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ROBERT S. WALLERSTEIN, M.D.

PSYCHOANALYTIC THERAPY RESEARCH: A COMMENTARY

Abstract. Psychoanalysis has had two contending perspectives on how to most effectively enhance its theoretical and clinical knowledge base: the traditional intensive case study method innovated by Freud (dubbed *qualitative research*) and the later developed formal empirical research in accord with the usual canons of objective, natural science (*quantitative research*). An article by Irwin Hoffman (2009), arguing that objective empirical research should not be privileged over traditional subjective intensive case study as an avenue to psychoanalytic knowledge increase, has aroused multiple responses, pro and con, by psychoanalytic clinicians and researchers. This article assesses four of those responses.

Keywords: psychotherapy research, psychodynamic psychotherapy, empirically supported treatments, evidence-based medicine, randomized control trial, psychoanalytic therapy research, clinical case studies

In 2009, Irwin Hoffman (in earlier years, Merton Gill's research partner in Gill's empirical research studies on the nature and process of psychoanalytic therapy) published in the *Journal of the American Psychoanalytic Association* an overview of his position on the long-standing comparison of intensive clinical case studies and formal empirical research as avenues for advancing psychoanalytic knowledge. Hoffman has long been known as an articulate and persuasive champion of the unmatched power of the intensive psychoanalytic study of the clinical case in process as an essential avenue to the growth of our theoretical and technical psychoanalytic knowledge base.

Address correspondence to Robert S. Wallerstein, M.D., 290 Beach Road, Belvedere, CA 94920. E-mail: judywall@comcast.net

In his very tempered article, Hoffman (2009) by no means embraced the extreme position that psychoanalysis is so distinctively a discipline sui generis, and is so disconnected from the other disciplines devoted to the accrual of knowledge of human mental functioning—of which I regard André Green (1996a, 1996b, 2000) as such a powerful proponent—that only the psychoanalytic situation studied from within by its participants can truly yield knowledge relevant to psychoanalytic theory and practice. Hoffman's posture in his article was rather to decry the claim of so many who are committed to formal empirical research embedded within the so-called natural science canons of research (e.g., objectivity, adequate sampling, statistical power, efforts towards reliability, and validity): these researchers privilege their findings (because established within the context of justification) over the prior naturalistic observations (which led to the formal empirical studies) because, in their view, the naturalistic observations only occurred within the context of discovery and, therefore, require more definitive and formal study to be scientifically credible.

Hoffman's argument was to reject the privileging of the one research avenue over the other; in his view, both could have their place in the advance of psychoanalytic knowledge, if deployed appropriately in regard to the psychoanalytic issue under question; and both approaches could be strongly supported by the argument that the same subjectivity and making research choices guided by our life experience, training indoctrination, and character propensities, operates similarly, though perhaps differently balanced, in both research avenues: the clinical qualitative and the formal quantitative. Hoffman declared, therefore, that the privileged status so usually accorded to systematic research is "unwarranted epistemologically and potentially damaging both to the development of our understanding of the analytic process itself and to the quality of our clinical work" (Hoffman, 2009, p. 1044).

In three successive issues—22(6), 23(1), and 23(2)—*Psychoanalytic Dialogues* published commentaries on Hoffman's article and his central thesis, along with Hoffman's response to the commentaries. Introducing this series, Lew Aron (2012) stated that "Hoffman's (2009) tour de force shook up the psychoanalytic community with its forcefully argued thesis" (p. 708); this was followed with commentaries by Jeremy Safran (2012), and—in the subsequent issue (2013)—commentaries by Donnel Stern and Peter Fonagy, two of the three (Safran and Fonagy) being well-known and established systematic psychoanalytic therapy researchers.

Safran (2012), in his commentary, and Hoffman (2012), in his response, argue differing positions forcefully, but respectfully, each according points to the other while holding firmly to his own perspective—the central value of the intensively studied psychoanalytic case versus the central value of the psychoanalytically conceived and deployed formal research study—and each noting major conceptual agreements in their research study positions. Together, they also identify the (at least implicitly argued) privileging tendencies by the other, in favor of each's preferred research position: clinical qualitative or formal quantitative. Both are psychoanalysts committed to the dissemination of enhanced psychoanalytic knowledge.

As a psychoanalytic clinician, educator, and researcher who has been committed over my professional lifetime to the advancement of psychoanalysis as a science of the mind, to be enhanced incrementally by psychoanalytically conceptualized formal research study, my own position in this discourse (as is well-known) is in accord with Jeremy Safran's point of view. But in my opinion, the Safran–Hoffman interchange, an argument about the values in each research posture, was argued to a draw, something I trust Aron, Safran, and Hoffman would all agree upon. I appreciate that my summary of these articles does not do justice to the three protagonists I quote to this point, and I urge the interested reader to study the three articles in *Psychoanalytic Dialogues* for a much fuller exposition of these central, ongoing issues of psychoanalytic contention, and of course, to read Hoffman's earlier statement in the *Journal of the American Psychoanalytic Association* (Hoffman, 2009).

I have stated what I believe to be a central issue, concerning the ways to advance psychoanalytic knowledge, in order to set the frame for my commentary on four of the previous articles, all of which can be considered statements in this ongoing analytic dialogue, and to indicate my own predilection in this debate as well. (For a fuller statement of my own research activities and research posture, see Wallerstein, 1986a, 1986b, 1988, 2009). Both Hoffman and Safran, I'm confident, would agree that the four new articles on psychotherapy research that I consider here would be placed outside their joint conceptual framework, at the extreme end—despite psychoanalytic researchers being associated with all four—of the objective natural science posture, whereby the randomized control trial (RCT), with all the accoutrements of adequate sampling, statistical controls, and detached neutral third party investigator, are the declared and widely accepted "gold standard" of psychotherapy research.

Within this framework, Leichsenring, Klein, and Salzer (this issue), seminal psychodynamic psychotherapy researchers in Germany, have undertaken a 2013 update of empirical evidence for the effectiveness of psychodynamic therapies, by doing comparative systematic meta-analyses of as many psychodynamic psychotherapy process and outcome studies as met their criteria for inclusion. Out of the 114 references that they accumulated, the authors found 44 that they felt were proper RCTs for inclusion, comparing findings of the psychoanalytically conceived therapies, usually based on the short-term models of Lester Luborsky and David Malan; the specific transference-focused psychotherapy (TFP) devised by Kernberg, Clarkin and their group; and the mentalization-based psychotherapy (MBT) devised by Fonagy, Bateman, and their group; with either cognitive-behavioral models (or dialectical behavioral models); pharmacotherapy; group therapy or hypnotherapy; guided self-help groups; untreated controls, or treatment as usual (TAU) controls (whatever that comprises) or more than one of the above. What many psychoanalytic clinicians or researchers would object to, of course, among other things, would be (1) the assumption that a brief psychodynamic therapy would adequately represent what the usual open-ended and long-term psychoanalytic treatment could properly accomplish, and (2) that randomization is not the way the natural clinical marketplace operates. Treatment characteristically begins with the effort of the practitioner, of whatever theoretical bent, to assess the fit of what is being theoretically offered against the clinical exigencies of the prospective patient, and is therefore a guided, not a randomized, treatment placement.

The symptomatic clinical entities covered comprised a vast array, depressions of varying depth, anxiety disorders (including panic states, social phobias, and generalized anxiety disorders), mixed anxiety and depressive states, PTSD, somatoform disorders (e.g., dyspepsia, irritable bowel disorders), bulimia nervosa, anorexia nervosa, binge eating disorders, substance related disorders (alcohol, cocaine, and opiate, with or without specific drug counseling), borderline personality disorders, or varying clusters of DSM personality disorders. This is surely a very complex grouping of illness categories, of widely varying degrees of severity, and also, of treatability.

What has actually come out of this arduous and time-consuming endeavor? This, they state summarily in the last sentence of the article.

Given that the researchers only included studies using the RCT model (a model widely accepted within the natural sciences research world as the "gold standard" of empirical psychotherapy research), they conclude that treatment by psychodynamic therapy is just as evidence-based as the rival cognitive-behavioral or pharmacotherapy models declare their models to be; that the psychodynamic therapies, in some cases, achieve better assessed outcomes; in some cases they are roughly equal, perhaps better according to some outcome measures, and not as good on others; and in perhaps some of the studies, they achieve something less. The last sentence of the article states, "The perception that PDT lacks empirical support is not consistent with available empirical evidence and may reflect selective dissemination of research findings" (Leichsenring et al., this issue, p. 120). Further, they reference a 2010 article by Jonathan Shedler, an American psychoanalytic psychotherapy researcher, as providing explicit and detailed support of their position.

Rolf Sandell, another established psychoanalytic psychotherapy researcher, working in Sweden, offers a similar comparative study, and he propounds the value—in fact, what he believes is the necessity—of maintaining what he dubs "double vision." For Sandell, this involves setting up research strategies that encompass both the generalizable and the individual, i.e., the province of both Safran and Hoffman, the formal systematic research program, and the intensive individual case study; both, not just one or the other.

Sandell begins by citing Arnold Cooper's description of the audience's reaction to Irwin Hoffman's (2009) plenary address—the "'wildly enthusiastic standing ovation that outdid anything I had previously seen or heard at the American'" (p. 44)—and then he states that "what bothers me [more than Hoffman's stated arguments] is the reaction they met, as if there were a war" (p. 44).

From there, Sandell states his own response to what he sees as Hoffman's major thesis, also indicating its central avowal by André Green (1996a, 1996b, 2000): that psychoanalysis, by its nature, is concerned with the study of the unconscious, that is, unobservable phenomena, which are, therefore, not reachable by "positivistic" or "objectivistic" methods. Sandell's response is that, by its nature, the whole of psychology, dealing with mental phenomena, is concerned with unobservables (except for those phenomena observed by what he calls "radical behaviorists"

[p. 45]). He conceptualizes psychology as involved in the "'indirect observation' of unobservable phenomena" (p. 45), citing, as examples, intelligence and memory, which no one can *see* directly.[1] And Sandell quotes widely known and highly respected psychoanalytic research programs, from the Mt. Zion Psychotherapy Research Group (San Francisco) and the Ulm University Group (Germany), which demonstrate the emergence of (previously unconscious) "'*warded off* mental contents'" (p. 46) and not necessarily by psychoanalytic methods alone.

Sandell then presents a summary of his group's research findings, a comparative study of 413 patients, who are in three types of treatment: psychoanalysis, psychoanalytic psychotherapy, and cognitive behavioral therapy. Using a variety of methods to assess outcome, he demonstrates, in accord with his "double vision," two findings: (1) what is generally expectable, in terms of the operation of generally agreed psychoanalytic conceptualizations of the working of the human mind, regularities that are called "evidence" in usual outcome research, and (2) a focus on the individually unique differences that make each of us distinct from all others in our unique synthesis of our constitutional givens, our particular life experiences, and our growth-promoting and traumatic experience.

The other two articles report the work and findings of two psychoanalytically based psychotherapy research groups, the Kernberg-Clarkin group at Cornell University Medical School in New York (Diamond et al.), and the group of Frans De Jonghe in Amsterdam (Dekker et al.). Both trace their long-term activities using the same conceptual framework of comparative RCTs, within the objectivist natural science epistemology.

The New York group (Diamond et al.) assesses changes in attachment as measured by the adult attachment interview (AAI), and related changes in mentalization, as measured by the reflective functioning (RF) scale. They use both measures to assess changes wrought by their manualized variant of psychoanalytic psychotherapy, transference-focused psychotherapy (TFP), when deployed in a comparative study of two patient groups, borderline personality disorders comorbid with narcissistic

[1] Here he differs somewhat from André Green, who does not regard psychoanalysis as a psychology at all, but rather as a discipline sui generis, distinct from psychology, which has its separate existence and separate findings, useful as they are, but not at all applicable to psychoanalysis, whose findings and theorizing can only come from the interaction within the psychoanalytic consulting room itself. See, in this connection, Green (1996a, 1996b, 2000) and Wallerstein (1996).

personality disorder (BPD/NPD), and borderline personality disorders alone.

Ninety patients with BPD alone were randomly assigned to one of three treatment groups: TFP (transference-focused psychotherapy, their devised treatment plan for these patient categories) in two sessions weekly; Marcia Linehan's DBT (dialectical-behavior therapy); or the traditional supportive psychodynamic psychotherapy. In this RCT, they found that in terms of positive changes in attachment status (various insecure attachments moving towards secure) and in reflective functioning (being better able to reflect intentional mental states), at the one-year mark, their own TFP outranked the two other patient groups, which were assigned randomly to the other (DBT or traditional supportive therapy) therapeutic approaches.

These overall statements are supplemented by two case reports; Sara, who had the two comorbid disorders, and Helena, who had the borderline disorder. Case descriptions are offered of both patients, giving an adequate picture of their character organizations and of experienced major life events, some of them expectedly traumatic. But not a word is given of the happenings within the therapeutic process in either case, nor of course in any of the other individual therapies studied in the RCT. Thus, though we are informed that the group's TFP did better than the other treatment modalities (the "what" of the study's outcome), there is nothing of the "why" or the "how" that sheds any light on what makes the TFP, when applied randomly across the board, a more successful treatment.

We do learn, however, that Sara, with the comorbid personality structure, differed from Helena, who had BPD alone. In her response to the TFP, at least to *my* surprise, Sara, with the more complex personality structure, did better on the measured AAI and RF. Of this, they only say, toward the end of their article, "Although improvement in RF and coherence in Sara's case is consistent with the overall study findings described earlier, the stasis in Helena's RF and coherence ratings was not consistent with the significant improvement in these ratings seen for the majority of patients in TFP in the overall study" (p. 203). The question that arose for me was this: Have we learned anything from this study about what makes TFP a therapy superior to dialectical-behavior therapy or traditional supportive psychodynamic therapy, at least for the quite severe patient categories treated and studied this way? Or is the study addressed to that question at all?

The research by the Amsterdam group (Dekker et al.) is set in the same conceptual frame as that of the New York group; it is comprised of a sequence of five RCTs over several decades, each contributing to its final total sample of 900 patients. However, it is focused on only one therapeutic mode—which they have manualized and designated as Short Psychodynamic Supportive Psychotherapy (SPSP)—and it is devoted to the therapy of but one condition, depression. SPSP seems to be constructed out of a mélange of models of brief psychoanalytically conceptualized treatment modes, and they mention the names of Malan, Luborsky, Mann, Sifneos, Davanloo, and Strupp, all of whom have published book-length descriptions of their individual model. The therapy offered in this study comprised 8 to 16 sessions, like some of those whom they acknowledge as their forebears. Some of the patients have had concomitant pharmacotherapy, handled in each such case by a medical colleague.

The outcome measure used was the Hamilton Depression Rating Scale (HAM-D), a judgment by the outside research observer, supplemented by a patient self-report, the Symptom Check List for depression (SCL-Depression). Each therapy case was supervised by a senior colleague or by peers within a group. Three successive meta-analyses have demonstrated that this "psychodynamic therapy" is more effective than the control circumstances, and is therefore empirically validated. In regard to concomitant pharmacotherapy, used or added when it seemed clinically indicated, at times the combined therapy seemed more effective than SPSP alone (the first meta-analysis), or not significantly different (the second), or it was less clear to me (the third).

As is the case with the New York group, the Amsterdam group does not report any therapeutic process, but they did make more of an effort to mention the characteristics of the treatment modality (SPSP). Thus, they say that it provides "proper gratification of unmet developmental needs" (p. 137); covers six domains: "sexuality, aggression, the need to engage in relationships, and the need to be protected, loved, and esteemed" (p. 137); and "recognizes the existence of transference but does not interpret it" (p. 137). They mention the supportive attitudes conveyed by the SPSP, "being empathic, accepting, committed, active, flexible, clear, definite, patient, and persistent" (p. 138), and the supportive techniques deployed, "reducing guilt, shame, and isolation, clarifying, confronting, rationalizing, enhancing self-esteem, advising, and modeling" (p. 138). This last list is somewhat reminiscent of Edward Bibring's (1954) article, which described five essential therapeutic techniques (each of which,

however, was defined at length with a description of how it could be used clinically) that, differentially used, would mark the differences between proper psychoanalysis and the psychodynamic psychotherapies. As to whether the stated descriptions by the Amsterdam Group give us more knowledge of what actually transpires in the SPSP treatment cases, each reader can decide for him- or herself.

With the Amsterdam group's research, the same questions will arise for the clinical reader as arose with the New York group's research, since the conceptual framework of the RCT, with its objective research requirements, is the same; and the same kinds of outcome measures, objective rating scales, are used, even though the treatment modalities under study, and the target clinical populations that comprise the sample in the two studies, differ. But the question is the same: Have we learned anything clinically helpful about what makes SPSP a more effective treatment than its controls?

Where does the consideration of all four articles in this sequence leave us? Certainly they have achieved their avowed aim. Using the established scientific canons of the RCT and widely accepted and used rating scales, they have presented adequate data to warrant their achieved goal, that psychodynamic (psychoanalytically based) psychotherapies are, broadly speaking, at least equally effective by their criteria (and in some instances, or in some studies, more so), and just as much evidence-based as their competing psychotherapeutic modalities (cognitive-behavioral or dialectic-behavioral), or pharmacotherapies, which have long declared their scientific bona fides as evidence-based within the rule of objective natural science. And this may be very useful, indeed, when making claims for monetary reimbursement for mental health care with the government or insurance companies, or when needed for research or epidemiological sampling studies. Regarding the usefulness for the psychoanalytic clinician, educator, or researcher who is operating within the kind of conceptual framework to which, I think, both Hoffman and Safran would subscribe, this is, to me, a more mixed picture.

It does help increase our credibility with cognate disciplines (developmental and clinical psychology, neural and cognitive science), no matter how much we individually may feel it reflects an oversimplified or even misconceived representation of our nature and operation as a discipline. On the other hand, and this, again, is from my perspective (represented fully in operation in Wallerstein [1986a], my book-length account of the 30-year-long Psychotherapy Research Project of the Menninger