows that one can find evidence of self-regulating defense in infants as young as 1 year of age. Thus, there is evidence that a 1-year-old avoidant infant who does not seem to respond to mother's absence and/or who does not turn to mother as a safe haven or safe base upon reunion in the Strange Situation nevertheless is responding with accelerated heart rate and increased level of cortisol secretion (e.g., Sroufe & Waters, 1977). One way of interpreting these findings is that the avoidant pattern serves to spare the infant the pain of rejection and the caregiver's anger. In other words, it serves as a defense.

Although insight and self-awareness are not in much favor these days, recent attachment research points to their great importance, including the role of mentalization and self-reflection in self-regulation. (e.g., Fonagy, Target, Gergely, & Jurist, 2002). We may yet find that such old-fashioned ideas as the goal of strengthening the observing function of the ego in psychoanalytic treatment may become acceptable and prominent again.

My strong belief is that one of the places we need go from here is the abandonment of different "schools" of psychoanalysis, each with its own training institutes, its own associations, and its own loyal band of followers. This sort of thing is more appropriate to political parties or religious sects than to a professional or scientific discipline. Adherence and loyalty to different schools are associated with a habit of mind that is parochial and will, in my view, contribute little to psychoanalysis. I know that a happy

[5] The research reported by Westen on this Panel will appear elsewhere.

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face is put on so-called psychoanalytic pluralism. It is hailed as a healthy alternative to past psychoanalytic orthodoxy and rigidity and the psychoanalytic version of "let a thousand flowers bloom." However, it seems to me to reflect more a Tower of Babel than anything else. I have much sympathy with Fonagy's (2002) conclusion that "this fragmentation and confusing absence of shared assumptions is what spells, to me, the inevitable demise of psychoanalysis—more than any of the external challenges we face" (p. 12). I am not suggesting (nor, I doubt, is Fonagy suggesting) a return to a period of monolithic orthodoxy and dogma. However, I do not believe, paradoxical as it may seem, that the existence of different schools constitutes a departure from orthodoxy and dogma. Rather, one finds that we now have a varieties of orthodoxies and dogmas rather than a simple predominant one.[6] Freedom from orthodoxy and dogma, it seems to me, lies not in multiplicity of "local" orthodoxies and dogmas, but in an openness and habit of mind that is sensitive to evidence and that is antithetical to loyalty to this or that school. In short, the current so-called pluralism is not the only alternative to past psychoanalytic orthodoxy and dogma. Another alternative is an enduring effort for a truly integrative theory that is the product of a relinquishment of quasi-political loyalties and a genuine openness—and openness includes a readiness to relinquish cherished ideas—to relevant empirical findings from a variety of sources.

One will be able to say that the psychoanalytic ethos has changed when one can write about the history of psychoanalytic ideas without reference to "dissidents" or "revisionists." As a former President of Division 39, I have been especially concerned with the direction the division has taken. I believed that as an organization primarily of psychologists, Division 39 could perhaps better integrate psychoanalysis with psychology, broaden its empirical base, and take it in a direction that the earlier psychoanalytic establishment could not. However, although there are exceptions—Drew Westen is a prime example of such an exception[7]—for the most part, this has not occurred. Indeed, in certain respects, I think the programs of the American Psychoanalytic Association meetings perhaps reflect a greater concern with scientific issues and input from other disciplines than the programs of Division 39 meetings. I am not alone in this assessment.

During the course of revising and expanding this article for publication, I had the occasion to read an exchange of e-mails on psychodynamic research on a Listserv that includes leading clinicians, researchers, and scholars in our field. It is clear from these e-mail exchanges that at least some leading people in our field are dissatisfied and disillusioned with the substance and content of Division 39 meetings, in particular with the relative absence of representation and interest in empirical research at these meetings. Drew Westen (April 21, 2005, personal communication) writes that "I mostly go to Division 39 meetings to see friends. Most of the talks have titles like the container, the contained, and the continental breakfast." I would add an observation on the hype and excessive claims one sometimes encounters implied in the titles of Division 39 workshops. Westen also notes the tendency of some authors to present "views about development, unencumbered by what anyone who has actually studied it has ever written" and suggests

[6] There are exceptions to this state of affairs. For example, Pine (1990), although not claiming or attempting theoretical integration, argues convincingly that the different aspects of psychic functioning, emphasized by different "schools"—drive, ego, object, and self— are all likely to be especially relevant at different points in psychoanalytic treatment.

[7] Others include John Auerbach, Sidney Blatt, Wilma, Bucci, Hartvig Dahl, Diana Diamond, Peter Fonagy, Kenneth Levy, Lester Luborsky, Joseph Masling, Harold Sampson, Robert Wallerstein, Sherwood Waldron, and Joseph Weiss. Of course, this is not meant to be an exhaustive list.

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that someone who presents or submits a paper on an issue that has been empirically investigated should at least demonstrate that he or she has carried out a PsycINFO search.

In accord with my own impression noted above, Westen also notes that "I'm actually finding that the medical analysts and the American Psychoanalytic Association and its institutes are some of the most excited about research. Many in Division 39 are psychologists and are dropping the ball rather than pushing it up the hill." Again, similar to my view, Westen writes that "unfortunately, when psychologists created their own alternative to the orthodoxy of medical psychoanalysis, they forgot that alongside learning to interpret meaning in graduate school (now a disappearing art), they also learned something else that could have distinguished a psychologically informed psychoanalysis: knowledge about evidence, methodology, and hypothesis testing."

An interesting and potentially fruitful suggestion made by Larry Josephs (April 21, 2005, personal communication) in that e-mail exchange is the scheduling of a regular Division 39 presentation in which a theory paper, a clinical case paper, and an empirical research paper are presented on a given topic or issue. This approach could serve as a model for cooperation and interpretation among clinical, theoretical, and research perspectives.

I believe that we need to strive to develop an integrated theory of mental functioning that incorporates findings from other disciplines and that does not neglect or trivialize the kinds of real-life complex phenomena and properties of the human mind with which, in contrast to other approaches, psychoanalysis has been traditionally concerned. I know of no current theory other than psychoanalysis that even attempts to do justice to the depth and complexity of the human mind. This is what needs to be preserved. Whether the integrated theory turns out to bear much resemblance to current psychoanalytic theories and schools seems to me of less importance than whether it constitutes an ecologically valid and comprehensive explanatory account that does justice to deep questions regarding the passions, subtleties, and complexities of the human mind.

It will be noted that I emphasize the need for an integrated theory of mental functioning and an explanatory account of the human mind rather than the need to demonstrate the effectiveness of psychoanalytic treatment—although one would hope that the former would have implications for the latter. Although I believe that it is important to carry out outcome and process research on psychoanalytic treatment, I do not believe that psychoanalysis' main claim on posterity will necessarily lie in its therapeutic effectiveness.[8] Indeed, Freud (1933) remarked that although "psychoanalysis began as a method of treatment, ...I did not want to commend it to your interest as a method of treatment but on account of the truths it contains. . ." (p. 155). He also famously expressed his need "to feel assured that the therapy will not destroy the science" (Freud, 1926, p. 254). From the very beginning, the main contribution of psychoanalysis was that it presumably constituted both a special means for the achievement of self-awareness and self-knowledge, as well as a treatment for neurosis. As I noted above, from the moment that Freud focused on lifting repression as the main process goal of psychoanalytic treatment, the Socratic imperative to know thyself and the clinical goal of cure for mental disorder converged. However, the Zeitgeist no longer permits the assumption of a

[8]Although there are studies suggesting that psychodynamic treatment is effective (e.g., Bateman & Fonagy, 2001; Fonagy, 2002; Leichsenring, 2005; Leichsenring, Fairbairn, & Leibing, 2003; Sandell et al., 2000, 2001).

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convergence between self-awareness and cure, at least not in any simple and unproblematic way.

If one can no longer assume a convergence between self-knowledge and cure, where does this leave psychoanalysis? We have witnessed many implicit and explicit answers to this question, including ones that focus on the therapeutic relationship, on corrective emotional experiences, on offering new perspectives, on constructing narratives, and so on. An adequate answer to this question could itself take up a full volume. One can read Freud's modesty regarding the therapy as expressing the concern that in an overemphasis on the cure aspect of psychoanalysis, its major role as a special vehicle for self-knowledge (as well as knowledge of others) would be weakened. Indeed, I suspect that if Freud was faced with the possible disjunction between self-knowledge and therapeutic effectiveness (the latter defined in a way that did not give a privileged position to self-knowledge) and was forced to choose between the two, he would opt for self-knowledge as the main mission of psychoanalysis.

One could argue that the emphasis on self-awareness and self-knowledge is economically suicidal insofar as such an emphasis would lead to the disqualification of psychoanalysis as a treatment modality by health maintenance organizations (HMOs) and insurance companies. However, they do so anyway, and it is just as likely that the claim that we offer coconstructed narratives, or retellings, or new perspectives would equally disqualify psychoanalysis as a treatment modality. It may well be the case that psychoanalysis cannot compete as a treatment for mental disorder in an age of quick fixes and randomly controlled trials but can honestly offer a unique opportunity to strengthen one's capacity for meaningful self-reflection and a unique experience in the quest for self-awareness, self-knowledge, and self-discovery. In a 1997 article, Stone, himself an analyst and former President of the American Psychiatric Association, takes the position that psychoanalytic treatment is not suitable for severe pathology but is irreplaceable as a means to self-knowledge. He challengingly writes: "I still believe that that a traditional psychoanalytic experience on the couch is the best way to explore the mysterious otherness of oneself. But I do not believe that psychoanalysis is an adequate form of treatment" (p. 39). He also writes that, contrary to Freud, "when a patient's *symptoms* are treated, he may then need a psychoanalyst to help him deal with his ordinary human suffering. That is the therapeutic domain in which the art of psychoanalysis will survive" (p. 39). It might well turn out, however, that the pursuit of self-knowledge and self-reflection is not only a worthy aim in itself, but may, in the long run, constitute the most reliable means of achieving cure or lessen the need to seek cure from the more narrowly focused forms of treatment.

In coming to the end of this article, I want to address briefly two underlying issue that run as a red thread through the entire discussion, namely the "proper" relationship between clinical work and research and the weight to be given to clinical experience and to the cumulative clinical experience of our field in developing theories of personality and in clinical practice. Does an emphasis on systematic research imply that these experiences are to count for nothing? An adequate examination of this complex issue requires a separate article or perhaps a separate volume. However, a few brief comments are in order.

I recall during my Presidency of Division 39 there would be periodic flurries of apparent interest in research generally generated by the urgent need to demonstrate that psychoanalytic treatment is effective and could, therefore, qualify for HMO and insurance coverage. As noted earlier, there is, in fact, a body of research that tends to demonstrate that psychodynamic treatment is effective. However, the sudden and periodic surges of interest in research were not motivated by any interest in information that research might

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provide, but by its public relations value. This, I believe, continues to be the typical attitude among many analysts. In a recent e-mail discussion of evidence-based therapy, a number of prominent analysts took the position that although we know that psychoanalysis works, we need the research for public relations purposes. In a recent article, articulating a viewpoint similar to the one expressed here, Schachter (2005) cites as an example of this attitude, Laufer's (2004) statement that universities "can contribute to systematic research methodology to *validate* our findings" (p. 16, Schachter's emphasis added). Schachter goes on to comment: "*she knows* that our finding are correct; university research would provide a pro forma validation, rather than a test of them" (p. 484, italics in original). This attitude toward research limits its usefulness and purpose to what might be called its *demonstration* value. That is, it is intended to demonstrate what we already know (or think we know). There is little or no interest in learning anything new from the research and little or no expectation that the research findings will surprise anyone by calling into question or disconfirming what we think we already know. Although understandable and necessary in particular contexts, the demonstration function itself is not likely to lead to new knowledge and to growth of our field.

My strong impression is that if a research finding did contradict or disconfirm the clinician's convictions, it would be ignored or rejected. In an e-mail discussion of a Listserv,[9] a prominent analyst—let us call him Dr. X—took the following position: "what if the findings [of a research study] were that medication worked just as well as medication and psychotherapy, but my conviction, based on my own experience, was that people who tried with just meds came back later in worse shape and people who continued with both therapy and meds did great. Well, I'm the therapist. Shouldn't my own experience take precedence over any study, regardless of the results, in which I was not the therapist? What kind of scientist would I be if I let a study in which that variable was left uncontrolled decide how I should practice?" One can charitably interpret these comments as making the valid point that in this hypothetical study, because the experience, talent, and qualifications of the therapist were not taken into account in comparing the relative effectiveness of medication and psychotherapy, the results should be taken with a grain of salt. Such criticism, taken seriously, would lead to a better-designed study. However, this is not the gist of Dr. X's comments. For even a better-designed study would still not include Dr. X who could continue to ask: "Shouldn't my own experience take precedence over any study, regardless of the results in which I was not the therapist?" Imagine that every therapist takes a similar position: "I don't care what the results of this or that study show. I was not the therapist in the study. And my clinical experience is what counts in how I practice." This position is, of course, a recipe for ignoring the findings of any study, however well designed and however ecologically valid.

What Dr. X does not address is the question of what variables are being omitted or ignored by virtue of his not being included in this hypothetical study. What is it about Dr. X's experience that could usefully be included as a factor in an investigation of psychotherapy process and outcome? Is that factor Dr. X the person or something he understands and does which, if included in a study, and carried out by others, would yield processes and outcomes congruent with Dr. X's experience? When Dr. X asks "What kind of scientist would I be if I let a study in which that variable was left uncontrolled decide how

[9] I understand that Listservs have an ambiguous privacy status. Hence, although I quote one of the contributors to the e-mail discussion, I do not identify him or her. Therefore, I will use the name "Dr. X".

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I should practice?" the variable he seems to be referring to is Dr. X the person or Dr. X the therapist. But surely, Dr. X knows, that he cannot be included in every study on psychotherapy process and outcome—just as every therapist who takes a similar position knows. What Dr. X position amounts to, then, is not only a guaranteed rejection of any findings that contradict his experience, regardless of how well the study is designed, but also silence regarding how his experience might contribute to better studies which, despite his not being included as a therapist, he (and others) would find convincing.

What then is a useful and productive relation between clinical experience and research (as well as other) findings? I think it is important to note that in clinical work (from both the patient's and the therapist's perspective), as well as in everyday experience, compelling insights may spontaneously emerge that cannot be controlled or predicted nor dictated by the use of a therapy manual, but nevertheless have the ring of truth about them. Meehl (1991), who has long advocated the importance of rigorous research data, provides a striking example of this kind of experience. Most of the time, these insights are not and perhaps cannot be validated by systematic research data. I am not suggesting that, therefore, one should dismiss or minimize the importance of these experiences.

It seems to me in doing clinical work one must anchor oneself in a set of convictions that are buttressed by background information, commonsense, personal intuitions, and cumulative clinical experience. However, although one may have such convictions—and I have expressed such convictions throughout this article—it is necessary. I believe, to recognize that contrary evidence may come along that will throw these convictions into question. All such convictions, then, particularly in the contexts of theory and professional practice, have a provisional status, subject to the test of systematic evidence.

I realize that this is a difficult attitude to maintain. One is always more comfortable with certainty and unshakeable convictions. However, there is a great cost to be paid for this greater comfort. One of the reasons I believe that loyalty to this or that psychoanalytic school is ultimately harmful to the field is that it serves to crystallize and ossify convictions on the basis of quasi-ideological affiliation and renders one's views more refractory to contrary evidence.

In the course of working on this article, I read a New York Times (Kolata, 2006) article on an extensive and well-controlled 6-year study on the effects of a low-fat diet on the incidence of cancer and heart disease in women. The results showed that the low-fat diet employed in the study was *not* a protective factor—evidence apparently dramatically contrary to the cumulative clinical wisdom and experience of the field. I have little doubt that the clinical wisdom of many, if not all, physicians dictated the prescription of a low-fat diet and that they could buttress their conviction with anecdotal evidence and self-selected case studies. However, their convictions and cumulative clinical experiences seem to be unsupported by systematic evidence. As one commentator noted, "We, in the scientific community, often, give strong advice based on flimsy evidence. That's why we have to do experiments" (as quoted in the *New York Times*, p. A15).

This study is hardly the last word on the topic. Letters to the *New York Times* have already appeared pointing to flaws in the study and undoubtedly more systematic critiques will follow. But cogent critiques will likely lead to better studies and modifications of the conclusions suggested by the earlier study. And so it will go. It is likely that despite the findings of this study, which like many studies is an imperfect one, many physicians will continue to prescribe a low-fat diet. (And, I, personally, will continue a low-fat diet.) However, the issue is now open to further systematic investigation. Any responsible physician, whatever his or her clinical convictions, will now have to be sensitive to relevant findings from future studies. I am reminded of Meehl's (1997) observation that

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there is no clinical intervention in human history, including those that in the course of time, have turned out to be useless or harmful, that has not been supported by personal conviction, clinical experience, testimonies, and anecdotes.

As noted, I am aware that in doing clinical work, one must have certain convictions and firm beliefs in order to function effectively. One cannot constantly keep at the forefront of one's mind an attitude of skepticism and an acute awareness of the fallibility of one's convictions. At its extreme, this degenerates into an obsessive stupor. However, at the other extreme is the dead end of unshakeable and dogmatic conviction and the inability to not know and to be surprised by findings that are contrary to one's dogged beliefs and convictions. I believe that in order for our field to grow, we must all wear two hats, one hat operating with some confidence, on the basis of what one thinks one knows, and with the other hat operating with an openness to not knowing, to being surprised, and to contrary evidence. This is an ideal stance, I believe, not only in the context of the relationship *between* clinical work and research, but also *within* clinical work itself. Just as there is the danger that excessive certainty will shut out critical information from external research sources, so there is a similar risk that excessive adherence to preset views will shut out critical information from within the clinical situation, that is, from the patient.

A final comment: The focus in the above discussion has been on clinical work and the relationship between clinical practices and research findings. However, as noted earlier, psychoanalysis is not only a treatment approach, but also, and perhaps most importantly, a theory of mental functioning and psychopathology. Even if one questions the relevance of research findings for clinical practice, can the development of psychoanalytic theory afford to ignore relevant research from a variety of other disciplines? How one answers this question will reveal one's vision of psychoanalysis. Is it a self-sufficient discipline, content to rely on data exclusively from the clinical situation or does its future lie in an openness to and enrichment form a variety of sources, including disciplined clinical data and a wide range of research findings? For whatever it is worth, it should be clear what my view is. I believe that the former position is a dead end and that a receptivity to findings from a variety of disciplines represents the best opportunity for psychoanalysis to survive and, perhaps, prosper.[10]

[10] Needless to say, I also believe that other disciplines can benefit from psychoanalytic insights (e.g., See Barron, Eagle, & Wolitzky, 1992; Eagle, 1997).

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