

Annual Meeting of the *Rapaport-Klein Study Group*
Austen Riggs Center, Stockbridge, Massachusetts, June 11-13, 2010

Panel discussion on "Clinical and Empirical Issues: Disagreements and Agreements"

(The background for this panel is the controversy stirred by Irwin Hoffman's article

["Doublethinking our way to 'scientific' legitimacy: The desiccation of human experience."](#)

Journal of the American Psychoanalytic Association, 2009, 57, 5: 1043-1069)

Discussion by Morris N. Eagle

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In issue no. 5/2009 (Vol. 57, pp. 1043-1069) of the *Journal of the American Psychoanalytic Association*, the lead article is Irwin Hoffman's plenary address, given at the 2007 Winter meetings of the *American Psychoanalytic Association*. Hoffman's paper calls for a careful and critical evaluation and reply not only in its own right, but also because it appears to reflect the attitudes and values of many analysts, as evidenced by the standing ovation it received following its delivery. It has also, understandably, triggered many unfavorable and critical reactions among psychodynamic researchers and those who believe in the importance of research on psychodynamic issues.

In what follows, I will state and then critically evaluate each of Hoffman's major arguments.

(1) The "privileged status" accorded to systematic empirical research on psychoanalytic process and outcome «as against in-depth case studies is unwarranted epistemologically and is potentially damaging to the development of our understanding of the analytic process itself and to the quality of our clinical work» (p. 1043).

A general debate between the epistemological status of systematic empirical research versus case studies is fruitless. Each makes different knowledge claims. If I want to know something about a particular person, I am likely better off turning to an in-depth case study as the source of knowledge (although it will depend on the quality of the case study). If, however, I want to determine the general outcome of a therapeutic approach, I should look to systematic empirical research. Systematic empirical research may, of course, vary in quality, adequacy, and "ecological validity"—just as case studies may. However, as far as the appropriate means and methodology is concerned, for certain purposes, the privileged status accorded to systematic empirical research *is* epistemologically warranted, just as for other purposes, the privileged status accorded to the method of case studies is warranted.

It is, of course, true, as Hoffman argues, that group results may not be applicable to a particular patient and that one should, therefore, tread carefully in employing such results in understanding a particular case. But it does not follow that keeping in mind group results is necessarily irrelevant or harmful in trying to understand a particular patient. Let me cite a concrete example. There is a good deal of evidence that exposure to feared situations is frequently an important factor in the treatment of phobias, including agoraphobia. Would Hoffman suggest that any consideration of this finding is irrelevant or harmful in treating a particular patient? Note that one can be quite critical of the DSM, the PDM, or any other diagnostic system and nevertheless recognize the clinical importance of certain general findings such as the importance of exposure in helping someone with agoraphobia. One can also recognize that the psychic role played by agoraphobia in one case may be different, in subtle ways, from the

role the symptom plays in another case and nevertheless continue to also recognize the potential importance of exposure. By the way, despite recognizing the dynamic significance of the symptom, Freud (1919 [1918]) wrote that in cases of “severe” agoraphobia, «one succeeds only when one can induce them by the influence of the analysis (...) to go into the street and to struggle with their anxiety while they make the attempt» (p. 166). I do not know how frequently Freud’s advice has been followed by analysts who work with agoraphobic patients.

Although, as noted, methodologically, the case study would appear to represent a “privileged” route to understanding the individual, this is not necessarily true insofar as case studies themselves are often fraught with serious problems. Hoffman is rightly concerned that systematic empirical research may impose ill-fitting data on the individual clinical case. However, although in a somewhat different way, a similar risk exists with regard to the case study. The clinical material included in the case study can be strongly influenced by the theoretical orientation of the author. Also, as Spence (1982) has pointed out, the case study presented in the literature is a selected, normalized, smoothed out version of the actual clinical material. Indeed, on Hoffman’s constructivist position, one would expect that this would be a ubiquitous phenomenon. Also, of all people, analysts should be aware of the motivational factors that are likely to influence, sometimes unwittingly, the nature and form in which the clinical material is presented.

Over the last few months, I have been carrying out supervision with the possibility of referring to audio-video recordings made of the therapy sessions. The experience has been a revelation. It is remarkable what is omitted from the therapist’s reports and process notes, material of which the therapist is unaware and perhaps cannot be aware. It is difficult for me now to carry out ordinary supervision without thinking about what important material may be omitted. In extolling the strengths of the case study, Hoffman does not adequately address its limitations and problems. Nor does he address the ways in which some of these problems could be dealt with, for example, through the use of audio or audio-video recordings. Thus, Hoffman limits his critique to systematic empirical research and seems to suggest that case studies are somehow immune to serious critique.

Hoffman deals with the argument that systematic research should be privileged over case studies because of the latter’s subjective bias by arguing that «the ambiguity of psychoanalytic data leave them relatively unmanipulable in the sense of stacking the cards in favor of one or another point of view» (p. 1052). He writes:

«The analyst can’t force something ambiguous simply to support the view that he or she advocates. The ambiguity in itself ensures the openness of the “data” to critical review and to multiple interpretations. Such data lend themselves to constructive dialogue among the reporting analyst and others. It’s noteworthy that with all the concern about how the reporting analyst, in the interest of supporting his or her point of view, can skew both the course of the analytic work and the way in which it is described to others, in point of fact the data that are customarily presented do not seem to prevent people from mounting critiques of the work, from suggesting alternative formulations of what went on in the process, and from offering suggestions as to better ways the analyst might have intervened and participated» (p. 1052).

It seems to me that Hoffman’s comments further exposes weaknesses in the case study method and defeat his own defense of it. As Hoffman notes, the clinical data presented do not seem to prevent «alternative formulations of what went on in the process, and from offering suggestions as to better ways the analyst might have intervened and participated.» We are all too familiar with these “second-guessing” responses at case presentations, characterized by each commentator offering his or her formulation of what went on in the process, what the presenting analyst

missed, etc., each alternative suggestion reflecting the commentator's favored theoretical approach. Are these presumed properties of clinical data and the case study method supposed to represent strengths? I, and I assume others, would view them as serious problems that need to be confronted. I am reminded of Meehl's (1973) wonderful article entitled "Why I do not attend case conferences."

The Issue of Accountability

There are many difficulties with systematic empirical research on psychotherapy process and outcome and the uses to which such research is sometimes put—reflected, for example, in the rigidity and narrowness of thinking that has resulted in the classifications of "empirically validated treatments." However, some of the most cogent critiques of that kind of thinking have been provided by those who engage in systematic empirical research (e.g., Westen, Morrison Novotny & Thompson-Brenner, 2004; Beutler, 2009). A meaningful and constructive way of dealing with the shortcomings of systematic empirical research is to do better, more creative, and more ecologically valid research, not to launch wholesale attacks on any systematic research.

Undergirding debates about this or that methodology is the fundamental moral as well as scientific issue of *accountability*. Are various treatment approaches, including a psychoanalytic approach, effective in helping people and do they do what they claim to do? Fortunately, these questions have been addressed with some degree of success, in relation to different therapeutic approaches, including psychoanalytic approaches, through the carrying out of systematic empirical research. Hoffman does not acknowledge the value of such research nor does he adequately confront the question of accountability at all. Does he believe that the case study can successfully address the issue of accountability?

It is true that the issue of accountability has been exploited politically by HMO's, insurance companies, and the guardians of "empirically validated treatments" (EST). However, that that is the case does not make the question of accountability any less legitimate and fundamental. Indeed, the lack of interest in and the neglect of that question by analysts over many years provided a fertile ground for exploitation of the issue by hostile critics. Hoffman seems to suggest that an attempt to respond to demands for accountability constitutes caving into political pressures. On Hoffman's view, responsiveness to these demands is equivalent to political surrender to the powers that be—«compromising of ourselves for practical ends» (p. 1057)—and constitutes Orwellian «doublethinking our way to 'scientific' legitimacy.» This equivalence is then rhetorically buttressed by the citation of a long passage from Orwell's *1984*; Fonagy's (2002) warning that «objections to research will not win the day (...) no matter what they strength of opposing arguments» (p. 58); and Strupp's (2001, p. 615) disillusionment with the «science game»—which, by the way, is cited outside of its original context, namely, opposition to the rigidity of the empirically supported treatment movement. There is an irony that in a paper concerned with deeply moral and human issues, Hoffman seems to leave no room for the relationship of systematic empirical research to the legitimate moral issue of accountability.

One alternative to linking accountability to systematic empirical research is to locate psychoanalysis entirely in what one might call a free market economy. That is, accountability is determined entirely by, so to speak, customer satisfaction. One implication of this alternative is that it would essentially mean that psychoanalytic practice would no longer be viewed as a professional *treatment* that is embedded in institutional and societal requirements and privileges such as, for example, the ability of patients to identify their analytic experience as a tax deductible treatment expense. Another problem with this alternative concept of accountability is that, as Meehl (19&&) has pointed out, there is not a 'therapeutic' intervention in the history of

humankind, including discredited ones, that has not had its advocates, its supportive anecdotal evidence, and its satisfied customers.

(2) A focus on systematic quantitative research «threatens to embody yet a new form of prescriptive, authoritarian objectivism.» (p. 1045).

I am not entirely clear what Hoffman means by “objectivism.” He has long been concerned with contrasting “objectivism” with “constructivism,” as he does in this paper (more about that later). Along with such terms as “positivism,” “objectivism” has become a buzz word that means different things to different people. Be that as it may, I assume that Hoffman is concerned that systematic quantitative research will be employed in an authoritarian way that will dictate such matters as the nature of our training, what we will and will not be paid for, and so on. I believe, as noted earlier, that these dangers do, indeed, exist. One simply need look at the “empirically supported treatment” movement in clinical psychology. However, Hoffman does not seem to recognize that there is an equal danger of what one may call “authoritarian subjectivism,” that is, the implicit position that all that is necessary to justify or validate one’s therapeutic approach is one’s subjective feelings and convictions or one’s adherence and loyalty to a particular “school” or orientation.

Consider the advice given by Greenberg & Mitchell (1983) that when it comes to embracing a theoretical point of view, adopt whatever theory «speaks to you» (p. &&). Given the plethora of different “schools” and analytic institutes, one is likely to find one that “speaks to you,” which will reinforce the conviction that one’s subjective experience is the only or main criterion for adopting a theoretical approach. Now, it may be that choosing an approach that generates the greatest emotional resonance contributes to being a better therapist. But note that the degree to which this is so can only be determined by systematic empirical research.

(3) We should devote our efforts and energy to learning about what works and how rather than to persuading powers that be to be the value of psychoanalysis.

This is patently a false dichotomy and begs the question by clearly implying that systematic empirical research is entirely in the business of persuasion, whereas case studies are in the business of learning about what works and how. Hoffman, I assume, is aware that systematic research concerns itself not only with outcome, but also with the process of therapy.

(4) «(...) the fact that systematic quantitative studies cannot control for “who the therapist is” detracts substantially from their scientific and pragmatic value.» (p. 1049-1050).

Although as Beutler (2009) has pointed out, randomly controlled studies (RCTs) do not take into account “who the therapist is”—that is one of their deficiencies—,one can take account in sophisticated studies of therapist characteristics, patient characteristics, match between them, and for of treatment to patient characteristics. Thus, it is simply not true that systematic quantitative studies cannot control for “who the therapist is.”

(5) Psychotherapy research promotes «denial of the sociopolitical context of the phenomena being studied» (p. 1063).

Both case studies and systematic empirical research can, and often do, ignore the sociopolitical context; and both can do better in attempting to incorporate it.

General Comments

There are other issues raised by Hoffman that do not have directly to do with the question of case studies or systematic empirical research, but, either directly or indirectly, express his broad philosophical views and help one understand his position regarding research.

(1) Free-will versus determinism. On that question, Hoffman prefers that psychoanalytic ally itself with an existentialist-humanistic tradition in which “free agency” rather than psychic determinism is assumed. A theme that runs through the entire paper is Hoffman’s rejection of any position or endeavor that ignores the uniqueness of the individual and of the patient-therapist encounter. My understanding of why he brings in the free will versus determinism issue is his implicit assumption that the former upholds the individual’s uniqueness. But that, of course, is not true, certainly not in any simple way. For one thing, uniqueness can emerge from the unique combination and patterning of determinants. For another thing, free will does not guarantee individual uniqueness. What if, for example, everyone willed the same thing?

(2) Hoffman’s objections to diagnostic classification systems such as the DSM and PDM, and his seeming discomfort with the concept of “disorder”, also reflect his concern with the danger that the uniqueness of the individual will be lost in the quest for categorization. And, of course, this is a legitimate concern, but also a dilemma insofar as it is difficult, for many reasons, to do without categorization. One way to, perhaps, at least minimize reliance on diagnostic classification is to revert to Szasz’s (1961) claim that so-called mental illness is not illness, reject any psychiatrically-based diagnostic classification, and limit one’s descriptions to individual «problems in living» (p. 1049) (of course, it is always possible to categorize “problems in living”). (See also Hyman, &&). Hoffman does deal with any of the considerable practical implications, such as financial sacrifices, entailed in moving in this direction.

(3) Hoffman makes clear that his primary motivation in critiquing the status of systematic empirical research lies in «the broader divide between constructivism and objectivism in psychoanalysis» (p. 1045). Hoffman (1991, 1995, 1998, etc.) has written extensively in favor of a position he refers to as “dialectical constructivism” and the interested reader can refer to these sources. I want to limit myself here to a reiteration of the observation that the term “objectivism” is vague and unclear and serves as a buzz word for things like research, science, interest in discovering objective truths, and so on. One question, however, with regard to Hoffman’s rejection of “objectivism.” Is learning about what works and how entirely a matter of subjective opinions or does such knowledge have some objective status that, at least to some degree, transcends individual subjective attitudes?

(4) Hoffman writes that «in a nonobjectivist hermeneutic paradigm best suited to psychoanalysis, the analyst embraces the existential uncertainty that accompanies the realization that there are multiple good ways to be, in the moment and more generally in life» (p. 1043). What is the link between systematic empirical research and the realization that there are multiple good ways to be or, for that matter, between “a nonobjectivistic hermeneutic paradigm” and the realization that there are multiple good ways to be? Does a so-called objectivistic paradigm (which Hoffman leaves undefined) rule out or somehow block that realization? If so, how and in what ways? The implication that “objectivism” or perhaps systematic empirical research claim special authority in regard to such questions as «What is a good way to be in this moment?» and «What constitutes a good life?» (p. 1049) is, of course, not true and critiques of that position are critiques of a straw man.

(5) Hoffman observes that «compelling critiques of traditional approaches to psychoanalytic work *have* emerged and taken hold, as have changes in the way many analysts practice» and that these changes «owe little if anything to systematic empirical research» but rather owe more to case presentations and to clinical experience and theorizing, as well as to changes in attitudes and values in our culture» (p. 1052). One wants to say: of course, that is precisely one of the problems with relying mainly or exclusively with case studies, which are often selectively invoked to support one's theoretical approach. Hoffman seems to assume that «changes in the way many analysts practice» is necessarily “better” practice associated with better therapeutic outcome rather than, at least in part, a reflection of changing fashions. As Hoffman himself notes, these developments are partly a product of «changes in attitudes and values in our culture.» One might add that they are probably also influenced by such factors as socio-economic conditions, the availability of patients, and the plethora of therapies and therapists. In short, that changes in the way many analysts practice owe little to systematic empirical research and a great deal to case studies tells us little about the value or validity of systematic empirical research or case studies or, for that matter, of changes in the way many analysts practice. It only tells us something about the relative influence of systematic empirical research and case studies on the way many analysts practice—hardly a great surprise. As Gedo (1984) has noted (approvingly), he assumes that psychoanalytic clinicians are «impervious to change except via the route of personal clinical experimentation» and believes that changes in psychoanalytic practice are brought about by «innovative ideas powerful enough to compel acceptance by significant portions of the analytic community» (p. 514). This may, indeed, be true. That it is a desideratum or virtue is, of course, another matter.

(6) Hoffman's paper, I believe, relies excessively on a variety of rhetorical devices rather than sound argument. These devices include repeated citations of other authors who share his view—as if these citations constitute a cogent argument; citation of a long passage from Orwell's *1984* on the perils of doublethink—a kind of guilt by association tactic' an impassioned protest against the bureaucratic and inhumane practices of HMO's and insurance companies—as if these practices are somehow linked to the issue of systematic empirical research, again a form of guilt by association. Does Hoffman believe that those carrying out systematic empirical research or those who endow it with a privileged epistemological status are necessarily any less outraged at these practices than those who favor the case study method? That there is no necessary link between the systematic empirical outcome research and the practices of HMO's and insurance companies is made evident when one considers that such research can demonstrate that effective treatment may require long term therapy much beyond the prescribed number of sessions allotted by HMO's and insurance companies.

The use of the kind of rhetorical devices I refer to is also illustrated by Hoffman's discussion of his 95 year old patient who is preoccupied with the fear of death. By imagining and describing how a Utilization Review doctor might absently and cruelly demand how many sessions might be required and whether the patient is suffering from existential death anxiety or neurotic anxiety, Hoffman seems to align systematic empirical research with the cruel and heartless bad guys and individual case studies with the sensitive and humane good guys. The fact is that both systematic empirical research and case studies can be misused and abused.

(7) As noted, much of Hoffman's discussion of, for example, the dangers of systematic empirical research and of diagnostic classifications is motivated by his sensitivity to respecting, protecting, and preserving the uniqueness of the whole individual. This is, indeed, a motive we can all understand and with which we can all identify. However, I believe that one needs to

exercise some caution regarding the claim that psychotherapy and psychoanalysis deals with the “whole person” or the “total situation.” My own view is that one needs to be skeptical toward any “totalistic” pretensions. The individual comes to therapy to deal with certain aspects of his or her life and it is inevitable that other aspects will not be prominent and will not be dealt with in the treatment or will be present as implicit background factors. There is no context, including the therapeutic one, that successfully addresses the “whole person” or the “total situation.” And that is the way it should be. I am reminded of Winnicott’s (&&) comment that there is a private core in each of us that does not want to be fully understood.

Hoffman quotes from *1984* in relation to doublethink. However, another, possibly more significant, theme in *1984* is the need for certain societies to exert totalitarian control over its citizen’s lives. I would maintain that one should be wary of “totalistic” ambitions and aims, including well-intentioned ones, in relation to any institution, including the therapeutic institution. There has been a recent tendency in the psychoanalytic literature, characterized by much talk about love for the patient, to deny the bounded, limited nature of the therapeutic situation, a situation characterized by among other things, a structure of specific meeting times and monetary exchange. Yes, it is true that in much systematic empirical research, the unique individual is lost. That is a limit of such endeavors. However, there are also risks in the ambition to capture the “whole person” and the “total situation” and to deny the bounded and limited structure of the clinical situation.

(8) Hoffman seems to think that psychoanalysis has much to offer with regard to «rais[ing] social consciousness and, ultimately, constructive political action» (p. 1062). What is the basis for believing that beyond participation as ordinary citizens, analysts possess some special qualifications or privileged status as agents of social change and constructive political action?

(9) At the end of his paper, in a seeming generous and conciliatory gesture, Hoffman writes that «with respect to doublethink, for example, I am challenged to find the good in it, the ally to my own convictions» (p. 1065). It is difficult to fully discern Hoffman’s message here. I find it a condescending one for a variety of reasons. The good that Hoffman is challenged to find in the views of «our seeming adversaries» (p. 1065) is the good present in “doublethink.” Note that “doublethink” continues as the characterization of those views.

Hoffman’s generosity extends to finding «in the fine print, in the footnotes, in the unintegrated paragraphs of those who seem to champion as the royal road to “knowledge” the privileging of hypothesis-testing, quantitative research, and the denigration of case studies, in those conceptual “parapraxes” we find the subversive, disenfranchised, dissociated yet still passionate, truly psychoanalytic voice. That voice stands up for the full richness, complexity, and mystery of each moment of human experience and for its manifold unrealized potentials» (p. 1065). In other words, it is only in the parapraxes and unintegrated paragraphs of those who privilege systematic research that one finds the “truly psychoanalytic voice.”

Just as is the case in the classical situation, a journal article has a tone and a context, denotative meanings and, in G.S. Klein’s (&&) words, «a hierarchy of connotative meanings» that contribute to its “feel” and basic communications and messages. The basic message communicated, both directly and indirectly, in Hoffman’s article is an identification and association of those who favor and privilege systematic research with a host of evils and presumed evils, including “authoritarian objectivism;” lack of regard for the “whole person;” dehumanizing diagnostic classification systems; determinism; doublethink; the desiccation of experience; inhumane practices of HMO’s and insurance companies; damager to understanding the psychoanalytic process; bowing under to the powers that be; a conformist psychoanalysis, and

genocide; whereas those who favor the case study method are identified and associated with the virtues and presumed virtues of a nonobjectivist hermeneutic paradigm; constructivism; regard for the uniqueness of the individual and the “whole person;” free will; the absence of doublethink; a critical psychoanalysis; and standing up for «human freedom, for the dignity of the individual, for the meaningfulness of community, and for the sacrosanct integrity of every moment of experience» (pp. 1064-1065). What we need at this juncture in our history is not an adversarial relationship between clinicians and researchers, between those who favor and privilege systematic empirical research and those who favor and privilege case studies, but rather a cooperative and joint effort to find the legitimate and constructive uses of each methodological approach.

(10) A final comment: Although Hoffman’s paper deals primarily with systematic empirical research concerned with psychotherapy process and outcome, it is important to keep in mind that psychoanalysis is also a theory—for some, mainly a theory—concerned with such matters as the nature of mind and the nature of human nature. What form should one expect the discussion to take when one gives due recognition to those aspects of psychoanalysis?

References [incomplete list]

- Beutler L.E. (2009). &&
Fonagy P. (2002). &&
Freud S. (1919 [1918]). Lines of advance in psycho-analytic therapy. *Standard Edition*, 17: 159-168.
London: Hogarth Press, 1955.
Gedo J.E. (1984). *Psychoanalysis and Its Discontents*. New York: Guilford.
Hoffman I.Z. (1991). Discussion: towards a social-constructivistic view of the psychoanalytic situation (Discussion of papers by L. Aron, A. Modell, and J. Greenberg). *Psychoanalytic Dialogues*, I, 1: 74-105.
Hoffman I.Z. (1995). “Ritual and spontaneity in the psychoanalytic situation”. Paper read at the Annual Meeting of the *Rapaport-Klein Study Group*, Austin Riggs Center, Stockbridge, Massachusetts, June 10, 1995. Internet edition: <http://www.psychomedia.it/rapaport-klein/hoffma95.htm>.
Hoffman I.Z. (1998). *Ritual and Spontaneity in the Psychoanalytic Process: A Dialectical-Constructivist View*. Hillsdale, NJ: The Analytic Press.
Hyman && (&&). &&
Klein G.S. (&&). &&
Meehl P.E. (19&&). &&.
Meehl P.E. (1973). Why I do not attend case conferences. In: *Psychodiagnosis: Selected papers*. Minneapolis: University of Minnesota Press, pp. 225-302. Internet edition in PDF: <http://www.tc.umn.edu/~pemeehl/099CaseConferences.pdf>.
Orwell G. (1949). *1984 (Nineteen Eighty-Four)*. London: Secker and Warburg.
Spence D.P. (1982). *Narrative Truth and Historical Truth. Meaning and Interpretation in Psychoanalysis*. New York: Norton.
Strupp H.H. (2001). &&
Szasz T.S. (1961). *The Myth of Mental Illness*. New York: Harper & Row.
Westen D., Morrison Novotny K. & Thompson-Brenner H. (2004). The empirical status of empirically supported psychotherapies: assumptions, findings, and reporting in controlled clinical trials. *Psychological Bulletin*, 130: 631-663.
Winnicott D.W. (19&&). Winnicott D.W. (1960). Ego distortions in terms of true or false Self. In: *The Maturation Process and the Facilitating Environment (1957-1963)*. New York: International Universities Press, 1965