



ISSN 2472 2472

On the Scientific Legitimacy of Psychoanalysis: The Controversy that Won't Go Away

David Lotto, PhD¹

This paper briefly examines the history and present status of the question: Is Psychoanalysis a Science? and the subsidiary question: Is Psychoanalysis an effective treatment method whose, either theory or practice, is "evidence based"? The long history of the controversy between two opposing ways of answering these questions is briefly summarized and described as a specific case of the opposing views regarding the famous "mind-body problem" which has been vexing philosophers and psychologists since at least the time of Descartes. There is also a discussion of the relative merits of the case study method as opposed to empirical research on process and outcomes of the clinical encounter for advancing psychoanalytic knowledge and legitimacy in the eyes of the psychotherapy profession, and the public.

Cet article examine brièvement l'histoire et l'état présent de la question : est-ce que la psychanalyse est une science ? ainsi que la question subsidiaire : est-ce que la psychanalyse est un traitement efficace dont la théorie ou la pratique est basée sur la preuve ? La longue histoire de controverse entre deux modes opposées de répondre ces questions est sommairement résumée et décrite en tant qu'un cas spécifique de perspectives opposées concernant le fameux problème « corps-esprit » qui a vexé les philosophes et psychologues depuis au moins le temps de Descartes. Il y a aussi une discussion des mérites de la méthode de l'étude de cas comparée à la recherche empirique en ce qui concerne le procès et le résultat de la rencontre clinique afin de hausser la connaissance et la légitimité psychanalytique dans l'opinion de la profession psychothérapeutique et celle du public. (Translation: Alireza Taheri)

¹ Editor, The Journal of Psychohistory

In January 2007, Irwin Hoffman gave the plenary address at the winter meetings of the American Psychoanalytic Association. The paper was later published in 2009 in JAPA under the title of *Double thinking Our Way to Scientific Legitimacy: the Desiccation of Human Experience*. This paper is a critique of that paper. It also examines one aspect of Hoffman's paper which is the question of what value the case study has in psychoanalysis for determining or validating either the theory of human behavior or the treatment process as compared to the validity of empirical clinical research in accomplishing the same goal. This is followed by a brief review of the substantial literature on the epistemological status of Psychoanalytic theory, and a discussion of whether there might be a way of reconciling the opposing stances psychoanalysts and philosophers of science have taken on the issue of the scientific legitimacy of Psychoanalysis.

The position that Hoffman presents in his paper is that psychoanalysis is best understood in a "non-objectivist hermeneutic paradigm". Hoffman identifies himself as part of a movement that has been in existence for the last twenty-five years which has been battling "objectivism" in psychoanalysis. He refers to this position as Hermeneutic/Constructivist.

The epistemological issues, questions about the scientific legitimacy of Psychoanalysis, that Hoffman and the many advocates of the Hermeneutic/Constructivist perspective as well as those who hold "objectivist" views are addressing have been a source of controversy within psychoanalysis from the beginning, going back to Freud's thoughts on these matters.

The essential question has most often been framed as: Is psychoanalysis a science, and if it is, what kind of science is it?

Back to Freud

The question of to what extent psychoanalysis is or should be a purely psychological theory versus a theory that is integrated with neurophysiology and evolutionary biology was a central concern in Freud's theoretical work throughout his lifetime.

It is sometimes said that Freud permanently gave up the attempt at working toward a neurophysiological basis for psychoanalysis and proceeded with the development of his theory solely on psychological grounds. For example, in *The Unconscious*, He says:

"every attempt . . . to discover a localization of mental process, every attempt to think of ideas as stored up in nerve cells and of excitations as traveling along nerve fibers, has miscarried completely." (Freud, 1915/1975, p. 174).

In the *Introductory Lectures on Psychoanalysis* he writes:

". . . psychoanalysis must keep itself free from any hypothesis that is alien to it whether of an anatomical, chemical or physiological kind, and must operate entirely with purely psychological auxiliary ideas. . ." (Freud, 1916/1975, p. 21)

Yet Freud never relinquished his desire to connect his psychological theory with brain physiology. In *On Narcissism: An Introduction* he writes: ". . . all our provisional ideas in psychology will presumably someday be based on an organic substructure." (Freud, 1914/1975, p. 78).

Freud's desire to provide a rational, coherent and broadly-based systematic understanding of his clinical discoveries and insights remained a lifelong goal. This level of theorizing, the metapsychology or the general psychology theory, remained a constant center of interest. In volume 14 of the *Standard Edition*, Strachey (1957/1975) includes an appendix titled: *List of Writings by Freud, Dealing Mainly with General Psychological Theory*. This list has twenty-six entries spanning the period from 1895 to 1938. The longest gap between adjacent listings is six years.

Freud was always concerned that psychoanalysis would be seen as unscientific. He took great pains to argue that the changes brought about by psychoanalysis were not due to “suggestion”, ever since Wilhelm Fliess’ accusation, at the time their friendship was turning to enmity, that “the reader of thoughts merely reads his own thoughts into other people.” (Masson, 1985, p. 447) In his Introductory Lectures on Psychoanalysis, in the course of a twenty page defense against the charge that psychoanalytic therapeutic gains are due solely to the effects of suggestion, he says:

“Conflicts will only be successfully solved and his resistances overcome if the anticipatory ideas he is given tally with what is real in him.” (Freud, 1916/1975, p. 452)

This is what Grunbaum (1984) in his epistemological critique of psychoanalysis has labeled the “tally argument”. Grunbaum (1984) and Eagle (1984) see the tally argument as central to demonstrating the validity of psychoanalysis.

For the most part Freud insisted that the archaeological metaphor - uncovering what was buried or hidden - was central to both theory and technique. He remained firm in the belief that psychoanalysis could create a factual explanatory narrative of an individual's life history, that is, a reconstruction rather than a construction. Freud viewed psychoanalytic treatment as a process of uncovering something that is really there and fitting what is discovered into a coherent explanation. Something quite different than the creation of a meaningful or therapeutically effective narrative; using Donald Spence’s terms, historical not narrative truth.

However, in 1937 when he was eighty-one he wrote *Constructions in Analysis*. Although the main theme of the paper is to reiterate the archeological uncovering and reconstructing model Freud does lean a bit in the other direction, at least with regard to therapeutic efficacy. He says:

"The path that starts from the analyst's construction ought to end in the patient's recollection: but it does not always lead so far. Quite often we do not succeed in bringing the patient to recollect what has been repressed. Instead of that, if the analysis is carried out correctly, we produce in him an assured conviction of the truth of the construction which achieves the same therapeutic result as recaptured memory." (Freud, 1937/1975, pp. 265-266)

From the Hermeneutic/Constructivist point of view both the tally argument and the archaeological metaphor are prime examples of what might be called “objectivism”, the belief that there is something which is “real” (in the sense that Freud uses the term in the archeological model and in the tally argument) to which an analytical finding could be compared or by which it could be evaluated. If one takes the Hermeneutic/Constructivist position that there is no such thing as an objective “real” then there is neither anything to uncover nor anything to tally with. It's quite a tempting solution to the epistemological difficulties that Freud and so many others including Rapaport, Klein, Holt, Gill, Schafer, Spence, Eagle, and Wolitzky, as well as Grunbaum have struggled with; namely, how does one demonstrate or validate or evaluate one’s clinical or theoretical claims. We will return to this later in the paper.

The History

In the 1940s and 1950s, one path this debate took was focused on the question of whether psychoanalysis was a nomothetic science - one that used traditional natural science methods or an ideographic science - one that studies individuals and the particular. Here is David Rapaport's (1944) position on these matters:

“The main point is that where psychology stands in this clash of methods is quite a question. Why? Because it shares with the ideographic sciences two different basic facts: the historical-clinical method, and the fact that the phenomena dealt with are unique, singular, and once occur. There is, however, sufficient justification for attempting to build a nomothetic (i. e., natural) science of psychology . . . Psychoanalysis is a natural science; it is meant to be the natural science of the psyche”. (pp. 185-186).

Or, as Benjamin Rubenstein said in 1976:

"In clinical practice we see man unequivocally as a person. However, to justify our hypotheses about unconscious mental events, we must turn our attention to the organism this person is also. Psychoanalysis as a science must thus combine the ways of looking at man, say, of history and literature with those of neurophysiology and other biological sciences. That is the challenge our discipline faces us with, but also its fascination." (Rubenstein, pp. 262-263).

Starting in the mid-1960s there was considerable criticism of the metapsychology from within the psychoanalytic community. This controversy culminated with a panel discussion in 1976 published by Psychological Issues under the title Psychology Versus Metapsychology. George Klein (1975), Roy Schafer (1975, 1976), Merton Gill (1976) along with Benjamin Rubenstein (1976), Robert Wallerstein (1976), and Donald Spence (1982) were all concerned with this clinical theory versus metapsychology controversy.

A good place to start to look at this controversy would be with the philosopher and analytic scholar Lloyd Home's 1966 paper *The Concept of Mind* in which he argues:

"... that there is a fundamental distinction between the humanistic method, which centers on the act of interpretation, and the scientific method, which involves the process of explanation. Science ask "how" questions which must be answered in terms of causal mechanisms while humanistic discipline asks "why" questions which are answered in terms of reasons and meanings." (Lotto, 1982, p.464)

Home's position has its roots in the work of German philosopher Wilhelm Dilthey who emphasized the distinction between the natural sciences (Naturwissenschaften) and the human sciences (Geisteswissenschaften). It is also quite similar to the ideographic versus the nomothetic distinction. Home argues that the proper domain of psychoanalysis is the study of meaning and that psychoanalysis should be a humanistic rather than a scientific enterprise.

George Klein, in his posthumously published book, *Psychoanalytic Theory: An Exploration of Essentials* (1975) takes the position that traditional psychoanalytic theory can be characterized as a hopelessly intertwined conglomeration of two very different kinds of theory: clinical and metapsychological. He argues that the metapsychology is unnecessary, irrelevant, and totally expendable. All we need is a clinical theory that can stand by itself without needing a mechanistic metatheory (the metapsychology) to endow it with scientific legitimacy.

Merton Gill's (1976) *Psychological Issues* monograph *Metapsychology is Not Psychology* takes a similar position. He argues that metapsychological propositions which are in the natural science framework of force, energy, and structure must be separated from psychological propositions which deal with intention and meaning. In agreement with Klein, he feels that the proper task for psychoanalysis is to construct a purely psychological theory.

Home, Klein, Gill, and Schaffer all share with Hoffman, at least to some extent, sympathy for constructivist and hermeneutic ways of understanding psychoanalysis.

Like Hoffman they see psychoanalysis and particularly psychoanalytic treatment as an enterprise that is primarily about meaning - a meaning that is either constructed by the subject or co-constructed with the analyst. They also share a highly critical view of traditional metapsychology, the drive theory, and the use of concepts that are overly abstract or are too far removed from the realities of the clinical encounter.

The language has changed somewhat since the mid-1970s but the split between the two camps within psychoanalysis is fundamentally the same. Those who look at it as more of an activity of interpreting and understanding meanings closer to the humanities than to the hard sciences; and those who prefer to see psychoanalysis as a science that can establish demonstrable truths about our nature and motives, as well as how people change over the course of an analysis.

Donald Spence in his 1982 book, *Narrative Truth and Historical Truth* wrestles with the issue of what it is we discover about a person in the course of a psychoanalytic treatment. He addresses some of the problems with maintaining that we are able to come to the "historical truth" that Freud was seeking.

He argues that one of the main problems in seeking "historical truth" is the potential for distortion whenever we rely on memory, either the patient's or the analyst's. After summarizing some of what we have learned about the nature of memory, particularly the work of Elizabeth Loftus (1980). Spence says:

". . . we have little reason to believe that the memories that emerge can be trusted very far. More than we realize, the past is continuously being reconstructed in the analytic process, influenced by (a) the repressed contents of consciousness; (b) subsequent happenings that are similar in form or content; (c) the words and phrases used by the analyst in listening and commenting on the early memories as they emerge; and (d) the language choices made by the patient as he tries to put his experience into words. The past, always in flux, is always being created anew." (Spence, 1982, p. 93)

Memory processes are not all that is subject to distortion. All through the analytic process, whether the patient or the analyst is speaking or listening, there are opportunities for multiple forms of distortion. As Spence says: "At every step of the way, we project our own meanings onto the clinical material." (p.30).

The Case Study

The question to be addressed here is what value the case study has in determining or advancing our knowledge about either human behavior and motives or what occurs in treatment. There is also the related question of the value of the case study in validating or providing evidence in support of psychoanalytic knowledge. I am referring here, primarily, to the traditional written case study presentation rather than empirical research studies of clinical treatment.

First, a brief and selective overview of the psychoanalytic literature on these issues. For most of this literature, which is not written from a Hermeneutic/Constructivist point of view, the central issue is whether case study material can avoid being significantly distorted by a host of factors.

Again we start with Freud. In *Studies on Hysteria*, at the beginning of his analytic career in 1895, in his account of the treatment of Elisabeth von R. he says:

". . . it still strikes me myself as strange that the case histories I write should read like short stories and that, as one might say, they lack the serious stamp of science." (Freud, 1895/1975, pp. 160-161)

As mentioned above, the scientific status of psychoanalysis was always important to Freud. In Freud's time the case study report was the sole method for demonstrating the "truth" of psychoanalytic propositions so he was understandably concerned that case study material be seen as factual rather than fictional.

Robert Michels' 2000 paper, *The Case Study*, focuses on a particular source of bias or distortion in case presentations, either written or oral. His contention is that case reports are always written for one or more purposes. Michels speaks mostly of conscious purposes. He points out that most case history or clinical vignette presentations are made in the context of making some sort of argument. Michels is quite harsh in judging the evidential value of case history reports saying: "I suggest that case reports that are offered as 'pure' scientific data are illustrations of implicit, concealed, or disavowed intentions or purposes ... " (p. 373)

In regard to motivated distortions, Patrick Mahoney (1986) was able to get access to Freud's process notes of his treatment with the Rat Man and did a detailed comparison between the notes and the published account of the treatment. He meticulously documents a large number of discrepancies regarding temporal sequences. In the published version most of these discrepancies were in the direction of saying that things took longer than they actually did. Mahoney concludes that Freud did this for rhetorical purposes – in Mahoney's words because "it made for a better story" (p. 74)

We also have some additional information about Freud's possible motivation for publishing both the Wolf Man and the Rat Man case studies. Ernst Lanzer, the Rat Man had previously been treated by Julius Wagner-Jauregg and the Wolf Man had previously been seen by Emil Kraepelin and Theodore Ziehen three of the most eminent German psychiatrists who had either attacked or ignored Freud and psychoanalysis (Gay, 1988, p. 285; Mahoney, 1986 p. 212).

So, in addition to the many motives and reasons Freud had for choosing to write about and publish these case studies when he did - the theoretical and technical arguments he was presenting and the political agenda of trying to demonstrate the superiority of his theory to those of Adler and Jung, there was also the narcissistic and public relations triumphs of achieving or at least being responsible for some degree of clinical improvement in patients, which his illustrious colleagues were unable to do.

There are also the political and social goals described in Owen Renik's 1994 paper *Publication of Clinical Facts* such as getting referrals and enhancing one's professional reputation as well as the attempt to satisfy a variety of narcissistic wishes for prestige, status, and, of course, money.

Another possible source of distortion in case study material can arise from disguising the patient's identity. There is a strong fear among psychotherapists, particularly after listening to lawyers, of potential adverse legal consequences of publishing or presenting case material. If a patient whose case is published by their therapist, or the therapist's supervisor, gets upset or takes offense, the patient could bring suit against the therapist for breach of confidentiality claiming that the material was not sufficiently disguised and caused the patient to be identified by a third party. Even if the suit is unsuccessful, what the therapist would have to deal with to mount a legal defense is something most members of the profession would go to great lengths to avoid. Strozier (2001), in his biography of Kohut, quotes Robert Stolorow as saying:

"In the current climate of litigiousness, you're in trouble if you don't make up a case. It is now considered good practice in the field to give case presentations that are actually amalgamations of several patients. It is considered legally problematic if you use an actual patient for case." (p. 312).

The point here is that this can lead to the therapist's changing too much of what is significant, seriously distorting the presentation and casting doubt on the validity of any contentions based on the clinical material.

As Freud says about this matter:

"I have come more and more to regard the distortions usually resorted to in such circumstances as useless and objectionable. If the distortions are slight, they fail in their object of protecting the patient from indiscreet curiosity, while if they go beyond this they require too great a sacrifice, for they destroy the intelligibility of the material, which depends for its coherence precisely upon the small details of real life." (Freud, 1909/1975, p. 156)

We have seen that in so many ways what the patient says happened or what the therapist comes to believe was or is the case has been subject to modification or distortion under the influence of multiple wishes and fears. My critique of the case study is that it is of necessity closer to narrative fiction than to the accurate reporting of something that "really happened".

Clinical Research

Many analysts including Eagle (1984, 2010), Wolitzky (2010), Edelson (1984), and Stein (1988) who have written about the methodological difficulties of using case studies to support general propositions about theory or technique, look to clinical research as a way of overcoming these difficulties. They believe in what might be called an amelioration strategy. Namely, the problems they have identified are capable of being solved, that some sort of reality or truth is out there somewhere which we can discover if we use more sophisticated and careful data-collecting and reporting techniques that will eliminate, or at least minimize, bias and distortion in case study material.

However, empirical research, even when it uses the most sophisticated and careful methods, has its own problems when used to validate conclusions about theory or technique.

There is by now a substantial literature on these difficulties. Starting in the 1960s Robert Rosenthal and his colleagues published a number of studies culminating in Rosenthal's 1966 book *Experimenter Effects in Behavioral Research* which, somewhat paradoxically, empirically demonstrates that results of empirical studies are strongly influenced by uncontrolled and unreported variables. Rosenthal's most interesting finding was the surprisingly strong effect of experimenter expectations; the phenomenon of researchers finding what they wanted or expected to find. Essentially the same criticism which has been brought against the validity of findings based on case study material.

More recently, Jonah Lehrer's 2010 *New Yorker* article *The Truth Wears Off: Is There Something Wrong with the Scientific Method?* describes a number of supposedly well-established empirical findings ranging from the therapeutic effectiveness of second-generation antipsychotics to various medical and biological empirically demonstrated truths which can't be empirically replicated.

These subtle perhaps somewhat mysterious forms of bias are of course in addition to the many not-so-subtle reasons for the fictionality of some empirical findings. In particular, the publication of so many of what turned out to be false positive results. These biases or distortions can result from multiple sources including outright fraud and data fudging. They can also be motivated by the enticement of financial incentives, most notoriously present in relation to the vast body of psychopharmacological research that is essentially paid for by the same drug companies that manufacture the drugs being investigated. Other motives for distorting empirical results on the part of individual or groups of researchers include desires for professional recognition, status, career advancement, publicity, the wish to find something novel, dramatic, or unexpected, or plain old financial gain. Again, in many ways not dissimilar to the motives which might influence case study reports.

And then there are the problems of the misuse of the test of significance to argue that a particular empirical finding is significant (in the ordinary sense of the word) when the data shows an inconsequentially small difference between experimental and control groups. These difficulties with relying on empirical findings could be summarized by saying that it is quite difficult to control for all the many factors which might influence the data being examined - any one of which could lead to drawing false conclusions.

This is the point made by Hans Strupp concerning clinical research quoted by Hoffman: "Given the uniqueness of every therapeutic dyad and the multitude of relevant interacting variables influencing the course of treatment, the 'empirical validation' of any therapy is utterly illusory" (Strupp, 2001, p. 613)

For these reasons I would agree with Hoffman that privileging empirical studies over case studies is problematic.

Famous Fictions

Perhaps the most well-known instance of fictionalized case material is that of Kohut's (1979) infamous paper *The Two Analyses of Mr. Z.* in which he creates a fictional person, Mr. Z., who first had a classical and then a self-psychological analysis with Kohut. There is convincing evidence that there was no Mr. Z., that the first analysis was an account of Kohut's treatment with his analyst, Ruth Eissler, and the second was his self-analysis.

Charles Strozier's 2001 biography *Heinz Kohut: The Making of a Psychoanalyst* presents the most complete account of the Mr. Z. story. The external motive for Kohut was that he needed an appropriate case to use in the book *The Restoration of the Self* published in 1977. Kohut had a suitable case which was one that he had been presented by a young German analytic candidate in 1970 in Germany. He did publish this case with significant disguises to protect disclosure of the patient's identity. However when he was going to publish a German translation of the book *Anita Eckstaedt*, the analyst of the case declined to give Kohut permission to publish it. Kohut's solution was to create the autobiographical two analyses of Mr. Z. The paper was published in the *International Journal of Psychoanalysis* in English in 1979.

In the course of his discussion Strozier mentions a number of other published case studies which were later found to be autobiographical but presented as if it were about an actual patient in treatment. The list of therapists who have done this includes William James, Freud (in his 1899 *Screen Memories* paper), Anna Freud (*Beating Fantasies and Daydreams*), James Jackson Putnam, Marie Bonaparte, Helene Deutsch, Karen Horney, Melanie Klein, and Eduardo Weiss. (Strozier, 2001, pp. 313-314)

A related issue is that of the totally fictitious case study. For example, D. M. Thomas' 1981 novel *The White Hotel* which was widely read by analysts as well as the general public, features a quite interesting totally fictitious case study of Freud's. There is also the phenomenon of the TV show *In Treatment* which has generated remarkable interest among analysts. Both of these ersatz treatments have been discussed by gatherings of analysts in ways that are similar to what would occur in a clinical case presentation or group supervision session.

Of course there are innumerable movies that present clinical material which is similarly discussed, studied, explored, and mined by analysts and psychotherapists as if they were real.

Epistemological Challenges

The origin of the term hermeneutic lies in biblical exegesis. The essence of the hermeneutic enterprise is to interpret a text. What transpires between the two individuals engaged in a psychoanalytic dialogue can certainly be approached in this way. The appeal of the Hermeneutic/Constructivist stance for understanding the nuanced complexities of the treatment encounter is clear. It allows us to take into account the full range of factors which may influence what happens in psychoanalytic psychotherapy. This approach fits well with what is done in the various humanistic disciplines. It also is consistent with post-modern sensibilities.

Yet there is something disturbing about such a state of affairs. Perhaps for psychoanalysts who consider themselves to be purely clinical pragmatists there is no problem. If what we do makes things better for the patient then who cares about what is true and what isn't. However, if one regards psychoanalysis, either the theory of explaining human behavior or the clinical technique, as based on some sort of "evidence"; that could be used to convince an intelligent and disinterested person that psychoanalytic theory describes the world in a meaningful way, then what is true and what is not do matter. I am using the word evidence here to refer to real evidence, empirical or otherwise, not the findings that come from poorly designed research conducted by pharmaceutical companies and Cognitive-Behavioral Therapy (CBT) advocates that are judged acceptable by insurance companies and have come to be accepted as effective "evidence based" treatments.

One of the consequences of accepting the Hermeneutic/Constructivist account is that it puts psychoanalysis in the same category as other purely interpretive activities. As Jeremy Safron (2010) says:

". . . 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 . . ." (p. 9)

There are other problematic aspects with adopting the Hermeneutic/Constructivist perspective. What does one say to the skeptic who asks why psychoanalytically informed treatment is better than say, past lives regression therapy, where all present day difficulties are seen as due to unresolved trauma one experienced in previous incarnations; or primal scream treatment, where all hinges on being able to master the trauma of being born; or the CBT model of psychological understanding and their treatment techniques?

For if all is a constructed narrative with no external or "objective" criteria or standards to use to support it, how does one establish what is true and what is not? Also, if one is adhering rigorously to the Hermeneutic/Constructivist position terms such as bias, distortion, or inaccuracy are either meaningless or must be understood in ways far removed from their customary meaning. In their commonly accepted usage these terms imply that there is some external "truth", deviations from which could be described in these ways. If there is no such bedrock, objective, or demonstrable truth the traditional notion of distortion makes no sense.

Being able to refute certain theories or beliefs because they are in some way counter-factual is one of the main reasons for the attachment, loyalty, and valorization of science. There are still some who cling to the modernist sensibility that relying on scientific methods is an important tool for protecting us from the consequences of false beliefs.

Another concern about the Hermeneutic/Constructivist paradigm is that it may accelerate the already strong push toward marginalization that Paul Stepansky (2009) has written about in his book *Psychoanalysis in the Margins*. The danger is that psychoanalysis moves further toward becoming insular and isolated. Insular in the sense of speaking and listening only to those who are already committed to or sympathetic with a psychoanalytic view of the world; talking more to ourselves and less to others. Isolated from not only the evidence based medicine advocates, but also from other disciplines which study the nature of human beings within a natural science framework.

Is Reconciliation Possible?

Hoffman, when pushed, retreats from the radical heart of his position - which is, despite protestations to the contrary, really that psychoanalysis is not a science, at least in any traditional or customary sense. Hoffman makes an attempt at an argument that hermeneutic doesn't necessarily mean nonscientific, but even allowing

for the broadest and most tolerant criteria for what constitutes science, psychoanalysis becomes an enterprise, as Safron says, which is far closer to what a literary critic does than to what a neuroscientist does.

The both/and position that Hoffman takes of arguing that empirical research on clinical process and outcome and a case presentation made from his perspective should be weighted equally as to their evidential value is also problematic. The two methods are based on fundamentally different epistemological positions. Empirical research is ultimately “tally” based – it seeks to find something that could be described as real, objective, and “the truth”. The Hermeneutic/Constructivist case study is not seeking something to tally with what is real.

In his Doublethinking paper, in the course of attempting to refute the criticism that his position leads to an “anything goes” relativism, Hoffman again winds up in a problematic both/and position. He says:

“. . . to say that many theories have some validity does not mean that critical thinking and empirical considerations don't play a part in excluding some theoretical propositions altogether for failing to fit the data.” (p. 1048)

Fitting the data is a concept that belongs in the tally based universe.

The tension and opposition between the two camps is real and important. The dilemma is how to reconcile two fundamentally important truths. The first being the Hermeneutic/Constructivist insight about the multiple and complex influences we are subject to, and that we are always embedded in a particular context. The second being that there do seem to be some properties or characteristics of “human nature”, along with some consistencies in motivation and some principles or laws, which apply to most, if not all, of us not just to a particular therapeutic dyad.

As mentioned earlier in the paper, the Hermeneutic/Constructivist position goes a long way toward solving the epistemological problem of the scientific status of psychoanalysis. Psychoanalysis need not prove itself by means of traditional natural science methods because it's either not a science or a science that should not have to adhere to natural science rules or methods. Also, if as Hoffman and many others who endorse his position often maintain, the primary focus should be on the pragmatic end of seeking to understand what leads to the best technique for achieving the goals of treatment, epistemological questions lose much of their importance. The only validation needed is a convincing demonstration that the treatment process has been genuinely helpful to the patient. Hoffman's position is that case studies are the optimum method for achieving these pragmatic ends.

Those who don't adhere to the Hermeneutic/Constructivist position have a different problem: how to be able to know and demonstrate something “general” without ignoring or oversimplify the immense array of processes, conscious and unconscious, environmental, cultural, or relational, which influence what goes on. They are faced with the enormously difficult task of using empirical research methods to control for, in Hans Strupp's (2001) words: “the uniqueness of every therapeutic dyad and the multitude of relevant interacting variables influencing the course of treatment” (p. 613). Those who believe that psychoanalysis is a scientific enterprise that can tell us something about people's nature that is demonstrably true believe that these difficulties can be overcome by means of being more vigilant and making greater efforts to limit sources of bias and distortion, but still operating within an empirical research paradigm.

So, is reconciliation possible? Is there a way within some sort of consistent epistemology of incorporating both of these truths in a single perspective? This was certainly Freud's wish as well as the hope of Rapaport, Rubenstein, Eagle, Wolitzky, and many other psychoanalysts past and present. It is also Hoffman's position when he argues that both empirical methods and his perspective case studies are both useful for establishing and advancing psychoanalytic knowledge.

But if one adheres to a strictly Hermeneutic/Constructivist position or a strictly empiricist view then the answer is clearly no. They are based on fundamentally different incompatible epistemologies.

From an unadulterated Hermeneutic/constructivist perspective there is no such thing as “objective” truth. Everything is constructed and everything is contextual. There is no truth that that is outside of a particular context. What is true in one context might not be true in another.

From a pure empiricist perspective, essentially positivism, the truth of a statement can only be established by comparing it to some objective, measurable reality that exists out there in the world and is largely independent of context. Additionally, only certain research methods, which must be mathematically rigorous (results must be statistically significant) and methodologically sophisticated, controlling for many variables simultaneously including experimenter bias and expectation effects, can generate findings that are acceptable to the scientific community which adheres to these criteria and standards. If one follows all of these caveats then the findings are considered, within this scientific community, to be validated in a way that, to use Hoffman's term, privileges them over constructed interpretations.

Perhaps there is a way to reconcile but it is difficult to see how. The scientifically minded will never give up their belief that there must be a way to interrogate nature in order to discover something that is true independent of context or what we think about it. The "laws" of physics being the paradigmatic example. The question, as yet unanswered as far as I can see, is whether an Hermeneutic/Constructivist stance can incorporate something that would constitute a way of interrogating nature that would provide evidence in support of psychoanalysis that would be acceptable to that disinterested, intelligent, and skeptical outsider.

As a final comment I would like to situate this debate about the scientific status of psychoanalysis in a broader context. This controversy can be seen as an aspect of, or something that is parallel to, the central conundrum of the mind-body problem that has bedeviled philosophers and all others who have sought to understand the workings of the mind in relation to our ever increasing knowledge about how the physical world works. In particular, with the recent advances in the technology of brain imaging and methods of neurochemical investigation and the resulting explosion in our understanding about how the brain works -- questions of if and how we can relate this new knowledge to our understanding of how the mind works must be addressed. Thus the birth of neuropsychology.

If we are serious about pursuing the neuropsychological enterprise we are faced with the questions, controversies, and paradoxes of how it is that the mind and the brain can interact and influence one another. Much has been written about such matters, from at least since Descartes' attempt to make the Pineal gland central to his explanation for how it happens, to the current popularity of dual or double aspect monism which, while preserving the fundamental monist position that all mental processes are generated by physical events in the brain and the body, maintains that "the mental and the material are different aspects or attributes of a unitary reality which itself is neither mental nor material ([https://www.britannica.com/double-aspect theory](https://www.britannica.com/double-aspect-theory)).

The Hermeneutic/Constructivist position is clearly well on the dualism side of the dualism -- monism issue, as they contend that explanations in terms of meaning, narratives, and the nature of relationships are sufficient for a full psychoanalytic understanding, while the opposing empiricist position is much more compatible with some form of monism.

The problems faced by both dualists and monists have much in common with the respective problems of the Hermeneutic/Constructivists and the empiricists. Dualists say, as Hoffman et al. mostly do, that they have a theory of mind that is sufficient for both understanding people and using this understanding to be helpful for their patients, without needing to know things about how the brain functions. They might see information about how the brain works as a source of ideas or hypotheses but would not see the need to draw closer connections or understand the mechanisms as neuropsychologists do.

The monists, including the neuropsychologists, have the problem of trying to explain the complexities and the vast range of things we have learned about how minds and the people to whom they belong function in terms of what is known about brains. A pure dualist might say that the task is impossible - as the hermeneuticists, following Lloyd Home might say - meanings and mechanisms belong to two separate and non-overlapping ontological categories. They speak different languages. Meanings cannot be explained by mechanisms, no matter how well the mechanisms are understood.

The monists would argue that, even if the task of understanding the mind in terms of brain function is fiendishly difficult and has a long way to go, it is, in theory, doable; even more they would argue that it must be doable - because when all is said and done the brain is all there really is and all the workings of the mind

must ultimately be traceable back to events occurring in the brain.

It seems to me that the latest attempt at solving the mind-body problem, dual aspect monism, does not provide a way of crossing the mind-body chasm. What language does one use to describe this unitary reality which is neither mental nor material?

References

- Eagle, M. N. (1984). *Recent Developments in Psychoanalysis: A Critical Evaluation*. New York: McGraw-Hill.
- Eagle, M. N. (2010). *Panel Discussion on Clinical and Empirical Issues: Disagreements and Agreements*. Unpublished.
- Edelson, M. (1984). *Hypothesis and Evidence in Psychoanalysis*. Chicago: Univ. of Chicago Press.
- Freud, A. (1923). Beating Fantasies and Daydreams. *Int. J. of Psychoanalysis*, 4, 89-102.
- Freud, S. (1975). Studies on Hysteria. In J. Strachey (Ed. & Trans.), *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, (Vol. 2). London: Hogarth Press. (Original work published 1895)
- Freud, S. (1975). Notes Upon a Case of Obsessional Neurosis. In J. Strachey (Ed. & Trans.), *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, (Vol. 10). London: Hogarth Press. (Original work published 1909)
- Freud, S. (1975). On Narcissism: An Introduction. In J. Strachey (Ed. & Trans.), *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, (Vol. 14). London: Hogarth Press. (Original work published 1914)
- Freud, S. (1975). The Unconscious. In J. Strachey (Ed. & Trans.), *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, (Vol. 14). London: Hogarth Press. (Original work published 1915)
- Freud, S. (1975). Introductory Lectures on Psychoanalysis. In J. Strachey (Ed. & Trans.), *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, (Vols. 15 & 16). London: Hogarth Press. (Original work published 1916)
- Freud, S. (1975). Constructions in Analysis. In J. Strachey (Ed. & Trans.), *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, (Vol. 23). London: Hogarth press. (Original work published 1937)
- Gay, P. (1988). *Freud: A Life for our Time*. New York: W. W. Norton & Co.
- Gill, M. M. (1976). Metapsychology is Not Psychology. *Psychological Issues*, 9(4), 71-105.
- Grunbaum, A. (1984). *The Foundations of Psychoanalysis: A Philosophical Critique*. Berkeley: Univ. of California Press.
- Hoffman, I. Z. (2009). Doublethinking Our Way to "Scientific" Legitimacy: The Desiccation of Human Experience. *J. of the American Psychoanalytic Association*, 57(5), 1043-1069.
- Home, H. J. (1966). The Concept of Mind. *Int. J. of Psychoanalysis*, 47, 42-49.
- Klein, G. (1975). *Psychoanalytic Theory: An Exploration of Essentials*. New York: Int. U. Press.
- Kohut, H. (1979). The Two Analyses of Mr. Z. *International J. of Psychoanalysis*, 60, 3-27.
- Lehrer, J. (2010). The Truth Wears Off: Is There Something Wrong with the Scientific Method? *The New Yorker*, Dec. 13, 2010.
- Lotto, D. (1982). Another Point of View on Freud's Metapsychology. *J. of the Amer. Academy of Psychoanalysis*, 10, 457-481.
- Mahoney, P. J. (1986). *Freud and the Rat Man*. New Haven: Yale Univ. Press.
- Masson, J.M. (1985). *The Complete Letters of Sigmund Freud to Wilhelm Fliess*. Cambridge: Harvard Univ. Press.
- Michels, R. (2000). The Case History. *J. of the American Psychoanalytic Association*, 48(2), 355-375.
- Rapaport, D. (1944). The Scientific Methodology of Psychoanalysis. In M. Gill (Ed.), *The Collected Papers of David Rapaport* (pp. 155-220). New York: Basic Books.
- Renik, O. (1994). The Publication of Clinical Facts. *Int. J. of Psychoanalysis*, 75, 1245-1260.
- Rosenthal, R. (1966). *Experimenter Effects in Behavioral Research*. New York: Appleton Century Crofts.
- Rubenstein, B. R. (1976). On the Possibility of a Strictly Clinical Psychoanalytic Theory: An Essay in the Philosophy of Psychoanalysis. *Psychological Issues*, 9(4), 229-264.

- Safron, J. D. (2010). *Doublethinking or Dialectical Thinking: A Critical Appreciation of Hoffman's "Doublethinking" Critique*. Unpublished.
- Schafer, R. (1975). Psychoanalysis Without Psychodynamics. *Int. J. of Psychoanalysis*, 56, 41-55.
- Schafer, R. (1976). *A New Language for Psychoanalysis*. New Haven: Yale Univ. Press.
- Spence, D. P. (1982). *Narrative Truth and Historical Truth: Meaning and Interpretation in Psychoanalysis*. New York: Norton.
- Stein, M. H. (1988). Writing About Psychoanalysis I, Analysts Who Write and Those Who Do Not. *J. of the Amer. Psychoanalytic Association*, 36, 105-124.
- Stepansky, P. E. (2009). *Psychoanalysis at the Margins*. New York: Other Press.
- Strachey, J. (1975). *List of Writings by Freud Dealing Mainly with General Psychological Theory* (S. E. Vol. 14, pp. 259-260). London: Hogarth Press. (Original work published 1957)
- Strozier, C. (2001). *Heinz Kohut: The Making of a Psychoanalyst*. New York: Farrar, Strauss, & Giroux.
- Strupp, H. (2001). Implications of the Empirically Supported Treatment Movement for Psychoanalysis. *Psychoanalytic Dialogues*, 11, 605-619.
- Thomas, D. T. (1981). *The White Hotel*. New York: Viking Press.
- Wallerstein, R. S. (1976). Psychoanalysis as a Science: Its Present Status and its Future Tasks. *Psychological Issues*, 9(4), 198-228.
- Wolitzky, D. L. (2010). *Panel Discussion on Clinical and Empirical Issues: Disagreements and Agreements*. Unpublished.