Doublethinking or Dialectical Thinking: A Critical Appreciation of Hoffman's “Doublethinking” Critique

Jeremy D. Safran Ph.D. a
a New School for Social Research and New York University
Postdoctoral Program in Psychotherapy & Psychoanalysis
Published online: 04 Dec 2012.


To link to this article: http://dx.doi.org/10.1080/10481885.2012.733655

PLEASE SCROLL DOWN FOR ARTICLE

Taylor & Francis makes every effort to ensure the accuracy of all the information (the “Content”) contained in the publications on our platform. However, Taylor & Francis, our agents, and our licensors make no representations or warranties whatsoever as to the accuracy, completeness, or suitability for any purpose of the Content. Any opinions and views expressed in this publication are the opinions and views of the authors, and are not the views of or endorsed by Taylor & Francis. The accuracy of the Content should not be relied upon and should be independently verified with primary sources of information. Taylor and Francis shall not be liable for any losses, actions, claims, proceedings, demands, costs, expenses, damages, and other liabilities whatsoever or howsoever caused arising directly or indirectly in connection with, in relation to or arising out of the use of the Content.

This article may be used for research, teaching, and private study purposes. Any substantial or systematic reproduction, redistribution, reselling, loan, sub-licensing, systematic supply, or distribution in any form to anyone is expressly forbidden. Terms & Conditions of access and use can be found at http://www.tandfonline.com/page/terms-and-conditions
Doublethinking or Dialectical Thinking: A Critical Appreciation of Hoffman’s “Doublethinking” Critique

Jeremy D. Safran, Ph.D.

New School for Social Research and New York University Postdoctoral Program in Psychotherapy & Psychoanalysis

Irwin Z. Hoffman’s (2009) “Doublethinking our way to ‘scientific’ legitimacy” is an important and thought-provoking paper that tends to evoke passionate and polarized responses. Important threads running throughout his paper include the pitting of objectivist against constructivist perspectives and a concern that the objectivist epistemology underlying most systematic empirical research endangers important psychoanalytic values. In this paper I underscore and elaborate on the importance of certain aspects of Hoffman’s paper, while at the same time arguing for a less polarized perspective, by appealing to contemporary developments in the philosophy of science that seek a middle ground between objectivism and constructivism. This middle ground recognizes that science has an irreducibly social, hermeneutic, and political character, and that data are only one element in an ongoing conversation between members of a scientific community. I also argue that the rules and standards of practice are worked out and modified over time by the scientific community, and that it is critical for psychoanalysts to be members of this larger community.

Irwin Z. Hoffman’s (2009) “Doublethinking our way to ‘scientific’ legitimacy” is an important, thought-provoking and passionately argued paper evoking 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 psychotherapy researcher and a psychoanalyst. I started my career as a psychotherapy researcher before I began my analytic training and still maintain an active involvement in the world of psychotherapy research in addition to my analytic practice. Nevertheless, I have real concerns about the clinical utility of much of the research that is conducted, and I am sympathetic to many of the concerns that Hoffman raises. There are, however, important areas of disagreement between us as well. In an effort to move this conversation in a less polarized direction, I outline areas of agreement and disagreement in our perspectives.

To briefly summarize Hoffman’s paper, I distill three major themes:

1. The first is a critique of the value or relevance of systematic empirical research on psychotherapy process and outcome. Hoffman also raises concerns about other forms of research and research enterprises (e.g., neuroscience research, the Psychodynamic Diagnostic Manual, and the Shelder-Westen SWAP Q-sort [a measure for assessing...
DOUBLETHINKING OR DIALECTICAL THINKING

change in therapy]. However, since the majority of his critique focuses on psychotherapy research and in particular randomized clinical trials, I restrict my discussion to this area.

2. The second and related argument involves a defense of the epistemic status of the traditional psychoanalytic case study method and a challenging of the claim that “systematic empirical research” should be privileged over the traditional psychoanalytic case study.

3. The third involves an examination of the philosophical, ethical, and political implications of privileging systematic empirical research over the psychoanalytic case study method. Related to this is a critique of psychoanalytically oriented researchers who “play the game of science” (a phrase that Hoffman borrows from a paper of Hans Strupp’s) in an attempt to establish the legitimacy of psychoanalysis. I briefly address each of these themes in turn.

RESEARCH ON PSYCHOANALYTIC/PSYCHOTHERAPY PROCESS AND OUTCOME IS OF LIMITED UTILITY TO THE CLINICIAN

I have no problem with this assertion. In fact, this has been documented in a number of surveys beginning with Morrow-Bradley and Elliott’s survey of members of Division 29 published in a 1986 issue of *American Psychologist*, where they found that in general clinicians found research to be considerably less relevant to their clinical practice than a variety of different other influences such as clinical/theoretical papers, clinical supervision, and the experience of being a client. Reasons endorsed for the lack of relevance of psychotherapy research included such factors as the tendency for research to treat therapists and patients as interchangeable units, the failure of research to do justice to the complexity of the clinical process, a lack of sufficient attention by researchers to clinically meaningful questions, and a tendency for patients in research studies to be very different then those seen in everyday clinical practice (e.g., many clinical trials screen out patients with comorbid diagnoses).

It is interesting for me to speculate about what kind of reaction I would have had to Hoffman’s paper if I had read it in the early 1980s when I first started as a psychotherapy researcher. I think I would have agreed with many of his critiques of empirical research. This would have been especially true of his critique of the “gold standard” of psychotherapy research—the randomized clinical trial. These critiques were in the air at the time, at least within the psychotherapy research community where I began my career and continue to participate in most actively. I’m referring to the Society for Psychotherapy Research. Hans Strupp and David Orlinsky, the two researchers who Hoffman portrays as “virtual defectors” (Hoffman, 2009, p. 1063) from the psychotherapy research camp, were active members of the Society for Psychotherapy Research community at the time. David Orlinsky still is, and Hans Strupp was until the day he died. Many of us at the time spoke and wrote extensively about the limitations of randomized clinical trials, and argued for the importance of intensive analysis of single cases. In addition we spoke about the problems associated with conceptualizing therapists as interchangeable units, and talked about the inseparability of therapist, technical and relational factors. We also wrote about the limitations of aggregate research or comparative treatment research (as opposed to the intensive analysis of single cases). As many psychotherapy researchers including myself have argued, 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 and context (Rice & Greenberg, 1984; Safran, Greenberg, & Rice, 1988; Safran & Muran, 1994).
Psychotherapy researchers have also written extensively about the lack of ecological validity of randomized clinical trials and the folly of conceptualizing technical and relational factors as independent. There has also been an emphasis on striving for a level of analysis that attempts to do justice to the real complexity of the clinical situation while still allowing for some degree of generalization (e.g., Elliott & Anderson, 1994).

Many alternative forms of research emerged out of those conversations: different forms of qualitative research, research on the mechanisms of change, the investigation of specific “events” of interest in therapy, the study of process in context, and various approaches to single case studies that are more rigorous than traditional psychoanalytic case studies. The clinical utility of research has improved in ways not recognized by Hoffman. 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 true among funding agencies and policymakers who both reflect and influence the direction of the field.

As someone who sits on National Institute of Mental Health grant proposal review panels, I can assure you that it is becoming increasingly difficult to obtain funding for any psychotherapy research (as opposed to basic brain science research). The review panels are dominated by biologically oriented researchers, and the majority of other reviewers are cognitive therapists. Any proposal remotely suggesting a connection to psychoanalysis is highly unlikely to receive a fair hearing, since everyone “knows” that psychoanalysis doesn’t work. This shift in political climate has serious implications for the construction of knowledge that is consumed by the public and for the kind of treatment that patients receive—and I am sympathetic to the urgency of Hoffman’s tone, because I too believe that there is a vitally important battle—a theme I return to later.

**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 thrust of his position here is that those such as Westen (2002) 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. (p. 1046).
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). I think he weakens his argument, however, by asserting that the yields of both systematic empirical research and the case study method are limited to the context of discovery. It is indisputable that systematic empirical research tests hypotheses, while the traditional case method does not. Nevertheless, as Paul Meehl (1978) argued many years ago, the problem is that because mainstream psychology has traditionally seen hypothesis testing as the sine qua non of science, researchers in psychology have a tendency to test hypotheses before they have hypotheses worth testing (Safran et al., 1988).

The distinction between the contexts of discovery versus justification, originally introduced by Hans Reichenback in the 1930s, is considered outdated by contemporary philosophers of science, who argue either 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. Rigorous and systematic observation of clinical cases should play a central role in psychotherapy research, and it is clear that the reward structures set in place by mainstream journals and granting agencies discourage this.

The case study method is an unparalleled tool for observing and discovering clinical phenomena, and there are also ways of using the case study method for testing hypotheses (e.g., Greenberg, 1986; McCullough & Carr, 1987). It is critical, however, to distinguish between the traditional psychoanalytic case study method that Hoffman defends and the kind of rigorous, systematic case study methodology advocated by people such as Daniel Fishman (who Hoffman cites as an ally) or for that matter Hans Strupp, who advocated for the use of what he termed research informed case studies. Fishman (1999) and others (e.g., Stiles, 1993, 2006) propose rigorous guidelines to guide “quality control” monitoring of qualitative data and for establishing the equivalent of psychometric reliability of case reports. Fishman also proposes guidelines for specifying contextual information sufficiently well, so that multiple case studies can be assembled as part of a database that can facilitate generalizability through inductive logic (as opposed to the type of deductive logic that allows for generalization on the basis of a group comparison study). Strupp (2001) proposes ways of combining narrative-based case analysis with quantitative measures that permit assessment of both process and outcome from the perspective of therapist, patient, and third party observer.

Hoffman defends the epistemic status of the traditional psychoanalytic case study by arguing for the virtue of “constructive critical dialogue” deriving from philosophical hermeneutics. To quote him:

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. (Hoffman, 2009, p. 1051)

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, such as patient self-report or 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. Lest I be misunderstood here, I am not arguing that the data can ever be accessed in any “pure form” since data are always shaped by observation (Hanson, 1958). Nor am I arguing that patient self-report or observer-based perspectives should be privileged over the analyst’s perspective. What I am arguing is that there is no reasonable defense for not taking into account all three perspectives.

I am in agreement with Hoffman about the value of the type of hermeneutic and dialogical enterprise he advocates. As I discuss below, however, I believe that it is critical for us to incorporate an understanding of the value of philosophical hermeneutics (e.g., Gadamer, 1960/1975) into a broader understanding of the way in which science actually works. Hoffman frames his argument in terms of the opposition between what he refers to as “constructivism” versus “objectivism.” To quote him, for example,

My critique of the premises for the privileging of systematic research and of neuroscience is not accurately lined up with the divide between the psychoanalytic researcher and psychoanalytic practitioner; it is lined up with the broader divide between constructivism and objectivism in psychoanalysis, a divide that can be located within the community of non-research-oriented psychoanalytic clinicians. (Hoffman, 2009, p. 3)

Hoffman advocates for what he has termed dialectical constructivism (a position that he has argued for eloquently for many years) as a middle ground between objectivism and a radical relativism. But I think that Hoffman’s failure to apply his trademark style of dialectical thinking to an analysis of the process of science leads him to a position that is less of a middle ground than he believes.

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 commonly accepted view of things. Science has an irreducibly social, hermeneutic, and political character. Data are only one element in a rhetorical transaction (yes, rhetoric plays a central role in science as well). The rules and standards of scientific practice are worked out by members of a scientific community and are modified over time (Safran, 2001; Safron & Aron, 2001). Finding a middle ground between objectivism and relativism is a central concern for many contemporary philosophers, and an understanding of the nature of science has emerged over the last thirty to forty years 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, 1996; Latour, 1987; Laudan, 1996; Shapin, 2010; Weimer, 1979). A central theme in this understanding is the importance of dialogue or conversation among members of a scientific community (Bernstein,
As Gadamer (1960/1975) 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.

Hoffman ignores developments in the contemporary philosophy of science that recognize the hermeneutic element to science and articulate a middle ground between constructivism and objectivism that acknowledges the absence of fixed criteria for arbitrating disagreements between competing theories. I believe that this leads him to underestimate the potential value of systematic empirical research and to give short shrift to the limitations of the traditional psychoanalytic case study method. Data emerging from systematic empirical research can be manipulated in various ways. But they really are more difficult to manipulate than the “data” of the psychoanalytic case study, and the critic does have the ability to access the original data in a less processed form. In some cases the data can actually include videotapes of the relevant therapy sessions that can then be observed and recoded in various ways. These data then become elements in an ongoing conversation in which other researchers can challenge the way in which it is interpreted, reanalyze it in different ways, or challenge or raise questions about what the most meaningful criteria are for making decisions.

For example, much to the chagrin of cognitive therapy proponents, the highly publicized National Institute of Mental Health Treatment of Depression Collaborative Research Program (Elkin, 1994; Shea, Pilkonis, Beckham, & Collins, 1990) found that at termination patients receiving cognitive therapy did not show significantly more improvement than patients in the placebo condition. Cognitive therapy proponents can and have found many ways of challenging the interpretation of these data. They have reanalyzed it; they have argued that the cognitive therapists in the study were not performing competently, and so on. But there are more constraints on the extent to which the data can be manipulated in this context than there are in the context of the traditional psychoanalytic case study.

Jonathan Shedler’s (2010) recent American Psychologist article reviewing the various meta-analyses supporting the efficacy of psychodynamically oriented treatments has been criticized by cognitive therapists who argue that he selectively included some studies and not others, that some of the studies included were not methodologically rigorous, or used inappropriate statistics, and so on (e.g., Beck & Bahr, 2009; Bhar & Beck, 2009; Bhar et al., 2010). In response psychoanalytic researchers are beginning to publish rejoinders pointing out that the studies included in meta-analyses supporting the efficacy of cognitive-behavioral therapy suffer from precisely the same type of methodological weaknesses (e.g., Leichsenring & Rabung, 2009). More recently a number of critiques of Shelder’s (2010) review have been published along with Shedler’s response to them in American Psychologist (2011, Vol. 2, No. 55). In this type of back and forth it is common for critics to reanalyze the original data using different procedures, or to conduct a revised meta-analysis that uses different criteria for deciding which studies will be incorporated. One can view this type of exchange cynically as nothing but politics and rhetoric “dressed up” as “science.” But as previously argued the scientific enterprise is intrinsically rhetorical and political in nature.
Now even if there are more constraints on the manipulation of the data emerging from systematic empirical research, this doesn’t resolve the concern that a randomized clinical trial doesn’t provide the kind of contextually sensitive information immediately relevant to the clinician. So does this mean that randomized clinical trials are a meaningless waste of time? Despite their limitations I don’t think so. First, there are important questions that are hard to meaningfully address without randomized clinical trials. For example, there is now a consistent body of evidence indicating that depressed patients who are treated with short-term cognitive therapy have substantially lower relapse rates than those who are treated with medication (e.g., Hollon et al., 2005). Some might argue that they have known this all along. But there is a difference between believing in the value of psychotherapy or of having some experiences consistent with this finding versus the type of evidence that is amassed through randomized clinical trials. Second, sometimes unexpected results emerge that raise intriguing questions worth exploring. For example, in the Vanderbilt II study, Hans Strupp and colleagues (Henry, Schacht, Strupp, Butler, & Binder, 1993) found that the therapeutic skills of a group of experienced therapists who underwent a one-year training program in short-term dynamic therapy (incorporating the use of transference interpretations) actually deteriorated. While the results of the study were disappointing to Strupp and his colleagues, the process of making sense of these findings led to a number of interesting discoveries and new avenues for research.

Moreover, sometimes findings that run counter to our cherished beliefs can spur us to ask important questions, rather than just seeking ways of explaining the findings away. For example, one of the stronger findings in psychotherapy research is that some therapists are consistently more (or less) helpful than other therapists, and that this individual therapist variable has a considerably larger impact on treatment outcome than the particular brand of therapy (e.g., Lutz, Leon, Martinovich, Lyon, & Stiles, 2007). Tim Anderson, a postdoctoral student of Strupp’s at the time, collaborated with him to identify the therapist in the Vanderbilt II study who had the most consistently positive outcomes across cases. They then observed videotapes of many of his sessions in an effort to develop some understanding of what accounted for his success (Anderson & Strupp, 2007). They also had other expert therapists examine the videotapes of those sessions. Interestingly, this particular therapist, while exhibiting some qualities we normally associate with the “good therapist” (e.g., warmth, spontaneity, playfulness, emotional liveliness and immediacy), in other respects did not fit the stereotype. For example, he was unpredictable, emotionally provocative, and at times defensive. Moreover, he seemed to have difficulty reflecting on his countertransference and had a marked tendency towards sexual provocativeness. How do we make sense of such findings? They may be an artifact of a methodological problem, or they may point to a phenomenon worth investigating further. Either way, there is something about the counterintuitive nature of the findings that can lead to further inquiry.

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 the 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 many readers of this journal 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 double-think, 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 are the potential opponents who are 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 (2002) 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, and Shoham (2009), who in a recent, highly publicized article insinuated 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 policymakers 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). In an earlier version of this paper I wrote that “nobody is arguing that philosophical and ethical positions can be arbitrated by science.” But to be fair to Hoffman, his argument is that philosophical and ethical positions are implicit in the way we conceptualize and examine change. And to the extent that researchers, policymakers, and funding agencies bypass this type of deliberation they are perpetuating certain values about the nature of the good life (cf. Safran, 2012).

I thus believe that it is critical for us make the case as effectively as possible within the scientific community, to policymakers 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 halfway 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. Many people do not seek treatment in order to deal with deeper existential concerns. They come to us because they are in pain and they want help. And they quite understandably want some reassurance that we can provide them with the help they want.

An unwillingness to engage with them at this level stems 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 long-standing 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.

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 policymakers 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 scientistic aspects of research and the evidence-based treatment movement are potential allies as well (see, e.g., 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 (pp. 1064–1065).” 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 time 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 Hoffman. My concern, however, is that
Hoffman’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 (Safran, 2012).

REFERENCES


Shapin, S. (2010). *Never pure: Historical studies of science as if it was produced by people with bodies, situated in time and space*. Baltimore, MD: John Hopkins University Press.


**CONTRIBUTOR**

Jeremy D. Safran, Ph.D., is Professor of Psychology at the New School for Social Research, and former Director of Clinical Psychology. He is also Senior Research Scientist in Psychiatry at Beth Israel Medical Center, and on faculty at the NYU Postdoctoral Program in Psychotherapy & Psychoanalysis, and the Stephen Mitchell Center for Relational Studies. Dr. Safran is Past-President of the International Association for Relational Psychoanalysis & Psychotherapy, and co-founder & co-chair (with Lewis Aron & Adrienne Harris) of the Sandor Ferenczi Center at the New School. In addition he is Associate Editor for *Psychoanalytic Dialogues*, and on the editorial boards of a number of other journals including *Psychoanalytic Psychology* and *Psychotherapy Research*. Dr. Safran has authored several books including *Emotion in Psychotherapy* (with Leslie S. Greenberg), *Negotiating the therapeutic alliance: A relational treatment guide* (with J. Christopher Muran), and most recently *Psychoanalysis and psychoanalytic therapies* (published by the American Psychological Association in 2012).