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Reply to Commentaries

Michael J. Bader, D.M.H.

Owen Renik

THE REASON THAT I AM "ARGUING WITH THE ANGELS," AS OWEN PUTS it, is that the angels need the help. The tendency in psychoanalysis to argue from theory and received wisdom rather than empirical observation and actual patient-specific outcomes has always been our field's Achilles heel, generating a host of so-called principles of technique that have led us to assume unnatural roles and treat patients in bizarre ways (e.g., analytic abstinence). The more bizarre we appear to be, the more we claim a special knowledge that justifies setting us apart from other helping professions. We cannot demonstrate that we actually *help* anybody better than nonanalytic practitioners do, so we act as if that shouldn't matter because our theory is deeper, more subtle, and more complex than that of those other folks. The more we are ridiculed by a popular culture preoccupied with quick-fix cures, the more we comfort ourselves with the notion that "they" don't appreciate the power of the unconscious like we do.

It is in this context that I examined the postmodern critique of positivism and empiricism in psychoanalysis. What I found was old wine in new wine skins. There is, on one hand, a new appreciation of the ways that the analyst's participation, both theoretical and personal, affects the clinical field. On the other hand, the critique of positivism tends to mystify how analysts understand their patients. By turning the process of intersubjective empathy, inference, and understanding into an almost infinitely complex endeavor, enshrouded in ambiguity and uncertainty, postmodern epistemology distracts us from developing clinical methodologies that use observable patient-specific data as guideposts to improve our understanding and therapeutic efficacy. In this sense, the postmodernists perpetuate the tendency to neglect therapeutic aims in psychoanalysis (Bader, 1994).

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Owen doesn't think I have made my case. He argues that there is nothing intrinsic in postmodern epistemology that militates against an empirical approach. As I argue in my article, postmodern writers repeatedly make this claim. They anticipate criticisms like mine and preemptively reassure us that they are showing us how complicated reality is, not denying its existence. Owen, for instance, tells us that Hoffman is not against all empiricism, just a "Newtonian" one. He tells us that his own belief in the epistemological equality of analyst and patient is not just ethically preferable, it is scientifically superior. Therefore, we are entitled to ask postmodernists to demonstrate how they use their "Einsteinian," as opposed to Newtonian, science to sharpen their analytic work to get deeper, more satisfying, and lasting therapeutic results. How does it help them "on the ground"? If their constructivist sensibility is clinically superior to a nonconstructivist approach, what do they use as evidence? If Owen thinks that postmodernism is not intrinsically biased against a study of validation, he should demonstrate and not simply assert it.

I don't think he does. Hoffman may "call" for a more sophisticated empiricism, as Owen says, but where is the "answer"? Hoffman may have "intended" in his 1983 article to insist on scientific rigor, but Owen fails to cite examples in which this intention has been realized. In my reading of this literature, there is a relative absence of attempts to show precisely how a shift to a constructivist epistemology produces more salutary outcomes. Even if such a theory can, in principle, generate such methodologies, shouldn't it somehow be held accountable if it doesn't? What does it mean that a theory that cannot easily be translated into practical principles of clinical technique is gaining popularity? Why hasn't this epistemological critique generated concrete validation criteria? For a partial answer, I looked to the ways in which postmodern epistemology subtly mirrors the cynicism about social change and progress so prevalent in contemporary social and political life. Owen obviously disagrees, attributing this popularity to the clinical power of the postmodern perspective. Our differences might be clarified if we look at Owen's discussion of my clinical material.

Owen's critique of my clinical case demonstrates an interesting variation on the prevailing tendency to discuss technique from the point of view of theory rather than results. It is interesting because Owen is so

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consistently outspoken in his insistence that we privelege practicality and outcome over theory for its own sake. For instance, he recently lamented (Renik, 1997) the fact that a "great deal of sophisticated psychoanalytic inquiry is currently being undertaken that is relevant to philosophy, rhetoric, and aesthetics, but not, for the moment anyway, to work with patients" (p. 281). Yet although he is adamant that his approach is scientifically and not just ethically preferable, Owen's concerns about my clinical stance are not empirical. He disagrees, for instance, with my appearing to divine my patient's transferences and acting to correct the pathogenic aspects without first sharing my thinking with the patient. But he disagrees neither with my results—the patient showed clear analytic and therapeutic progress—nor with the fact that my methodology enabled me to test and validate my hypothesis using patient-specific feedback. Owen's problem is that I generated and tested hypotheses about my patient without disclosing them to her. The problem for him is that I am not fully intersubjective.

Owen and I are basically in accord about the possibility and desirability of getting at the patient's subjective truth to help him or her feel better, however differently we do so. And neither he nor I believe that truisms about observer bias and the subjectivity of perception necessarily lead to giving up the search for accurate prediction. But because Owen does not attempt here to demonstrate *how* his approach yields greater accuracy, his position in this discussion tends to come across as a plea for a different *attitude*, not better science. He is defending modesty and self-disclosure, not postmodern uncertainty. He argues with me about how much you tell a patient about your theories, not about whether the analyst can ever make truth claims with confidence. In Owen's model, the emphasis is on how the analyst and patient make such claims together, not whether it is epistemologically possible. This is an important discussion but it is not one that I took up in my article.

Having said that, I should say that I do not find Owen's preferences on this issue compelling. His emphasis on the foundational role of analytic candor, although a useful counterpoint to our tendency to subtly promote an illusion of omniscience, cannot possibly be a *defining* criteria of analytic mutuality for two reasons: First, it is not patient-specific enough, and second, it doesn't do justice to the extent to which therapists are necessarily strategic and calculating in their work. In terms of (not) being patient-specific, Owen's approach is not

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sensitive to those patients who would experience the therapist's self-disclosure as burdensome, narcissistic, or otherwise misattuned. In my experience, some patients need the therapist to simply *do* the right thing, or function for a time as a protective authority without that authority being deconstructed by the analyst's interpretations or other self-revelations. So although Owen's approach might be very useful to a patient who had struggled with painful feelings of exclusion in his or her family and who found Owen's candor and willingness to "think out loud" about his hypotheses a welcoming affirmation and display of respect, for another patient Owen's approach might be reminiscent of a narcissistic parent's inability to tune into and prioritize the child's experience. It is important to note that the analyst doesn't need a decoder ring or powers of divination to assess and recognize these radically different situations. The analyst uses direct inquiry and observation, trial and error, knowledge about the patient's historical past as well as the current focus of treatment, and countertransference to develop a working picture of the patient's strengths and weaknesses and, on the basis of this picture, to decide what and how much the patient can and should hear. And although it is usually preferable for the patient and analyst to share and compare understandings about the nature of the problem and its treatment—more awareness is generally better than less for purposes of mastery—this general consideration is neither necessary nor sufficient as a clinical principle that can guide our moment-to-moment decisions. Clearly, we strive for our technique to be as flexibly attuned to the idiosyncratic needs of the patient as possible. Because it all depends on the moment and the patient, I believe that Owen's position is insufficiently flexible and specific. It is a good attitude but not a reliable guide to technique "on the ground."

In addition, I think that Owen minimizes the strategic aspect of the analyst's activity. Although he is right in asserting that analysts have traditionally underestimated the degree to which our subjectivity is immanent in our technique, I think that he is wrong when he implies that we cannot or should not deliberately feature one aspect of our thinking and withhold another at a given moment. To do this is not equivalent to making the patient "blind" in some experiment; it is an attempt to meet the patient's needs. In my opinion, patients are never blind because they are always pursuing an agenda of their own, an

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agenda that reflects their push for mastery, development, and health. Patients are always seeking (often unconsciously) certain kinds of help from the analyst for particular problems, testing the analyst to find out important information about those aspects of the analyst's psychology and theory that bear on their fears and conflicts, and making use of particular attitudes and insights and not others. Patients, in other words, are actively working to move toward particular conscious and unconscious goals that either are obvious or can be discerned by the analyst. Whatever the analyst does, including making candid self-disclosures, will be perceived by the patient as helpful or not. The analyst should obviously be trying to maximize what is helpful and minimize what is not, and his or her theory of technique should further this process. In the context of a particular patient who wants to solve a particular problem and an analyst committed to helping him or her to do that, the relevant question is whether the analyst is tuning in and supporting the patient's agenda, not whether or not the analyst is candid about her or his internal process.

In addition, even if the analyst wanted to, he or she could not speak to all of the tributaries of theory and feeling that lie behind his or her interventions, if for no other reason than considerations of tact and timing. My point is that spelling out one's hypotheses always reflects a complex clinical *choice* that should always be completely patient-specific. This decision-making process necessarily includes low-level hypotheses and predictions about the effect of one's disclosure or other interventions on the patient. Thus, whether I am intervening by providing corrective emotional experiences, making transference interpretations, or laying out the full complexity of my thinking à la Owen, I base my decision, in part, on an assessment of the clinical moment and use the patient's responses as confirmatory data about whether we are on the "right track." The goal is to get on the right track *by whatever means possible* and to have some empirical way of staying there. In my opinion, once this strategic and calculated dimension of the analyst's activity is acknowledged, Owen's ethic of self-disclosure seems less like a general principle and more like a good attitude with some patients at certain moments. It is still a useful corrective to a tendency among analysts to idealize and mystify technique, but it must inevitably be subordinated to other, more important, clinical concerns.

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Under the guise of a critique of a naive positivism, postmodern epistemology tends to make things more mysterious than they are. Owen's discussion of my attempt to link this epistemology to the prevailing *Zeitgeist* of cynicism about social change and about the ethic of helping reflects this problem. Owen doesn't find the connection self-evident. I had hoped that my article attempted to persuade the reader with more than a simple assertion. Actually, I wonder if Owen thinks that *any* generalizations can be made about the social psychology of a particular historical epoch. Or is he suggesting that if one acknowledges the subjectivity of perception, then somehow this makes it impossible to make valid truth claims about social phenomenon? Since when does human subjectivity make scientific theory-building impossible? What kind of explanations would he take as persuasive?

Given the possibility that my description of the social context was too discursive and painted with strokes that were too broad, I suggest the following kinds of evidence that, taken together, points toward the existence of the kind of *Zeitgeist* that I am describing: 1) the decline of activism on college campuses; 2) the relative absence of a significant civil rights movement, welfare rights movement, or other organized movement to defend and improve the rights of the poor; 3) the declining willingness of our society to take care of the disadvantaged, as evidenced by systematic cuts in spending for health and welfare services since 1980; 4) the decline in the percentage of adults who vote in presidential elections to an all-time low in 1996; 5) the increasing preoccupation of the press with the personal peccadilloes of political leaders; 6) the widely reported increase in personal and bitter character assassination and acrimony among and between politicians; 7) recent polls that report that people have lost faith in the ethics of their elected leaders; 8) declines in union membership; 9) a veritable explosion of books about spirituality that repeatedly focus on the "spiritual impoverishment" and cynicism of our contemporary culture; and 10) a series of books by respected intellectuals (Jacoby, 1987; Gitlin, 1995; Lasch, 1995) that explicitly endorse the main theses of my article and that include detailed studies of the regressive political meanings of the rise of postmodernism in the academy. In addition, readers who were alive during the New Deal and World War II or who talk to people who were alive then could simply reflect on the different experiences of community and collective responsibility between then and now. I am

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arguing that there was more then and less now. Readers who remember their reactions to the messages of the Kennedys or Martin Luther King should take their own experiences and observations about the prevalence of cynicism today as validation criteria of the correctness of my hypothesis. One's personal experience is not the defining criteria, but it is a part of what has always enabled social theorists and commentators to talk about social processes.

It may be that Owen shares with most of us the resistance to being seen as a vehicle for the expression of broader social forces. As the postmodernists themselves have shown us, no one likes to see oneself as influenced by one's social context, particularly if that context is described in negative terms. Owen's dismissal of my argument as "naive objectivism" seems to flow, at best, from his disagreement with my particular thesis or, at worst, from his skepticism that any generalizations about social reality can be made at all. If it is the former, then Owen should raise a stronger challenge to my evidence than an off-the-cuff reference to militias or a quip (completely incorrect, as it turns out) about the audience for my ideas consisting of New York Review of Books readers. If it is the latter, then I would argue that this is an example of the basic postmodern problem, which is to proceed as if reality—in this case, social reality—is mysterious and opaque. The implication that social reality is not analyzable seems more naive to me than the belief that it is.

Kenneth J. Gergen

Kenneth Gergen's thoughtful discussion enables me to elaborate on the concepts of validation and hypothesis testing that are so central in my article. Gergen's article reminded me that these notions are bound to be viewed in the light of 30 years of debate about the scientific standing of psychoanalysis in general and even in light of old controversies about whether psychoanalysis should speak in the materialist language of forces or the hermeneutic language of meaning and values. By collapsing my critique of constructionist epistemology with other aspects of modernism and positivism, Gergen can dismiss my argument as a futile attempt to salvage the scientific standing of the entire leaky

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ship of psychoanalysis. Gergen has me arguing with Adolph Grunbaum, defending 19th-century hierarchies that privilege material over spiritual reality, and denying the relational basis of knowledge and meaning! Needless to say, I have no interest in being the standard-bearer for all of modernism's ills, and I believe that my concerns should be considered on their own terms.

For the postmodernist, the obvious facts of language, intersubjectivity, and the social construction of knowledge unnecessarily blur the equally obvious facts that minds are phenomenologically private and that in the course of social life it is imperative that these minds get to know each other reasonably well. That is, just because we are socially constructed, and even though the very concept of "mind" is a linguistic and cultural convention, it doesn't follow that notions of accuracy and truth disappear. Mental states can be seen as socially constructed and privately experienced and still be "known" with more or less accuracy. It is highly adaptive to be able to correctly "read" one's social world—for instance, to be able to predict with a fair degree of accuracy how your mother is going to respond to you pouring milk on your sister's head. If you don't know, you could be in danger. We need accurate information to survive. We acquire it by observation, inference, trial action, and hypothesis testing. You don't have to be a 19th-century positivist to do this; babies, adults, patients, and analysts all do it. We interact and collide with other minds as well as the material world, and we learn enough to ensure our safety, satisfy our needs, overcome obstacles, and maintain our communities. We sometimes distort what we perceive and draw the wrong conclusions. As the social constructivists argue, misunderstanding is a fact of life. But this truism misses the point that social and intersubjective life requires a high degree of accurate perception and inference as well.

Thus, when Gergen argues that we can only establish the validity of the statement "the tree will bear fruit" if we first consensually establish the meaning of the various terms of the sentence, he is at once stating the obvious and confusing the issue. It is obvious that a common language is essential for communication, whether it is among farmers or scientists. But signifiers also refer to something—a fact that would soon become evident if you tried to bite into a pinecone that you thought was fruit. We define *fruit* as something edible, and on the basis of that we can learn, through direct perception and trial and error, a great deal

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about the world. Gergen's "local agreement" about what constitutes fruit does more than simply allow for an interesting conversation or for a farmer to recognize and connect with fellow farmers; it furthers our ability to survive by making a crucial distinction between a substance that will hurt us from another one that will nourish us. The fact that we have to agree in advance that the signifier *fruit* refers to a particular substance in order to make the prediction "the tree will bear fruit" is less important than the fact that once we define our terms, we can make crucial predictions and test them with repeated observation and practice. Survival and "getting it right" are synonymous in this case.

Gergen seems to appreciate the relation between knowledge and activity. He says, for instance, that once a "local community" such as physicists agree on what constitutes an atom, they can go ahead and split it. But he then argues that the relevant discussion is whether splitting atoms contributes to society rather than whether something "real" has been accurately described. In my view, these things cannot possibly be separated. You have to be right about certain things to change them, and in the course of changing the world you learn more about it. For instance, when a baby acts on the world and tries to change it, he or she learns a lot about what is real and what is not, what is wishful thinking and what is external and fixed. The baby develops a rudimentary objectivity because it is adapted to do so. The practice of changing the world *should* intimately shape one's epistemology. What is "real" is indistinguishable from what works because it *matters* to get things right. For college activists, for instance, it matters whether the building they are going to occupy houses chemical warfare research or the campus dental clinic; the consequences of being wrong could be embarrassing. Similarly, in regards to psychotherapy, responding to a patient's question of whether you have a sliding scale by saying that he or she really wants to kill you will likely inhibit rather than facilitate the patient's ability to trust you. In addition, chances are you would be wrong.

All scientific research proceeds by specifying the meaning of its terms and the criteria for judging its results as significant or not. Therefore, it is not particularly interesting to state the obvious fact that we have to define repression to study it. It is true that repression is just a word, not a thing, and yet it is intended to describe a psychological experience that is as real as a thing in the sense that you can know what it is. It is

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real in the sense that a piece of fruit is real and understanding how repression functions and feels in a particular person is important if we want to avoid the equivalent of biting into a pine cone. In analysis, if I am correct about what you have repressed, I am a lot more likely to say and do things that will lead you to feel understood. It is likely that your anxiety will decrease and you will have a real feeling of connectedness and recognition. It will probably lead you to trust me more and help give you the courage to take on a new challenge or face a difficult truth. This is not a linear, clear-cut, cause-and-effect process, nor is there some kind of unified field theory that can explain "the mind" with oracular authority. My point is that even though knowledge about human psychology is socially constructed, it is still testable in terms that are far more similar to the conventional scientific model of hypothesis testing and prediction than, say, the model of poetics. We need not degrade the tremendous creative basis of analytic skill to also accept that some of these intuitive inferences might be subject to more logical and rigorous types of validation and testing. "Getting it right" matters, particularly if you are the patient.

Repression is a good example of a hypothesis that is useful to the extent that it is right, and it is as adaptive for the lay person to understand how repression works as for the analyst. For instance, it is crucially adaptive in everyday social life to understand that people don't always mean what they say or that we all prefer to remember the good times rather than the bad. Were we not able to discern these dynamics in other people and ourselves, we could not survive. We would be constantly biting into pine cones, taking others at face value and being traumatized when they turn out to have other agendas. We are, in other words, implicitly skeptical of manifest reality and operate socially on the basis of the presence of the existence of repression.

Finally, I take up Gergen's suggestion that once we get rid of the modernist notions of validity and efficacy we can appreciate the ways that psychoanalysis makes other contributions to therapy and society—valuing the individual and defending depth, mystery, and complexity in an environment hostile to these values. I agree with many of these sentiments: I believe in the essential worth of the individual and I believe that psychological experience is complex and often contains hidden depths of meaning. What I object to in postmodern theorizing is the tendency to make absolutist claims about complexity and to render

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psychological experience so mysterious that we cannot hope to improve our clinical methodology for understanding that experience. Gergen's argument that "in the postmodern world there are no fixed 'disorders,' 'treatments,' or 'cures'" sounds progressive at first, but I cannot figure out how it is supposed to help me treat the depressed man sitting across from me. This man seems to have a *fixed* belief that he is no good, and experience—a combination of hypothesis testing, observation, and trial and error—has taught us that if I tell him how he acquired this pathogenic belief and convey that he really deserves to be happy, he actually feels better. This is almost an invariant sequence—for a time, in other words, a *fixed* treatment—and it seems to me that it has resulted in the beginning of a *cure*. Although there is a lot that remains a mystery about this complex and multilayered patient, this fact impresses me less than our growing understanding and certainty and the patient's sense of mastery and therapeutic progress.

Again, I agree with Gergen that psychoanalysis carries many values that are under attack in contemporary society. However, postmodern epistemology undermines one of the most crucial values: understanding. Our business is to understand and to use that understanding to help cure patients of their suffering. We should find ways to make our understanding better and better, our empathy more and more attuned. We don't have to think that people are things, or that scientific progress will lead us to the Promised Land, or that spirituality is unimportant, or even that most of the truth claims of psychoanalysis have been "proven" to believe that there are systematic ways to tell if we are on the right track with our patients and to make that track even more "right." When we do so, the patient feels that something real has been touched, that there has been an authentic connection, and a process has been facilitated that leads to therapeutic change. Our patients expect us to value accurate understanding and so would view Gergen's philosophical position as confusing, if not disturbing.

If you are trying to act on the world to change it, correctly grasping certain key elements of that world will help. If you have given up on the hope of transforming social or psychological reality, you might begin to move toward privileging language over minds and viewing all knowledge claims as contingent. I believe that this is the situation within which both clinical psychoanalysis and social change movements find themselves.

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Tim Dean

Tim Dean challenges my belief in the possibility of intersubjective understanding in more ways than one. He and I seem to inhabit different universes. To the Lacanian, my endorsement of the analyst's ability to "know" a patient and my advocacy of corrective emotional experiences turn the pure gold of analysis into an abusive exercise of technical rationality and a collusion with the patient's ego and, thus, with her or his wish to remain ill. To me, Dean's account of Lacan's system reproduces the worst of the classical analytic strait jacket that argues exclusively from theory, defines analysis in the most narrow and exclusionary way, and wraps discredited notions of analytic abstinence in erudite formulations that don't have much to do with real people. While reading Dean's article, I imagined him reading mine, each of us thinking, "Does he really believe that?"

In this context, it makes sense that Dean doesn't comment much on the content of my article but, rather, uses it as a stimulus for his presentation of the radically different Lacanian system. True, there are digs about my lack of sophistication in philosophical matters and my "comic naïveté about academia," but, in the main, Dean offers us the Lacanian narrative for purposes of comparison.

I will try to follow Dean's example and use his account of Lacan to develop several ideas contained in my original article; along the way, I will try to respond to some of the specifics of Dean's discussion.

It seems to me that much of classical analytic theory and technique (and I include here its Lacanian offshoot) violates common sense and the assumptions governing normal role relationships. It intends to. Common sense, after all, was deconstructed by Freud to reveal the workings of the unconscious, and the analytic relationship was deliberately fashioned to contradict the patient's normal social expectations to get at fantasies that these expectations both expressed and hid. The problem is that common sense also contains a great deal of wisdom, and role relationships mediate the best of social life as well as the worst. By developing a theory and technique that was so far off the beaten track and that relied so much on its founder and political loyalties for its legitimacy rather than any empirical system of validation, psychoanalysis has often found itself defending ideas and techniques that

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seem counterintuitive or even bizarre. Such ideas included primary penis envy in girls; the a-social nature of infants; the exclusive motivational primacy of sex and aggression; the assumption that development ends with adolescence; the various forms of reductionism in which every fear and symptom under the sun is seen as a derivative of castration anxiety; the axiom that humor is essentially disguised aggression; and that adult choices of career, morality, or politics always importantly express infantile wishes and conflicts. These ideas often violated the common sense and intuitive sensibilities of the average person, and later research proved them to be wrong.

In the realm of technique, a similar trend can be found. I include here all of the permutations of abstinence and neutrality that have led to the familiar caricatures of the analyst never answering questions (except after analyzing them), accepting gifts, giving advice, or disclosing personal information of any kind. These "rules" violated the norms of social life to such a degree that patients regularly felt confused, rejected, and hurt, and they could only continue in such treatments through compliance. In addition, analysts maintained a skepticism about whether their theory or technique could ever be subjected to research or validation. Deprived of the orienting checks and balances of common sense, interpersonal norms, and scientific research, psychoanalysts were able to develop a theoretical system, treatment approach, and culture of training that tended toward the solipsistic; took public skepticism as confirmation in much the same way that they interpreted patients' disagreements as resistance; and argued their case on the basis of received wisdom rather than evidence.

From this point of view, much of contemporary theory can be seen as an attempt to normalize analytic theory and technique, to bring it into line with common sense and everyday social life. The analytic relationship is now construed as sharing with other relationships elements of real mutuality. Allowances can now be made for the analyst's personality to come into play, with a corresponding permission for the analyst to be more conversational and "human." The analytic relationship can even include certain self-disclosures by the analyst and feature the healing role of his or her positive regard for the patient, processes similar to those that occur in other relationships. By bringing analytic theory and technique more into line with the basic sensibilities and expectations that govern normal social life, analysis

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has been able to treat a wider range of people, make its approach more patient-specific, and produce better outcomes. We are less "weird" and more human. And although a few might disagree, I think that our field is better off for such change. In fact, one of my purposes was to argue that although postmodern epistemology made the analyst free to become more natural and spontaneous, it also ignored something that is ubiquitous in everyday social life and that reflects basic common sense: We can and do correctly "read" the psychological states of other people all of the time.

Dean, following Lacan, turns back the clock. Under the banner of fighting the good fight against American narcissism, Dean makes a series of pronouncements about what does and does not constitute analysis in which he argues exclusively from theory and appeals to authority. We are either Lacanian analysts or no analysts at all. If we are not "engaging the unconscious" as defined by Lacan, we are merely role-modeling. If the analysand acts with anything other than words, he or she is doing something other than analysis. If we don't accept Lacan's view of transference, then somehow we must be taking everything the patient does with us at face value. When Dean says, "The temptation to empathize with the patient, although completely understandable, is antipsychoanalytic insofar as it seeks to avoid unconscious material," we are led to wonder on what basis Dean can be so definitive. Unable to provide clinical data, Dean appeals to authority in his attempt to persuade: He quotes Freud. Freud, we learn, has "clarified the degree to which empathy and analysis are mutually exclusive."

This is the kind of argumentation that has marked psychoanalysis almost from the beginning. I'm wrong because Freud said I'm wrong. Because I'm not a Lacanian, I'm barely an analyst. Dean won't be distracted by evidence: He ignores my clinical example; he is not worried that non-Lacanian analyses sometimes actually produce healthier, more insightful, and happier people; and he doesn't consider it relevant to consider research inside or outside of psychoanalysis. In this sense, his rhetorical style mirrors the historic intolerance of diversity and debate that had a chilling effect on generations of analysts who were told that this or that was "not analysis."

In so doing, however, Dean, like psychoanalysis as a whole at times, leaves himself open to the criticism that he is ultimately not discussing real people, but pieces in a theoretical puzzle. Does Dean believe that

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the analyst is always, at every moment, only a transference object? Are there really no moments of authentic contact? If an analyst gives advice (e.g., recommends that a patient who is about to jump off a bridge not do so), is it always the case that he or she and the patient are locked into the imaginary register? If an analyst feels and expresses empathy, does Dean really think that this makes explorations of the patient's unconscious impossible? Can't I use my empathy to know more about what the patient is feeling? Furthermore, if I convey my empathy to a patient, isn't it possible that he or she might sometimes feel safer and better able to face his or her unconscious? It certainly has been my clinical experience that patients are better able to bring forth painful and repressed thoughts and feelings when they feel safe than when they do not. Maybe it is "easier" to be empathic not because of the inherent discomfort and anxiety of facing the unconscious as a "true" analyst, but because sometimes a relationship that feels easier helps the patient trust the analyst enough to risk a scary self-exploration. Dean's problem in arguing the case is *not* a function of the fact that he's not a clinician. Lacan was a clinician, and he didn't rely on clinical evidence either. In my view, the real problem is that this position is entirely theory driven.

In a sense, Dean and Lacan's theory is one you can't tell a patient, at least if you want the patient to return, because it violates the patient's common sense, reasonable social expectations, and need for the analyst to be of concrete help. It reminds me of a training analyst who once remarked in a conference, "My job is to analyze, not cure." Except for the poor candidates who had to be in treatment with him, one wonders how many of his other patients would stay if they knew that this was his theory. If an analyst, following Dean, explained to a patient that this was "not an interpersonal relationship," that "empathy and analysis were mutually exclusive," and that "unlike your therapist, the Lacanian analyst will never tell you what to do," would that patient fork over the money to continue seeing that analyst?

My argument can obviously be viewed as taking cheap shots because 1) there should be room for intellectual debate in psychoanalysis without appeals to evidence; 2) "evidence" is inherently biased and ambiguous anyway; 3) a Lacanian analyst might well be able to translate his or her theory into everyday language that would make sense to a patient; and 4) Lacanians are no doubt helping patients all over the

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world at this very moment. I'm certainly aware that I am open to the charge of misrepresenting Lacan and Dean, who, after all, is only giving the slimmest and most cursory introduction to a complex theory. Unfortunately, Dean's discussion has to stand on its own. A theory should be describable in language that is accessible and intuitively convincing, and Dean's effort fails in this regard. But these caveats miss the issue that is of primary concern to me in the article to which Dean has responded and this discussion. Lacanian theory, like postmodern theory, leaves the clinician floating in epistemological space when it comes to providing concrete and useful guidelines to technique. It is not interested in validation and is skeptical of focusing on therapeutic results; it is highly abstract and invites an idealization of philosophical discourse; and it undermines analysts' confidence that they are helping patients by sowing doubts about the legitimacy of an ethic of helping to begin with. In this way, it shares with postmodernism an antitherapeutic bias that is especially troubling in today's climate.

Following Lacan, Dean does make a reasonable argument for the analyst listening to the patient in a way that does not foreclose the emergence of unconscious meaning. In my terms—quite different, I understand, from Dean's—the analyst should be modest and open to the continued corrective feedback of the patient. However, I think that Lacanians both under- and overestimate the analyst's influence on the patient. They underestimate it insofar as they presume that the analyst can possibly operate, even for one nanosecond, without goals and ambitions for the patient or that the analyst can obviate the effect of these aims through interpretation or "positive nonacting." Like the ethic of neutrality and abstinence, Lacan's injunction that the analyst "pay with his person" is an impossible and undesirable ideal. We all want things from our patients, and if we did not we would be useless to them. At the least, analysts want their patients to get better, and we have gone to school for decades to learn how to facilitate this. If Dean thinks that an analyst's commitment to "directing the treatment" is not as powerful an interpersonal influence as "directing the patient," he is deceiving himself. A Lacanian analyst has an agenda for the patient that reflects his or her theory, personal preferences, and unconscious dispositions, and the patient senses and is responsive to all of it. The question for analysts isn't whether to influence, but how to influence in

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a way that is maximally respectful of the patient's autonomy and sensitive to the patient's needs.

On the other hand, Dean also overestimates the analyst's influence. In the Lacanian universe, the fact that the patient locates the analyst "in the realm of the Other" is cause for great alarm because the analyst can easily become an abusive authority. I share this concern in asmuch as analysts routinely think that they are smarter than they are; their technical stance of neutrality and abstinence reinforces this myth and invites the patient's compliance. And sometimes, of course, the patient's transference dependence is exploited more overtly and destructively by the analyst. But Dean's position overestimates the analyst by underestimating the patient. First, his theory doesn't appreciate the obvious fact that the analyst is not always *only* a transference object any more than a spouse, best friend, or teacher is. The idea that the analyst only occupies the realm of the Other violates common sense and makes it hard to understand how an adult could ever be affected by or change in response to new social experiences. Second, Dean's theory blinds him to the patient's ability to correctly perceive the analyst's actual psychology. His theory suffers from a version of what Hoffman (1983) called the "fallacy of the naive patient" in which the analyst's unconscious is forever hidden from the patient because of the patient's psychopathology. In my view, patients often correctly read the analyst's motives and intentions and "cover" for the analyst in the same way they did with weak or troubled parents. But to cover for the analyst is not the same as being a helpless victim, and Dean can't distinguish between the two. Patients are more resilient and prescient—and analysts less powerful—than Dean thinks. Dean's depiction of the abusive narcissistic analyst dominating a patient trapped in a hall of transference mirrors reflects a cardboard analysis, not a real one.

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