

CLINICAL AND OBSERVATIONAL PSYCHOANALYTIC RESEARCH: ROOTS OF A CONTROVERSY. Edited by *Joseph Sandler, Anne-Marie Sandler, and Rosemary Davies*. Madison, CT: International Universities Press, 2001, \$30.55 hardcover, \$27.00 paperback.

Claude Levi-Strauss said the scientist is not necessarily the person who gives the right answers, but the person who asks the right questions. This volume illustrates that some psychoanalysts are still a long way from asking helpful questions about the role of research. The volume contains the edited proceedings of a conference *cum* debate, primarily between Daniel Stern and André Green, on the value of empirical research to psychoanalysis. The conference organizers and editors of this volume clearly intended to illuminate. Instead, the book reinforces the same unhelpful assumptions that have squelched psychoanalytic research for generations.

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André Green casts himself in the role of arch antiempiricist and staunch defender of the psychoanalytic faith, or his version of it. He sees empirical research as not only irrelevant to psychoanalysis but as a subversive threat to its spirit. He impugns the credentials of psychoanalytic researchers and implies they are unqualified to be psychoanalysts: “Being listed in the membership roster of the International Psychoanalytical Association is no longer enough,” he writes. “Screen memories are a psychoanalytic notion. We could also speak of ‘screen trainings’” (p. 26).

Green opens the debate with a fallacy: “the noble term ‘research’ carries such an amount of prestige that it is to be expected that any reference to it might compel one to bow before it. Unfortunately, compared with the richness of the clinical experience of psychoanalysis, the findings of researchers look very meager” (p. 21). The fallacy is that psychoanalysis has accorded research undue prestige. Green adopts the aggrieved tone of the marginalized minority, as if his were a lone voice in a wilderness of analysts who privilege empirical data over clinical experience. Of course the opposite is true. Historically, psychoanalysis has greeted research with indifference (for a discussion of the complex and problematic relationship between psychoanalysis and academic research, see Hornstein 1992).

Glen Gabbard has described the situation more honestly: “As co-chair (and a longtime member) of the program committee of the American Psychoanalytic Association, I have seen a highly disconcert-

ing trend. Most presentations with the word ‘research’ in the title were voted down by the committee. We had a format for many years entitled ‘Research in Progress,’ where analytic researchers could present their work and receive feedback from the audience. Sadly, these presentations were poorly attended” (Chiarandini and Gabbard 1999).

This historical indifference toward research bears on Green’s next comment, that its contributions have been meager. There are two responses. The first is *what* research? There are few studies of the psychoanalytic process, and few analysts have made their work available for independent scrutiny. Compared to the mountains of clinical contributions (and the voluminous research literature on other forms of psychotherapy), the number of psychoanalytic empirical publications is an anthill. So if it is true that research has contributed little, we must ask, How could it be otherwise? Attitudes like Green’s have long discouraged psychoanalytic research, and now he points to its absence to rationalize that very attitude.

If we broaden our focus to include empirical studies that bear on psychoanalytic knowledge, we can respond differently. There are hundreds. Some personal favorites include the empirical demonstration that the more closely psychotherapists adhere to a psychoanalytic approach, the better the treatment outcome, regardless of the kind of therapy they *think* they are practicing (Ablon and Jones 1998); the finding that disclosure and exploration of the meaning of distressing experiences improve immune functioning (Pennebaker 1997; Pennebaker, Kiecolt-Glaser, and Glaser 1988); the mapping of unconscious associative networks and the demonstration that conscious and unconscious motives are often contradictory (Weinberger in press); the empirical finding that sexual desires find expression through disguised symbolic derivatives, much as Freud described (Mussen and Scodel 1955); experimental demonstrations of the distorting impact of transference on perceptions of others (Anderson and Baum 1994; Anderson and Cole 1991); and my own research showing that defensive denial of distress has physiological consequences and increases the risk of heart disease and other illnesses (Shedler, Mayman, and Manis 1993). Westen (1998, 1999) has reviewed many studies that bear on psychoanalytic conceptions of the unconscious. The series *Empirical Studies of Psychoanalytic Theories* now spans ten volumes (see, e.g., Bornstein and Masling 1998).

The contributors to this volume make no mention of such research. The debate focuses instead on infant research as *the* paradigm for

psychoanalytic research. This focus seems odd, since infants don't seek psychoanalytic treatment, report their associations, or form transferences that lend themselves to analytic exploration. The findings of infant researchers, however fascinating, are removed from the day-to-day practice of most psychoanalysts. Stern acknowledges this. Instead he maintains, modestly, that infant research is *indirectly* relevant to psychoanalysis. His argument is simply that there should be some relatedness between psychoanalytic knowledge and knowledge in other disciplines. "When this relatedness is broken or becomes weakened, the psychoanalytic discourse stops being interesting and gets left behind" (p. 75).

Stern's argument is so modest and reasonable that it is hard to imagine anyone would disagree, although Green and other conference participants disagree vehemently. For Green, "The greater risk for the future of psychoanalysis is the decline and fall of the spirit of psychoanalysis," and it is our "moral task" to keep this spirit alive (p. 26). Far from keeping psychoanalysis alive, such attitudes have strangled it—a point to which I will return. Note, however, that the debate has taken a curious turn. Stern accepts the premise that infant research (which in this volume appears to stand for *all* research) cannot contribute directly to psychoanalytic knowledge. In arguing for an indirect role, he too marginalizes research, implicitly relegating it to the sidelines of psychoanalytic inquiry. Thus, the debate is really whether research should have a marginal role or none at all.

The argument against research. I have thus far not addressed Green's central argument. It is that empirical research is concerned with what can be observed, and psychoanalysis is explicitly concerned with what *cannot* be observed. The unconscious is the proper subject of psychoanalysis, and empirical research methods are unsuited to the study of unconscious mental life. The phenomena of greatest interest to psychoanalysis—for example, conflict and compromise formation, transference, and resistance—can only be deduced or inferred, never observed directly. Empirical efforts to study psychoanalytic concepts can lead only to their trivialization, and to a simplistic reductionism that does violence to their spirit.

Green applies this reasoning to infant research and argues that the research can never, in principle, contribute to psychoanalytic knowledge because it is based on observation of behavior. Psychoanalysis, in contrast, is not concerned with behavior, but with its *meaning*. Thus, "no infant researcher could have discovered the concepts of projective iden-

tification or of reparation, which were drawn from the couch” (p. 65). Further: “Psychology is bound to restrict its domain to consciousness . . . , whereas psychoanalytic metapsychology deals with what is beyond the walls of consciousness” (p. 25).

It is time to lay such arguments to rest. First, research need not restrict its domain to consciousness, as many of the studies cited above attest, and as I will further illustrate below. Second, such arguments betray an either/or approach to scientific inquiry that is unsophisticated and unhelpful. Empirical research cannot and should not replace inferences and deductions drawn from the couch, but can complement them synergistically. Philosophers of science distinguish two aspects of scientific inquiry: the “context of discovery” and the “context of justification” (Reichenbach 1938). The former refers to the circumstances appropriate to *generating* hypotheses, in which inference and deduction are paramount. The latter refers to the circumstances appropriate to *testing* the resulting hypotheses, which requires different methods.

The psychoanalytic encounter is the “context of discovery” par excellence. We may agree that concepts like projective identification could have arisen only from the couch. But the moment one wishes to generalize from one patient to people in general—that is, the moment one wishes to articulate general principles, or theory—then we require additional methodologies to provide checks and balances. Without a credible context of justification, a discipline becomes a hodgepodge of competing theories with no systematic method for sifting sound from unsound beliefs. The progress of knowledge requires both contexts: it is a matter of both/and, not either/or.

Psychoanalysis is, of course, characterized by competing models, with different treatment implications. As McWilliams (in press) put it, “The historical stew of psychodynamic theory and practice, from Freud on, is peppered with enough conflict, disagreement and schism to rival some medieval heresy controversies.” Theoretical beliefs too often seem determined by identifications with one’s own analyst, supervisors, and institute, not evidence. Questions that are inherently empirical too often get resolved in favor of seniority, charisma, or appeal to authority. It is impossible to overstate the extent to which this has damaged our credibility in the eyes of other mental health professionals and the educated public (e.g., Crews 1993). Green illustrates this very problem when he speaks of “screen trainings” and dismisses so categorically the views of other analysts. The problem, of course, is that these analysts

have undergone analytic training; they just understand things differently. The solution to such schisms is not more rhetoric. Instead we need a systematic basis for sifting sound from unsound ideas. *That* is the role of research.

Harnessing psychoanalytic inference. Can empirical methods address such issues as transference, resistance, conflict, and unconscious mental life more generally? They can, but doing so requires a change in the way most of us think about research. Historically, psychological and psychiatric researchers have tried to maximize the reliability or reproducibility of their measures by eliminating clinical deduction and inference, or reducing it to the lowest common denominator. An alternative is to *harness* clinical deductions and inferences.

An example of this approach is the Shedler-Westen Assessment Procedure (SWAP), an instrument that allows a psychoanalyst to describe a patient's personality organization (Shedler 2000; Shedler and Westen in press a,b; Westen and Shedler 1999a,b). It consists of two hundred statements that the analyst arranges into categories according to their relevance. Combinations of statements allow the analyst to describe complex psychoanalytic constructs (e.g., characteristic defenses, conflicts, fantasies, self- and object representations, transference propensities, drive derivatives). Green cites "projective identification" as a concept that does not lend itself to empirical research. However, the following three SWAP items, taken in combination, capture at least one meaning of the term, consistent with Kernberg's usage (1984; Clarkin, Yeomans, and Kernberg 1999): (1) tends to see own unacceptable feelings or impulses in other people instead of in him/herself; (2) manages to elicit in others feelings similar to those he or she is experiencing (e.g., when angry, acts in such a way as to provoke anger in others; when anxious, acts in such a way as to induce anxiety in others); (3) tends to draw others into scenarios, or "pull" them into roles, that feel alien or unfamiliar (e.g., being uncharacteristically cruel, feeling like the only person in the world who can help, etc.).

Psychoanalysts who have used SWAP report that it captures their formulations quite well (Shedler 2002). The instrument allows us to test and refine widely held psychoanalytic beliefs about hysterical, obsessional, paranoid, and other familiar personality syndromes. It has allowed us to develop a classification of personality syndromes (as an alternative to Axis II of DSM-IV) based on character organization, not manifest symptoms (Shedler 2002; Shedler and Westen in press b; Westen

and Shedler 1999b). It can measure intrapsychic or “structural” change (vs. symptom remission) during treatment, and thus demonstrate some of the unique benefits of psychoanalytic therapies (e.g., Josephs et al. 2003; Lingiard, Gazzillo, and Porzio Giusto 2003; Cogan and Porcerelli 2003).

The Psychotherapy Process Q-Sort, or PQS, (Jones 2000) is another example of a research tool that harnesses clinical inference. It provides a systematic method for describing the clinical interaction, providing quantifiable answers to questions like the following: To what extent does the therapist offer supportive or educative comments, rather than maintain an analytic stance? Does he or she consistently interpret defenses? Is this done with timing and tact? Does he or she point out similarities between the therapeutic relationship and other relationships? Most important, the PQS allows us to examine relationships between technique and outcome. One profoundly important finding (published in the psychological literature but virtually unknown in the psychoanalytic world) is that the more closely a therapist adheres to a psychoanalytic approach (i.e., interpreting transference and resistance), the better the outcome—even when the therapist purports to be practicing cognitive-behavior therapy (Ablon and Jones 1998; Jones and Pulos 1993).

These instruments take as their subject matter the inferences and deductions of clinicians, not simple behavior. Thus, an emphasis on deduction and inference need not preclude the use of empirical methods. Indeed, the title of this book—*Clinical and Observational Psychoanalytic Research*—implies a dichotomy where none need exist.

Research does require reliable or reproducible findings. Some contributors to this volume appear to consider it heresy to question the reliability of psychoanalytic formulations, but Freud (1916–1917) explicitly recognized its importance. For example, in discussing the technique of dream interpretation, he anticipated the criticism that “so much room is left to the interpreter’s arbitrary decisions as to be incompatible with objective certainty . . .” (p. 228). Freud’s response was that we can establish the soundness of the psychoanalytic method “if we *compare the results* produced by correctly trained analysts” (p. 232; emphasis added). Freud made an analogy with the deciphering of ancient cuneiform, which had been widely considered a hoax until a “decisive experiment” demonstrated that independent scholars could arrive at similar translations. Freud’s logic is exactly the logic of contemporary research: he was describing interrater reliability.

All of which brings us back to the question of the “spirit” of psychoanalysis. For a growing number of psychoanalysts, this spirit is one of open-minded inquiry. It is not about ossification of theoretical beliefs; it is not about privileging authority or clever argumentation over evidence; and it is not about insulating ourselves from knowledge and methodologies that happen to fall outside the domain of our personal expertise. Serious challenges seriously addressed do not undermine a discipline, they strengthen it.

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