

Annual Meeting of the *Rapaport-Klein Study Group*
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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."](#)

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Discussion by Jeremy D. Safran^{*}

Irwin Hoffman has written an important, thought provoking and passionately argued paper that evokes polarized responses in readers that tend to be mediated by their prior attitudes towards research. By way of situating myself in this discussion, I should make it clear that I am both a psychoanalyst and a psychotherapy researcher. Despite the fact that I am a researcher, I have concerns about the clinical utility of much of the research that it conducted – concerns shared by many of my researcher colleagues. These concerns are not new to the field of psychotherapy research. Over the years many researchers (myself included) (Safran, Greenberg & Rice, 1988; Safran & Muran, 1994) have argued that comparative outcome research or randomized clinical trials (RCT) have serious limitations when it comes to clinical utility. Information about how the average patient responds to a particular brand of treatment is irrelevant to the practicing clinician, who needs help reflecting on how to respond with a particular patient in a particular moment. Psychotherapy researchers have also written extensively about the lack of ecological validity of RCTs, and the folly of conceptualizing technical and relational factors as independent. There has also been an emphasis on attempting to find a level of analysis that does some justice to the real complexity of the clinical situation, while still allowing for some degree of generalization (e.g., Elliot, 1994).

Many alternative research strategies have been developed over the years in an attempt to address these concerns. For example: research studying the mechanisms of change, an emphasis on studying process in context, qualitative research, approaches combing both qualitative and quantitative research, systematic and rigorous approaches to the intensive analysis of individual sessions from multiple perspectives, research based case histories, and so on. Despite these advances, many psychotherapy researchers will acknowledge that even the more innovative research approaches still have their limitations when it comes to immediate clinical utility. Moreover, notwithstanding the ongoing critiques of comparative outcome studies, there is little doubt that randomized clinical trials have retained and actually increased their privileged status within the mainstream that has come to be dominated by biologically oriented researchers and cognitive therapists. And this is certainly the case when it comes to policy makers and government funded granting agencies such as NIMH. This has serious implications for the resources that go into the construction of knowledge that is consumed by the public and for the kind of treatment that patients receive.

Should systematic empirical research be privileged over the case study method?

I want to turn now to Hoffman's argument that systematic empirical research should not be privileged over the case study method.....that they are essentially of equivalent epistemic status. The

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thrust of his position here is that those who argue that systematic research should be privileged over the case study method because its yield is limited to the “context of discovery” (in contrast to systematic empirical research which purportedly yields findings relevant to the context of “justification.”) are misguided - misguided in their belief that systematic research yields findings relevant to the context of justification. To quote Hoffman: “systematic, allegedly hypothesis-testing research is not likely to do anything more than generate possibilities for practitioners to have in mind as they work with particular patients. In other words, such research usually accomplishes nothing more in that regard than do case studies and therefore deserve no higher status as scientific contributions. To the extent they are accorded such higher status and authority, which too readily becomes *prescriptive* authority, they pose serious dangers to the quality of any psychoanalytic practice, any psychoanalytic attitude, that they affect.”

Let me state clearly at the outset that I believe that Hoffman’s concerns about the use of “science” to seek prescriptive authority are well warranted. I also agree with him that the results of systematic empirical research study are as a rule of no more immediate relevance to the practicing clinician than the case study (and in some respects less so). But by accepting the premise that hypothesis testing is the essence of the scientific method in the first place, Hoffman is accepting a problematic understanding of the nature of science. The distinction between the between the *context of discovery* vs. *the context of justification*, originally introduced by Hans Reichenback in the 1950s, is considered outdated by contemporary philosophers of science, who either argue that the distinction is not meaningful, or that privileging “justification” over “discovery” reflects an idealized reconstruction of the scientific enterprise, that has little to do with the way science really works (Godfrey-Smith, 2003). Discovery plays a central role in science. It is not something that takes place before the real work of science begins. While there is no one unified perspective in the contemporary philosophy of science, there is general agreement that the process through which science evolves is very different from the picture portrayed by logical positivists. Science has an irreducibly social, hermeneutic, and political character. Data are only one element in a rhetorical transaction. The rules and standards of scientific practice are worked out by members of a scientific community and are modified over time.

Finding a middle ground between objectivism and relativism is a central concern for many contemporary philosophers, and a new understanding of the nature of science has emerged that is informed by developments in disciplines such as sociology, anthropology, history and psychology that study the way in which science actually works (Bernstein, 1983; Feyerabend, 1975; Godfrey-Smith, 2003; Hacking, 1983; Kuhn, 1970; Latour, 1987; Laudan, 1977; Weimer, 1979). A central theme in this understanding is the importance of dialogue or conversation among members of a scientific community (Bernstein, 1983). As Gadamer (1980) suggested, the reason that this dialogue (or “genuine conversation,” as he termed it) is critical is that it provides a means of moving beyond our preconceptions. Evidence plays an important role, but this evidence is always subject to interpretation. The data do not “speak” for themselves. Scientific practice involves deliberation among members of the scientific community, interpretation of existing research, application of agreed-on criteria for making judgments, and debate about which criteria are relevant. While randomized clinical trials and other forms of psychotherapy research have various limitations, they do play meaningful roles within the context of a broader ongoing conversation that incorporates, interprets and weighs various forms of evidence.

In what follows, it is important to distinguish between the traditional psychoanalytic case study and more systematic and rigorous approaches to the intensive analysis of single cases. Since both David and Morris have already elaborated on some of these differences, I will not do so here. But bear in mind that my comments focus on Hoffman’s defense of the traditional psychoanalytic case study. Interestingly, he defends the epistemic status of the case study by arguing for the value of “constructive critical dialogue” deriving from philosophical hermeneutics. To quote Hoffman: “Such dialogue and

debate can foster transformation of theory and even the emergence of new paradigms. I think the value of constructive critical dialogue (as represented in the thought of Gadamer, Habermas, Taylor, and others) is vastly underrated by the advocates of systematic research.” p. 1051

Hoffman, however ignores developments in the contemporary philosophy of science *that recognize the hermeneutic element to science* and emphasize the ultimately progressive nature of science despite the absence of fixed criteria for arbitrating decisions about competing theories. Invoking the value of hermeneutic analysis as a critical tool, he defends the psychoanalytic case study against critics who raise concerns about the “subjective bias of the reporting analyst, ” by arguing that different readers can offer different interpretations of the clinical case material presented.

Readers can certainly offer different interpretations of the narrative the analyst presents, but this type of hermeneutic enterprise is no different in kind than the type of hermeneutic enterprise employed in literary criticism. The critic has nothing to work with but the narrative provided by the analyst- a narrative that has been constructed for illustrative and rhetorical purposes. There is no way of accessing the original data in a form less processed by the analyst (e.g., patient self-report, transcripts or videotapes of psychoanalytic sessions). If there is one thing I have learned as a psychotherapy researcher it is that therapist, patient, and third party observer perspectives on therapeutic process and outcome often disagree.

Doublethinking our way to scientific legitimacy?

The most emotionally charged theme of Hoffman’s paper involves an examination of the philosophical, ethical and political implications of privileging systematic empirical research over the psychoanalytic case study method. There are two aspects to Hoffman’s argument here, and I think it is important to disentangle them. The first aspect is a critique of the tendency for systematic empirical research to “desiccate human experience” by buying into a model of technical rationality that fails to recognize consequential uniqueness of every analytic dyad and every moment. This tendency is exacerbated by the evidence based practice model that has come to dominate our healthcare system. I am in complete agreement with Hoffman here, as I suspect that most people in the audience are.

The second aspect to Hoffman’s argument is summarized pithily in the title of his paper: “Doublethinking our way to scientific legitimacy.” It is important to read Hoffman closely here to get at a central thrust of his argument. To quote him: “*A whole genre of literature has emerged in recent years in which, via an artful version of doublethink, the privileging of controlled studies is justified alongside the articulation of rather devastating critiques of their special authority. The offering of the caveat, the reservation regarding what research can accomplish is essentially disarming. The very act of admitting limitations of controlled studies empowers the systematic research advocate and disempowers potential opponents. .*” (p. 1058)

Who is the potential opponent who is being disempowered? People like Hoffman who champion values such as human freedom and dignity. And who is doing the disempowering? “Who is the systematic research advocate in this context”? The supporter of psychoanalytically oriented research who is viewed as a collaborator with the scientific establishment that is attempting to disempower psychoanalysts who stand for values such as complexity, human freedom and the dignity of the individual. Now I can understand in part why Hoffman sees things this way. When for example, Drew Westen compares the analyst who ignores the empirical literature to the physician who treats leukemia on the basis of theories that he resonates with rather than the most up-to-date research (a rhetorical ploy that Hoffman quite rightly takes him to task for), he does begin to sound like Baker, McFall & Shoham (2009) who in a recent, highly publicized article insinuate that therapists who are not guided by the latest scientific evidence are charlatans. But I think it is important not respond to Westen’s rhetorical excess with further polarization.

The medical model from which both the randomized clinical trial and the evidence based practice movement derive has serious flaws when applied to the world of psychotherapy and to a lesser extent to the world of medicine as well. But the public and policy makers do have an understandable and legitimate desire for evidence that psychoanalysis is helpful. Does psychoanalysis work? Once again, Hoffman is on target when he highlights the role that philosophical and ethical deliberation must inevitably play in any attempt to answer questions of this type. To quote him: “Questions such as “What is a good way to be in this moment?” “Which human motives are most important?” “What constitutes the good life?” are implicitly involved. Such questions cannot and should not be adjudicated entirely by “science.” To the extent that we give the authority of science the power to arbitrate these choices we are falling into the worst kind of scientism, in which moral positions masquerade as scientific “findings.” “p. 1049

I believe that it is critical for us make the case as effectively as possible within the scientific community, to policy makers and to the general public that is absolutely essential for us to grapple with the relevant philosophical and ethical issues implicit in the way we think about and measure change. But to refuse to respond to the question of whether psychoanalysis works in terms that are meaningful to people is not only self-defeating for analysts – it also represents a type of disrespect for a public that has become increasingly frustrated with and resentful towards this type of response. I think there is a link between our reluctance to meet the public half way in an attempt to answer questions in terms that are meaningful to them, and the traditional psychoanalytic reluctance to help people solve their immediate practical problems, or focus on their symptoms.

Of course attitudes of this type stem in part from a deep respect and appreciation for the complexity and ambiguity of life. I believe, however, that there is also a subtle level at which a traditional and deeply entrenched psychoanalytic habit of arrogance and elitism seeps into Hoffman’s argument. This is particularly ironic given his longstanding commitment to challenging such attitudes. I am also concerned that one potential impact of Hoffman’s article is to justify a type of complacency among analysts – a sense that he has successfully fended off the assaults of those who demand “hard evidence” and that we can all return to business as usual. Hoffman argues that by deprivileging the role of science in this conversation he is not arguing for radical relativism. As I discussed earlier, he argues for the role of “constructive critical dialogue” as a third way between the alternatives of radical relativism and systematic research. But in perpetuating an outmoded view of science and hermeneutics as *either/or alternatives*, Hoffman maintains an unfortunate polarization. In doing so he underestimates the potential value of systematic empirical research and ignores or gives short shrift to the limitations of traditional psychoanalytic discourse.

Conclusion

I do believe that Hoffman is in important respects “fighting the good fight,” but think he is fighting it with the wrong opponents. I agree with him that there is a vitally important battle that needs to be fought. But this battle needs to be fought within the scientific community, in the eyes of the funding agencies and policy makers and in the court of public appeal. Reassuring ourselves that we have been right all along will not in the final analysis be helpful. The tendency towards insularity among psychoanalysts, has not served us well in the past, and it certainly will not serve us well in the future.

The advocates of systematic empirical research who Hoffman is attacking in his paper, are potentially important allies in a vitally important fight, who tend to be disenfranchised by mainstream academia and funding agencies because of their belief in the value of psychoanalysis, and by many psychoanalysts, because they are researchers, who in many cases not formally trained analysts. It is also important for us to recognize that many nonpsychoanalytically oriented researchers who are strong critics of the scientific aspects of research and the evidence based treatment movement are potential

allies as well (see example, Norcross, Beutler & Levant's, 2006 edited collection of dialogues about philosophical and methodological issues relevant to the implementation of the evidence based practice model in the realm of mental health).

I certainly view myself as an ally of Hoffman's when he argues for "a critical rather than a conformist psychoanalysis....as a bastion in our culture that will stand for human freedom, for the dignity of the individual, for the meaningfulness of community, and for the sacrosanct integrity of every moment of experience." I have already made it clear that I share many of Hoffman's concerns about the limitations of systematic empirical research. *And I believe that we need to take his concerns about those who would use the mantle of science to seek prescriptive authority extremely seriously.* Because there has indeed been a massive and in many ways successful effort by biologically oriented researchers and cognitive therapists to use the mantle of science to seek prescriptive authority.

I have spent many hours defending aspects of Hoffman's position among psychoanalytic researchers who tend to respond to his paper dismissively without fully grappling with his important points, just as I have spent time debating these issues with Irwin. My concern, however, is that Irwin's paper is in part a call to the besieged to close ranks and circle the wagons. And I just don't think that circling the wagons in this way is healthy for the future of psychoanalysis or the future of our culture at large. At this important historical juncture we need to be engaging in critical and constructive dialogue with the larger community rather than turning inwards.