

BARBARA VON ECKARDT, PH.D.

**ADOLF GRÜNBAUM:
PSYCHOANALYTIC EPISTEMOLOGY**

BEYOND FREUD

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ADOLF GRÜNBAUM: PSYCHOANALYTIC EPISTEMOLOGY

BARBARA VON ECKARDT, PH.D.

Adolf Grünbaum was born on May 15, 1923 in Cologne, Germany. He received his B.A. from Wesleyan University in 1943 with high distinction in mathematics and philosophy, his M.S. in physics from Yale University in 1948, and his Ph.D. in philosophy from Yale University in 1951. He began his teaching career at Lehigh University in 1950. Five years later he was appointed William Wilson Selfridge Professor of Philosophy. In 1960 he accepted a position at the University of Pittsburgh as Andrew Mellon Professor of Philosophy, where he has been ever since. In 1979 he was also appointed Research Professor of Psychiatry at the same university on the basis of his work on psychoanalytic epistemology.

Grünbaum is currently one of the leading figures in contemporary philosophy of science. He has been president of the Philosophy of Science Association for two terms, 1965-67 and 1968-70, and was elected president of the American Philosophical Association (Eastern Division) for 1982-83. In addition, he has received numerous honors and awards for his work, the most recent of which is a festschrift in his honor (Cohen & Laudan, 1983) containing essays by 14 of today's principal researchers in philosophy of science as well as two leading psychoanalysts.

Grünbaum's interest in psychoanalysis is relatively recent. His past work primarily concerned philosophical problems of space and time and the theory of scientific rationality (see Cohen & Laudan, 1983 for a complete bibliography). Since 1976, however, when his first paper on psychoanalytic epistemology appeared (Grünbaum, 1976), he has produced at least 10 papers as well as a book on the subject, which have succeeded in completely changing the state of the art. The purpose of this essay, then, is to provide a summary and critique of this work.

The two fundamental questions that Grünbaum's work on psychoanalysis addresses are these:

1. What sort of standards of assessment ought we to invoke in evaluating psychoanalysis? That is, ought we to regard it as making knowledge claims, and, if so, what kind?
2. Given that we have chosen certain standards of assessment, how does psychoanalysis measure up to those standards?

With respect to the first question, Grünbaum has argued emphatically that (a) the most appropriate standards of assessment for psychoanalysis are those derived from empirical science, contrary to the claims of the hermeneuts, Jurgen Habermas, Paul Ricoeur, and George Klein (Grünbaum, 1983c, 1984); and (b) psychoanalysis meets the minimal conditions necessary for applying those standards, contrary to the claims of Karl Popper (1963) (who accepts Grünbaum's

first statement but denies the second on the grounds that psychoanalysis is unfalsifiable) (Grünbaum, 1976, 1977, 1979). With respect to the second question, however, his stance has been severely critical. In his view, there are serious difficulties in the way of regarding psychoanalysis as good science. These stem not only from serious liabilities involved in the use of clinical data but also from the modes of reasoning that Freud used to provide evidential support for his theory (Grünbaum, 1983b, 1984).

It should be clear that any attempt to argue convincingly either for or against the scientific status of psychoanalysis ought to be informed by *both* a thorough understanding of the psychoanalytic literature and a sophisticated conception of the nature of science. The literature prior to Grünbaum's recent outpouring on the subject suffers, in my view, in both of these respects. That is, either it exhibits a very superficial understanding of psychoanalysis or it is naive about the nature of science. The importance of Grünbaum's contribution in the area of psychoanalytic epistemology rests on the fact that his work is unparalleled on both counts. Not only does he bring to bear a very great sophistication in the philosophy of science but, in addition, he has done his psychoanalytic homework.

In 1959, the philosopher John Hospers summed up the results of one of the first major conferences on philosophy and psychoanalysis as follows:

As I try to get a composite picture of the results of the conference, the thing that stands out most in my mind is the lack of genuine communication between the psychoanalysts and the philosophers.

Psychoanalysts are, quite understandably, too busy treating patients to have acquainted themselves with the latest guns in the arsenal of epistemology and philosophy of science, and are therefore at a loss to reply to the charges leveled at them by the philosophers in the way the philosophers want. The philosophers, for their part, are—equally understandably—ignorant of the vast amount of empirical detail garnered by psychoanalysts in the last half-century as well as the complexity of many of the theoretical concepts employed in psychoanalysis. The inevitable result is that each party to the dispute only feels confirmed in his previous suspicion, namely that the other party's remarks are either incompetent or irrelevant, given to making either scandalously overblown claims or excessively demanding systematic requirements [p. 336].

I believe that Grünbaum has gone more than halfway toward closing this communication gap from the philosophical side. Not only is his work impressively learned with respect to the psychoanalytic literature, as already mentioned, but he has also worked very hard at establishing lines of communication with the psychoanalytic community. For all of this, however, his writing may not be easily accessible to psychoanalysts and students of psychoanalysis, for it does presuppose a considerable sophistication in the philosophy of science and the techniques of philosophical argumentation. It is chiefly this consideration that has dictated the style of the present essay. Grünbaum's work merits serious attention from anyone interested in the cognitive status of psychoanalysis. My principal concern, therefore, has been to make the most important of his ideas and arguments accessible to the reader. This approach has had several consequences. First, I have devoted a certain amount of space to providing background that seemed to me essential to understanding either the content of Grünbaum's writing

or its importance. Second, I have had to strike a compromise between the demands of depth and breadth in the discussion of Grünbaum's work itself. Grünbaum's writing is exceedingly rich. In attempting to present clearly the central lines of argumentation, much of this richness has necessarily been lost. I thus urge the reader interested in his work to consult the original. In addition, certain topics have simply not been touched on at all. Where this is the case, I have tried to indicate what has been omitted in the appropriate place in my discussion.

SHOULD FREUDIAN PSYCHOANALYSIS BE ASSESSED AS SCIENCE?

Grünbaum's approach to this question has been twofold. First, he has repeatedly emphasized that Freud himself regarded psychoanalysis as scientific. In support of this claim, he cites passages such as the one in which Freud states that the explanatory gains from positing unconscious mental processes "enabled psychology to take its place as a natural science like any other" (Freud, 1940a, p. 158, see also 1925, p. 58; 1933, p. 159; 1940b, p. 282). Second, Grünbaum has devoted considerable effort to providing counterarguments to those who have suggested that, for one reason or another, Freudian theory ought not be regarded as scientific on the grounds that it fails to satisfy certain minimal requirements for scientific candidacy. These arguments have been directed, in particular, against Karl Popper, Jurgen Habermas, Paul Ricoeur, and George Klein.

Although it might appear that Grünbaum has simply adopted the strategy of

shifting the burden of proof to those who wish to deny scientific status to Freudian theory, it is possible to view his discussion as part of an overall implicit positive argument as follows:

1. A body of work should be judged by the standards of adequacy subscribed to by the author or creator unless there is compelling reason not to.
2. Freud took himself to be doing science.
3. The reasons that have been offered in the literature against assessing psychoanalytic theory in terms of the standards of science are uniformly unconvincing.
4. Therefore, Freudian psychoanalytic theory ought to be assessed as science.

Since the second premise is not difficult to establish, the bulk of Grünbaum's discussion on this matter has been devoted to justifying the third premise. In the discussion that follows, we shall focus on his consideration of the arguments of Popper and Habermas. Readers interested in his discussion of Ricoeur and Klein should consult Grünbaum, 1984, pp. 43-93.

PSYCHOANALYSIS AS PSEUDO-SCIENCE

POPPER'S CHALLENGE

In 1953, in a paper reviewing his philosophical work of the past 30 or more years, Karl Popper challenged the scientific status of psychoanalysis, claiming that it was nothing more than a pseudoscience. His reasoning was this: To be scientific, a theory must be falsifiable; however, psychoanalytic theory is not falsifiable. Therefore, psychoanalytic theory is not scientific. Interestingly enough, it was in part the case of psychoanalysis that led Popper to see the importance of falsifiability in the scientific process in the first place.

When the problematic nature of psychoanalysis first occurred to him, Popper's principal concern was the so-called "problem of demarcation." This is the problem of distinguishing theories that can *legitimately* be considered candidates for scientific evaluation from those that cannot, in particular, from those "pseudoscientific" theories such as astrology that share certain superficial characteristics with genuine scientific theories but that lack some essential feature. The accepted demarcation principle at the time was an inductivist one: A theory is scientific just in case it is inductively well confirmed on the basis of empirical evidence. It was in part the contrast between Freud's psychoanalytic theory and Einstein's theory of gravitation that led Popper to believe that this was an incorrect view. On intuitive grounds, something seemed to be wrong with psychoanalysis, but the problem could not be its lack of "verifications" because these seemed to be rampant. Popper began to suspect that the difficulty was precisely that psychoanalytic theory could always be verified *no matter what*. In contrast, a genuine scientific theory like Einstein's was distinguished by the fact

that, *if* it were false, it could be falsified so easily, because potentially falsifying test outcomes were readily imagined. Popper (1963) wrote:

I found that those of my friends who were admirers of Marx, Freud, and Adler, were impressed by a number of points common to these theories, and especially by their apparent *explanatory power*. These theories appeared to be able to explain practically everything that happened within the fields to which they referred. The study of any of them seemed to have the effect of an intellectual conversion or revelation, opening your eyes to a new truth hidden from those not yet initiated. Once your eyes were thus opened you saw confirming instances everywhere: the world was full of *verifications* of the theory. Whatever happened always confirmed it. Thus its truth appeared manifest; and unbelievers were clearly people who did not want to see the manifest truth; who refused to see it, either because it was against their class interest, or because of their repressions which were still 'un-analyzed' and crying aloud for treatment. The most characteristic element in this situation seemed to me the incessant stream of confirmations, of observations which "verified" the theories in question. ...It began to dawn on me that this apparent strength was in fact their weakness [p. 34].

In contrast, the situation with Einstein's theory was "strikingly different." On the basis of his theory of gravitation, Einstein had predicted that light from a distant star would be bent near the sun. What was impressive about this case, according to Popper (1963), was the risk involved in a prediction of this kind. For

if observation shows that the predicted effect is definitely absent, then the theory is simply refuted. The theory is *incompatible with certain possible results of observation*—in fact with results which everybody before Einstein would have expected. This is quite different from the situation I have previously described, when it turned out that the theories in question were compatible with the most divergent human behaviour, so that it was practically impossible to describe any human behaviour that might not be

claimed to be a verification of these theories [p. 36].

It was this purported insight that led Popper to his well-known principle of falsifiability. In addition, he proposed that *the* method of science is essentially one of bold conjectures and attempted refutations whose rationality lies in the facts that first, scientists are always seeking to falsify their theories and, second, they accept their theories only (and always only tentatively) when they have successfully resisted numerous attempts at falsification.

THE RESPONSE TO POPPER'S CHALLENGE

The philosophical response to Popper's challenge over the past 20 years has taken a variety of forms. In order to understand Grünbaum's contribution to this discussion, it will be useful to indicate briefly the major positions that have been taken.

It was noted quite early on that there is an important ambiguity in the claim that psychoanalysis is not falsifiable. Kennedy (1959), for example, pointed out that psychoanalysis can be considered unfalsifiable for two very different reasons: first, because of the attitude of its proponents in the face of allegedly unfavorable evidence; and, second, because of the logical structure of the theory. Martin (1964b) refined this distinction further by introducing four possible senses of the notion of refutability, two of which concerned the attitudes of proponents of the theory, and two of which concerned its logical structure. He wrote:

When we ask whether a theory T is a refutable theory, we may be asking any of the following questions:

1. Are people who are advocates of theory T willing to specify what evidence could count against theory T?
2. Are people who believe in theory T willing to accept some of the evidence brought forth to refute theory T instead of explaining it all away?
3. Is the relation between the theoretical language and the observational language of theory T clear and unambiguous?
4. Is it possible to give theory T, in which the relation between the theoretical and observational language is extremely vague and ambiguous, clear and unambiguous formulation [p. 81]?

Martin claimed, however, that the fourth question is not an interesting sense of 'refutable,' since *any* theory can be considered refutable in that sense, including those that we consider paradigm cases of unrefutable theories (such as that the absolute is perfect and developing in history).

If we subdivide Popper's challenge into two parts, one directed at the attitudes of its proponents and one at the logical structure of psychoanalytic theory, we find endorsements of both positions in the literature. For example, a number of people have argued that the proponents of psychoanalytic theory typically exhibit a very unscientific attitude with respect to putative disconfirming data. After proposing the four senses of 'refutability,' Martin (1964b) claimed that the answer to the first two questions is no. Typically, psychoanalysts are unwilling

to specify what evidence will count against their theory. Furthermore, they tend to discount any allegedly disconfirming evidence. A similar view had been voiced earlier by Hook (1959a).

Cioffi (1970) took the charge much further. Psychoanalysis is a pseudoscience, he wrote, principally because it uses methodologically defective procedures:

For an activity to be scientific it is not enough that there should be states of affairs which would constitute disconfirmation of the theses it purports to investigate; it must also be the case that its procedure should be such that it is calculated to discover whether such states of affairs exist. I use the word "calculated" advisedly. For to establish that an enterprise is pseudo-scientific it is not sufficient to show that the procedures it employs would *in fact* prevent or obstruct the discovery of disconfirmatory states of affairs but that it is their function to obstruct such discovery. To claim that an enterprise is pseudo-scientific is to claim that it involves the habitual and willful employment of methodologically defective procedures (in a sense of willful which encompasses refined self-deception) [p. 472].

Cioffi goes on to argue that Freudian psychoanalysis is pseudoscientific in precisely this sense. For it is characterized by a "host of peculiarities...which are apparently gratuitous and unrelated, but which can be understood when once they are seen as manifestations of the same impulse: the need to avoid refutation" (p. 473). The principal devices that Freud uses to accomplish this end, according to Cioffi, are these: First, hypotheses presented prior to the discovery of apparently disconfirming evidence are, typically, formulated in a narrow and determinate sense; afterwards, however, they are construed in a "broader and

hazier” way so as to avoid the disconfirmation. Second, prior to the discovery of apparently disconfirming evidence, Freud allows for the relevance of evidence from a number of intersubjective sources, including observation of the behavior of children, inquiry into the distinctive features of the current sexual lives or actual infantile sexual history of neurotics, or determination of the outcome of therapy based on his theory. In the face of apparently disconfirming evidence, however, he typically retreats to the claim that the only reliable source of evidence is material obtained during the psychoanalytic session and subjected to interpretation by a trained analyst. Third, his theory contains such a variety of mechanisms and interpretative principles that it is possible for him to interpret any phenomenon in a way consistent with his theory. Thus, “he typically proceeds by beginning with whatever content his theoretical preconceptions compel him to maintain underlies the symptoms, and then, by working back and forth between it and the explanandum, constructing persuasive but spurious links between them” (Cioffi, 1970, p. 497). Finally, his interpretations are not even constrained by considerations of logic; for it is not even necessary for the various meanings of a symptom to be compatible with one another.

The principal early supporter of Popper’s position with respect to the nonfalsifiability of Freudian theory in the logical sense was Nagel. In a classic paper (Nagel, 1959) he offers us an analysis of precisely *why* psychoanalytic theory is problematic:

The theory does not seem to me to satisfy two requirements which any theory must satisfy if it is to be capable of empirical validation....In the first place, it must be possible to deduce determinate consequences from the assumptions of the theory, so that one can decide on the basis of logical considerations, and prior to the examination of any empirical data, whether or not an alleged consequence of the theory is indeed implied by the latter. For unless this requirement is fulfilled, the theory has no definite content, and questions as to what the theory asserts cannot be settled except by recourse to some privileged authority or arbitrary caprice. In the second place, even though the theoretical notions are not explicitly defined by way of overt empirical procedures and observable traits of things, nevertheless at least some theoretical notions must be *tied down to fairly definite and unambiguously specified* observable materials, by way of rules of procedure variously called "correspondence rules," "coordinating definitions," and "operational definitions." For if this condition is not satisfied, the theory can have no determinate consequences about empirical subject matter [p. 40].

Nagel argued that Freudian theory failed both of these conditions primarily because of its vagueness and metaphorical character.

Freudian formulations seem to me to have so much "open texture," to be so loose in statement, that while they are unquestionably suggestive, it is well-nigh impossible to decide whether what is thus suggested is genuinely implied by the theory or whether it is related to the latter only by the circumstance that someone happens to associate one with the other [p. 41].

Martin (1964b) provided further support for Nagel's view. As the quote from Nagel makes clear, the accepted view at the time was that the empirical import of a genuinely scientific theory (i.e., the link to its observation base) is mediated by so-called correspondence rules, consisting either of explicit or partial definitions of the theoretical vocabulary in terms of an observational vocabulary. Since the

existence of such correspondence rules is a necessary condition of a theory being falsifiable, one way to ascertain the scientific status of psychoanalytic theory, according to Martin (1964b), is to try one's best "to separate the observational basis of the theory from the theoretical structure, and to extract rules of correspondence from the context of the uses of the two languages" (p. 85). When Madison (1961) used this strategy, he concluded that for some aspects of psychoanalysis, there was no associated observational language and rules of correspondence, whereas for others, there was. Martin, however, argues that Madison's allegedly positive results are incorrect. What Madison actually found, according to Martin (1964b), are "the rudiments of an observational language and rules of correspondence" (p. 86). Madison takes these rudiments and reformulates them into a clearer and more precise form, but he fails to distinguish his formulations from Freud's. Thus, he only shows that Freudian theory is falsifiable in Martin's fourth and, presumably, uninteresting sense.

There have been, however, a few dissenting voices. Salmon (1959) argued that psychoanalytic theory appears to be unfalsifiable only if one assumes that "a few restricted items of behavior can constitute evidence for or against the hypothesis" (p. 262). It is true, according to Salmon, that any single item of behavior may be compatible with a hypothesis, for example, that the patient suffers from unconscious hostility toward his father, for according to psychoanalytic theory, unconscious hostility can be expressed in a variety of ways and is served by a variety of mechanisms. This does not mean, however, "that

every total behavior pattern is compatible with the hypothesis of unconscious hostility” (p. 262). A similar point was made by Hospers (1959). There are no “crucial experiments” for psychoanalysis, but neither do they exist for physics. What validates or invalidates psychoanalytic hypotheses are *patterns* of behavior. Correspondence rules do not take the form of “If *p*, then *q*”; rather they look like “If *p*, then *q* or *r* or *s* or...” followed by a finite disjunction of propositions. And since the disjunction is finite, Hospers (1959) argues, “it is emphatically *not* true that the Oedipus complex would be believed in no matter *what* the empirical facts are: if none of the items *q*, *r*, *s*... occurred, it would have to be concluded (and would be) that the individual in question had no Oedipus complex” (p. 343). More recently, Glymour (1974, 1980) has argued that there is a rational strategy for testing important parts of psychoanalysis and that this strategy was immanent in at least one of Freud’s (1909) case studies, that of the Rat Man. In particular, the best available evidence concerning the actual life history of the Rat Man, Paul Lorenz, had refuted the hypothesis Freud held at the time concerning the sexual etiology of adult obsessional neurosis.

A number of philosophers came to Freud’s defense in a quite different way. They agreed that if the falsifiability of a theory requires that the theory alone (mediated only by correspondence rules) entails a falsifiable observation statement, then Freudian psychoanalytic theory is, strictly speaking, unfalsifiable. However, this does not necessarily make it a pseudoscience. Why not? Farrell (1963, 1964) suggested that there was another option available. Psychoanalysis is,

on his view, a *protoscience*. That is, it is an “empirical and speculative synthesis, which is premature in that it runs far ahead of the evidence that can upset or support it with reasonable certainty” (Farrell, 1963, p. 24). Nevertheless, there is reason, he claims, to take it seriously as a tentative basis for future research. The psychoanalytic method has produced an enormous amount of factual material, which the theory has to some degree succeeded in ordering, describing, and explaining. In addition, a lot of experimental work by psychologists attempting to test psychoanalytic theory seems to show that in places, at least, Freudian theory is “on to something.”

Another, far more damaging reason for rejecting Popper’s claim that psychoanalytic theory is pseudoscientific *because* it is unfalsifiable was offered by Lakatos (1970, 1971). He suggested that Popper’s demarcation criterion can be assessed in terms of the following metacriterion: “If a demarcation criterion is inconsistent with the “basic’ appraisals of the scientific elite, it should be rejected” (Lakatos, 1971, p. 125). Given this metacriterion, Popper’s demarcation principle is clearly problematic. For “exactly the most admired scientific theories simply fail to forbid any observable state of affairs” (Lakatos, 1970, p. 100). The principal reason for this is that most scientific theories are normally interpreted as containing a so-called *ceteris paribus* clause. That is, they “*forbid an event occurring in some specified finite spatio-temporal region...only on the condition that no other factor...has any influence on it*” (p. 101). But then if a prediction of the theory is not borne out, the theory is not automatically falsified because “by

replacing the *ceteris paribus* clause by a different one the *specific* theory can always be retained whatever the tests say” (p. 101-102). In the philosophy of science literature of recent years, a more general version of this point has become commonplace. As we noted, in the early responses to Popper’s challenge, it was generally assumed that theory and observation are mediated by correspondence rules. In recent years, however, it has been argued that the so-called “received view” of correspondence rules vastly oversimplifies the relationship between a theoretical hypothesis undergoing test and the observable evidence adduced in its behalf. Careful examination of scientific case studies has revealed that theory and data are often mediated by a complex array of auxiliary propositions: hypotheses from related theories, theories of measurement and theories of the data, assumptions about the experimental situation, and assumptions about the ways in which the putative theoretical states causally influence the observable states of affairs (see Schaffner, 1969; Suppes, 1962, 1967; and more recently, Hempel, 1970, 1973).

That theories are connected with observable results only via a mediating link of auxiliary hypotheses has important implications for the testing of theories. For if theories confront data only in conjunction with other theories or hypotheses, then if a theory’s prediction is not borne out, the most one can conclude is that *either* the theory *or* one of the auxiliary hypotheses is wrong. As the nineteenth century philosopher and physicist, Pierre Duhem (1906) wrote:

The physicist can never subject an isolated hypothesis to experimental test but only a whole group of hypotheses; when the experiment is in disagreement with its predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed [p. 187].

The point again is that if one takes Popperian falsifiability to require that the theory whose status is being determined can *in itself* make falsifiable predictions, then very few legitimate scientific theories, if any, will be falsifiable by themselves. Hence—so the argument goes—falsifiability ought to be rejected as a demarcation criterion.

Whether a revised version of falsifiability can be formulated that will be serviceable as a demarcation criterion is still a matter of controversy. Popper (1963, p. 112) himself briefly considers the matter (see Grünbaum, 1976 for a discussion of this passage). I have made some positive suggestions in this area (Von Eckardt, 1982) as has Lakatos (1970). In contrast, Laudan (1983) has recently argued that no satisfactory demarcation principle will be forthcoming, especially not one formulated along Popperian lines. However this issue is resolved, it is important to keep in mind that even if falsifiability cannot function as a demarcation principle (which requires it to be both necessary and sufficient for a theory's being scientifically entertainable), it may well constitute simply a necessary condition. In any case, it certainly behooves us to appraise Popper's rejection of psychoanalysis as pseudoscience on the alleged ground of

unfalsifiability. Thus, if we are interested in the scientific status of Freudian psychoanalytic theory, it remains a worthwhile project to inquire into its falsifiability.

GRÜNBAUM'S CONTRIBUTION

Grünbaum has had something to say on virtually every aspect of the issue of the falsifiability of Freudian theory. What makes his discussion so noteworthy is that it takes place against a background of serious consideration of the importance and relevance of the requirement of falsifiability in the scientific enterprise in general. Thus, before we turn to a discussion of his response to the pseudoscience challenge, I shall briefly summarize his work in philosophy of science that pertains to falsifiability.

As the previous discussion should have made clear, there are two extreme positions that someone can take on the importance of falsifiability in science. On the one hand, it can be argued, as Popper has done, that falsifiability is the “touchstone of scientific rationality.” On the other hand, there is the view, inspired by Duhem, that falsifiability is completely unimportant in science because no scientific theory is ever, strictly speaking, falsifiable. In a series of important papers in the 1960s and 1970s, Grünbaum took on both of these extreme positions, advocating instead a more reasonable, middle-of-the-road view.

Grünbaum (1969) considered what he calls the “D-thesis,” a view, that if not

historically attributed to Duhem, represents the Duhemian philosophical legacy in contemporary philosophy of science. The D-thesis consists of the following two claims.

D1. No constituent hypothesis H of a wider theory can ever be sufficiently isolated from some set or other of auxiliary assumptions so as to be separately falsifiable observationally. H is here understood to be a constituent of a wider theory in the sense that no observational consequence can be deduced from H alone.

It is a corollary of this subthesis that *no* such hypothesis H *ever* lends itself to a crucially falsifying experiment any more than it does to a crucially verifying one.

D2. In order to state the second subthesis D2, we let T be a theory of *any* domain of empirical knowledge, and we let H be *any* of its component subhypotheses, while A is the collection of the remainder of its subhypotheses. Also, we assume that the observationally testable consequence O entailed by the conjunction H & A is taken to be empirically false, because the observed findings are taken to have yielded a result O' *incompatible* with O. Then D2 asserts the following: For all potential empirical findings O' of this kind, there exists at least one suitably revised set of auxiliary assumptions A' such that the conjunction of H with A' *can be held to be true and explains* O'. Thus D2 claims that H can be held to be true and can be used to explain O' no matter what O' turns out to be, i.e., *come what may* [p. 1070-1071].

Grünbaum (1966) argued the following three points with respect to the D-thesis: (1) There are quite trivial senses in which D1 and D2 are uninterestingly true and in which no one would wish to contest them (see pp. 276-280); (2) In its nontrivial form, D2 has not been demonstrated (see pp. 280-281); and (3) D1 is false, as shown by counterexamples from physical geometry (see pp. 283-295;

Grünbaum, 1968, 1969). Grünbaum (1969) discusses this further and, in response to criticism, introduces a qualification with respect to the third point (3). Grünbaum concedes to Hesse (1968) that if the falsification of H denied by D1 is construed as *irrevocable*, then his geometrical example does not succeed as a counterexample. However, he insists that it does succeed if falsification is construed in a scientifically realistic sense, that is, if one requires “only falsification to all intents and purposes of the scientific enterprise” (p. 1092). In sum, then, in Grünbaum’s view, falsifiability is a meaningful notion in science.

Falsifiability is not, however, the only possible basis for a demarcation principle or the only possible ground for a theory of scientific rationality, as Popper has claimed. In Grünbaum’s view, the alternative—inductivism—which Popper summarily dismisses, merits serious consideration as well. Grünbaum (1976, 1977, 1979) argues that Popper’s rejection of inductivism rests on a serious misportrayal.

Inductivism offers the following demarcation principle: A theory is scientific (“I-scientific”) if and only if it qualifies as empirically well supported by neo-Baconian standards of controlled inquiry. Note that this is quite different from a demarcation principle based on falsifiability, in that the focus is on the *credibility* of the theory rather than simply its *entertainability*. Thus, a speculative theory in physics, for example, for whom evidence has not yet been gathered, would not count as actually I-scientific, but only as *potentially* so, although the latter would

qualify it as scientifically entertainable.

Grünbaum's dispute with Popper concerns what sorts of theories inductivism would count as being empirically well supported. According to Grünbaum (1977), Popper attributes the following to inductivism: "If a theory T can explain a sufficiently large *number* of observational results or has a suitably large number of so-called positive instance, then T *automatically* qualifies as *well-supported* by the evidence" (p. 224). The distinction between being a positive instance and being a supportive one is crucial here. According to Grünbaum (1976), "an instance is a 'positive' one with respect to a *non*-statistical theory T, if its occurrence or being the case can be deduced from T in conjunction with suitable initial conditions. But an instance is supportive of T, if it is positive *and* has the probative significance of conferring a stronger truth presumption on T than T has without that instance" (p. 217). Thus, Popper would claim that inductivism requires a positive instance to be *sufficient* for being a supportive one. This claim overlooks two important features of the inductivist position, in Grünbaum's view:

1. *The "declared consequence restriction."* Grünbaum (1979) states: "If, at a particular time, S is *declared* to be a logical consequence of T under the assumption of stated initial conditions, or is declared not to be such a consequence, then *neither* declaration is allowed to depend on *knowing* at the time whether S is true" (p. 133). The point is that what counts as a consequence of a

theory T (and, hence, as a positive instance) is a function solely of the logical relations between T and this consequence; whether it is true or false is completely irrelevant. According to Grünbaum (1977) this requirement is “at least implicitly imposed by inductivists to preclude ‘retroactive’ tampering with the construal of T as follows: S is only *ex post facto* held to have followed from T after having been found to be true” (p. 227).

2. *The need for controls with respect to causal hypotheses.* According to Grünbaum, this need has been emphasized by inductivists ever since Francis Bacon wrote three centuries ago. Consider a causal hypothesis of the form, “Events of kind X are causally relevant to (either causally necessary for, causally sufficient for, or stochastically relevant to) events of kind Y.” A merely *positive* instance for such a hypothesis will be an event of kind X coupled with an event of kind Y. For example, if the hypothesis (H) in question is “*Ceteris paribus*, daily consumption of at least one-fifth pound of coffee for two weeks [X] is causally sufficient as well as causally necessary for the remission of colds [Y],” then a positive instance of H would be one case of a person with a cold drinking at least one-fifth pound of coffee for two weeks and getting rid of his or her cold at the end of that period. Such a positive instance would not, however, count as *supportive*, in the inductivist view, *unless* it is conjoined with findings from an appropriate control group. For, as Grünbaum (1977) states “Even a large number of cases of X which are also cases of Y does not preclude that an equally large number of cases of non-X are also cases of Y. But being an X should make a difference with respect

to being a Y,” (p. 232) given the claim of causally sufficiency. In addition, “if there is to be inductive warrant for deeming coffee to be remedially necessary, *every* known case of non-X would have to be a case of non-Y” (p. 232). In sum, Grünbaum concludes, “only the *combination* of positive instances with instances of non-X and non-Y could constitute inductively supportive instances of our strong causal hypothesis H” (p. 232).

Note that given this more accurate portrayal of inductivism, any theory containing causal hypotheses that is I-scientific will necessarily be falsifiable as well, although, of course, the converse will not be true. Thus, in Grünbaum’s view, Popper was completely wrong in claiming that, in contrast to falsifiability, inductivism is powerless to impugn the scientific credentials of a theory like psychoanalysis. In fact, as we shall discuss later, one of Grünbaum’s principal theses is that the weakness of Freudian theory lies not in its unfalsifiability but in the fact that it fails to satisfy neo-Baconian standards of inductive credibility.

Let us turn now to Grünbaum’s response to the challenge of unfalsifiability. His principal points are the following: First, the *arguments* that have been offered by Popper and others to show that psychoanalytic theory is unfalsifiable are inadequate. Second, although there is some merit to the charge that the majority of Freud’s defenders, and even sometimes Freud himself, have exhibited a “tenacious unwillingness...to accept adverse evidence” (Grünbaum, 1979, p. 138), Cioffi’s *global* indictment of Freud’s methodology as pseudoscientific cannot be

sustained. And, third, given any reasonable scientific sense of falsifiable (that is, *modulo* revocable auxiliary assumptions and initial conditions), there are clear counterexamples to the thesis of unfalsifiability.

The thesis of unfalsifiability says that there does not exist even one way in which Freudian theory could, in principle, be falsified. As Grünbaum (1983b) points out, since a negative claim is here being made about an infinite class of consequences of the theory, it is not even clear what a good argument for this claim would look like. Certainly, what Popper offers us is not satisfactory. For instead of providing a *general* argument to support his general claim, he simply gives us a single alleged example of how Freudian theory could explain the facts no matter how they turn out. Popper (1963) describes two cases: that of a man who pushes a child into the water with the intention of drowning it; and that of a man who sacrifices his life in an attempt to save the child. He writes: "Each of these two cases can be explained with equal ease in Freudian...terms. According to Freud the first man suffered from repression (say, of some component of his Oedipus complex), while the second man had achieved sublimation" (p. 35).

As an argument for the unfalsifiability thesis, this example fails miserably, according to Grünbaum (1979). First, "why would it necessarily be a liability of psychoanalysis, if it *actually* could *explain* the two cases of behavior with equal ease? Presumably there actually are such instances of self-sacrificing child-*rescuing* behavior no less than such cases of *infanticidal* conduct. And a *fruitful*

psychological theory might well succeed in actually explaining each of them” (pp. 134-135). Second, even if this case were cogent, it is certainly not clear how it is supposed to *generalize* to cover the infinite class of cases which fall under the thesis. Popper seems to be relying on the method of “induction by enumeration,” which he himself has rejected as inadequate. Third, the example is totally contrived. Popper should, at least, have chosen an example based on the Freudian text. Finally, Popper claims that Freudian theory could *explain* both of these cases. However, such explanations are forthcoming only if the psychoanalytic theorist is at liberty to posit initial conditions *at will*. But, asks Grünbaum (1979): “Is it clear that the postulation of initial conditions *ad libitum* without any *independent* evidence of their fulfillment is quite generally countenanced by that theory to a far greater extent than in, say, physics, which Popper deems to be a bona fide science?” (p. 135). Certainly, Popper gives us no argument to that effect. Eysenck (Eysenck & Wilson, 1973) puts forth another similar argument, which Grünbaum (1979, pp. 138-139) discusses and dismisses as inadequate.

Grünbaum considers Cioffi’s (1970) claim that Freud’s methodology was prompted chiefly by the need to avoid refutation. After carefully reexamining the textual passages on which Cioffi builds his case, Grünbaum (1980b) concludes that Cioffi “mishandled” his examination of Freud’s reasoning and “was thereby driven to the gratuitous or mistaken conclusion that concern with pertinent evidence had played no essential role in Freud’s rationale for espousing psychoanalysis” (p. 84). Freud was willing to acknowledge both the possibility and, on several occasions,

the fact of falsification, according to Grünbaum. In support of this contention, he cites the following cases:

1. In his “Reply to Criticisms of My Paper on Anxiety Neurosis” Freud (1895) stated explicitly what sort of finding he would acknowledge to be a *refuting* instance for his hypothesis concerning the etiology of anxiety neurosis.
2. In 1897 Freud abandoned his hypothesis that actual episodes of traumatic seduction in childhood were responsible for the occurrence of hysteria in adulthood. Among the reasons that he explicitly cites (see Freud, 1954, pp. 215-216) is the fact that the hypothesis had extremely implausible consequences; in particular, the required incidence of perverted acts against children would have had to have been preposterously high (Grünbaum, 1979, p. 135).
3. In 1909 Freud recognized that the best available evidence concerning the actual life history of his “Rat Man,” Paul Lorenz, refuted his prior hypothesis concerning the etiology of adult obsessional neurosis (Grünbaum, 1979, p. 137).
4. In “A Case of Paranoia Running Counter to the Psychoanalytic Theory of the Disease” Freud (1915) considered the case of a young woman who appeared to be paranoid but who initially failed to give any indication

of the underlying homosexual attachment that Freud had hypothesized to be causally necessary for paranoia. At this point, he reasoned: “Either the theory must be given up or else, in view of this departure from our [theoretical] expectations, we must side with the lawyer and assume that this was no paranoic combination but an actual experience which had been correctly interpreted” (p. 266; Grünbaum, 1983b, p. 155).

5. Freud’s (1933) “Revision of the Theory of Dreams” presents an acknowledged falsification on the basis of the recurrent dreams of war neurotics.

These cases not only suffice to undermine Cioffi’s pseudoscience charge, they also function as counterexamples to the claim that, from a logical point of view, Freudian theory is unfalsifiable. To further emphasize the incorrectness of the logical unfalsifiability thesis, Grünbaum mentions a number of additional cases of either possible or actual (revocable) falsification:

1. In Freud’s theory of personality types, both personality traits and a specific childhood etiology are associated with each character type. Thus, for example, Freud claims that the “oral” character is associated with dependency, submissiveness, need for approval, and pessimism and originates in such unfavorable childhood experiences as

premature weaning. Grünbaum (1979, p. 137) suggests that this coupling of certain personality traits with certain childhood experiences is at least *prima facie* falsifiable.

2. Grünbaum (1979, p. 137) notes that experimental work has provided evidence counter to both Freud's doctrine of repression (see Holmes, 1974) and his theory of dreams (see Fisher & Greenberg, 1977).

3. Certain of Freud's hypotheses entail "statistical" predictions that might be tested. For example, Grünbaum (1983b) writes, Freud's hypothesis that repressed homosexuality is the specific etiologic factor for paranoia entails that

the decline of the taboo on homosexuality in our society should be accompanied by a decreased incidence of male paranoia. And by the same token, there ought to have been relatively less paranoia in those ancient societies in which male homosexuality was condoned or even sanctioned, for the reduction of massive anxiety and repression with respect to homosexual feelings would contribute to the removal of Freud's *conditio sine qua non* for this syndrome [p. 157].

PSYCHOANALYSIS AS CRITICAL THEORY

HABERMAS'S READING OF FREUD

Like Popper, Habermas wants to hold that psychoanalysis cannot appropriately be regarded as natural science. However, his attitude toward

psychoanalysis is quite different. Contrary to Popper who, as we have seen, wants to relegate Freudian theory to the epistemological dustbin of pseudoscience, Habermas seeks to make it an object of profound study. He believes that Freudian theory and practice represent a prototype (along with Marxian theory and practice) of a completely new form of knowledge—one he has chosen to designate “critical theory.” Habermas’ interest in Freud is part of a much larger concern with the nature of knowledge in general. Although I cannot here do justice to his views, it will be useful for our purposes to attempt a rough characterization of some of his basic doctrines. First, Habermas assumes there to be three fundamentally different kinds of knowledge (*Wissenschaft*): (1) *empirical-analytic sciences*, of which the natural sciences are the paradigm; (2) *historical-hermeneutic sciences*, including the humanities (*Geisteswissenschaften*) and the historical and social sciences insofar as they aim at interpretive understanding of their subject matter; and (3) *critically oriented sciences*, in which he includes psychoanalysis as well as the critique of ideology (critical social theory)¹. Each kind of knowledge is distinguished, in his view, by both the cognitive structure of its theories and the mode of “testing” appropriate to it. Note that in taking this position, Habermas is consciously going counter to one of the principal theses of the logical positivist unity of science movement, namely, that the logic of inquiry of *any* science (*Wissenschaft*) is the same.

Second, Habermas has emphasized the importance of locating knowledge in the course of human life. According to McCarthy (1978), Habermas’ central thesis

is that “the specific view points from which we apprehend reality,” the “general cognitive strategies” that guide systematic inquiry, have their “basis in the natural history of the human species” (p. 55). In particular, Habermas believes that any search for knowledge is guided by certain cognitive interests and that distinct forms of knowledge are associated with distinct cognitive interests. Thus, Habermas (1971) assumes that each of the three kinds of knowledge he distinguishes is associated with its own kind of cognitive interest: “The approach of the empirical-analytic sciences incorporates a *technical* cognitive interest; that of the historical-hermeneutic sciences incorporates a *practical* one; and the approach of critically oriented sciences incorporates the *emancipatory* cognitive interest” (p. 308). Roughly speaking, the technical interest is an interest in making use of causal knowledge of nature for the purposes of prediction and control; the practical interest is an interest in establishing reliable intersubjective understanding in ordinary language communication; and the emancipatory interest is an interest in freeing oneself from ideological delusion and establishing social or intrapsychic relations “organized on the basis of communication free from domination” (McCarthy, 1978, p. 93). Furthermore, Habermas says that the specific kind of cognitive interest associated with a specific kind of theory shapes the cognitive structure of that theory to a large extent.

Much of Habermas’ intellectual effort over the past 15 years has been devoted to elucidating and arguing for the existence of the third category of knowledge, critical theory. His first attempt to articulate the logic, methodology,

and structure of a critical theory were published in 1967 and 1971. His more recent views on the topic are to be found in *Communication and the Evolution of Society* (1979). But it is the earlier *Knowledge and Human Interests* (1971) which is of most concern to us, for it is here that Habermas' most extended treatment of Freud is to be found.

Habermas' (1971) two principal claims about Freudian psychoanalysis are stated in the opening passage of his discussion of Freud:

The end of the 19th century saw a discipline emerge, primarily as the work of a single man, that from the beginning moved in the element of self-reflection and at the same time could credibly claim legitimation as a scientific procedure in a rigorous sense....Psychoanalysis is relevant to us as the only tangible example of a science incorporating methodological self-reflection. The birth of psychoanalysis opens up the possibility of arriving at the dimension that positivism closed off....This possibility has remained unrealized. For the scientific self-misunderstanding of psychoanalysis inaugurated by Freud himself, as the physiologist that he originally was, sealed off this possibility [p. 214].

Habermas' claim that psychoanalysis involves self-reflection is, as we shall see shortly, essential to his construing it as a critical theory. "The dimension that positivism closed off" I take to be a reference to the possibility of a science existing (*Wissenschaft*) that differs in important ways from the natural sciences. Thus, Habermas is making two claims: (1) psychoanalysis is a "tangible example" of a critical theory; and (2) this fact has not been recognized because Freud himself was guilty of perpetuating a misunderstanding of his own enterprise, namely, the

mistaken view that what he was doing was empirical-analytic science rather than critical theory and practice.

Habermas attempts to argue for his first claim by providing us with a description of Freudian doctrine that makes salient its “critical” features. To understand his reading of Freud, we need to say a bit more about the aims of a critical theory. We have already noted that, for Habermas, a critical theory is essentially tied to the emancipatory interest. More specifically, it has as its aim the emancipation of the agents that make use of it by means of their self-enlightenment. We can gain a clearer picture of what this emancipation and enlightenment is supposed to come to by viewing it as a transition from an initial to a final state. Geuss (1981) characterizes these states as follows:

- (a) The initial state is one *both* of false consciousness and error, *and* of ‘unfree existence.’...
- (b) In the initial state false consciousness and unfree existence are inherently connected so that agents can be liberated from one only if they are also at the same time freed from the other....
- (c) The “unfree existence’ from which the agents in the initial state suffer is a form of self-imposed coercion; their false consciousness is a kind of self-delusion....
- (d) The coercion from which the agents suffer in the initial state is one whose ‘power’...or ‘objectivity’...derives only from the fact that the agents do not realize it is self-imposed.
- (e) The final state is one in which the agents are free of false consciousness—they have been enlightened—and free of self-imposed coercion—

they have been emancipated [p. 58].

A critical theory is supposed to achieve such enlightenment and emancipation by inducing what Habermas calls “self-reflection.” It is by reflecting, Geuss (1981) says, that the agents in question “come to realize that their form of consciousness is ideologically false and that the coercion from which they suffer is self-imposed. But, by (d) above, once they have realized this, the coercion loses its ‘power’ or ‘objectivity’ and the agents are emancipated” (p. 61).

It is not difficult to see how Freudian psychoanalysis can fit in with Geuss’ schema. The first four statements constitute a quite straightforward (if abstract) description of certain of the central features of psychoanalytic *therapy*. Thus, we find Habermas arguing his thesis “that psychoanalytic knowledge belongs to the category of self-reflection” by reference to Freud’s papers on analytic technique (see Habermas, 1971, pp. 228-236). The important point, however, is this: Because of his doctrine of cognitive interests, Habermas’ view of psychoanalytic therapy as emancipatory self-reflection has certain consequences for his reading of the psychoanalytic theory of personality. That is, because he, in effect, subordinates the theory to the therapy, he ends up representing Freud’s theoretical claims in a certain idiosyncratic way. It is not only this idiosyncratic reading of Freudian theory but also his fundamentally mistaken views about the nature of (natural) science that become the target of Grünbaum’s criticisms.

THE HABERMAS-GRÜNBAUM DISPUTE

Like most interpreters of Freud, Habermas divides Freud's theoretical claims into two parts: the metapsychology and the clinical theory. As I read him (which is not always a straightforward matter), in arguing that Freud was guilty of misunderstanding his own enterprise, Habermas provides us with two sets of arguments to the effect that Freudian psychoanalysis cannot correctly be regarded as an empirical-analytic science. The first of these considers the relationship of the clinical theory to the metapsychology; the second considers the scientific characters of the clinical theory itself.

Habermas begins by arguing that Freud took psychoanalysis to be scientific because psychoanalytic assumptions could be "reformulate[d]...in the categorical framework of a strictly empirical science" (p. 252), namely, the energy model of the metapsychology. That is, he attributes to Freud two beliefs: first, that the clinical theory could be "reduced" to the metapsychology, and, second, that the metapsychology was a "strictly empirical science." With respect to the second point, Habermas (1971) writes: "Freud surely assumed tacitly that his metapsychology, which severs the structural model from the basis of communication between doctor and patient and instead attaches it to the energy-distribution model by means of definitions, represented an empirically rigorous scientific formulation of this sort" (p. 253). However, in Habermas' view, Freud "erred" in adopting this reductionistic approach, because "psychology, insofar as it understands itself as a strict empirical science, cannot content itself with a model that keeps to a physicalistic use of language without seriously leading to

operationalizable assumptions” (p. 253). That is, the metapsychology is not genuinely scientific *unless* its underlying energy model is operationalizable. But, Habermas continues, this is not the case:

The energy-distribution model only creates the semblance that psychoanalytic statements are about measurable transformations of energy. Not a single statement about quantitative relations derived from the conception of instinctual economics has ever been tested experimentally. The model of the psychic apparatus is so constructed that metapsychological statements imply the observability of the events they are about. But these events are never observed—*nor can they be observed* [p. 253; italics added].

Grünbaum’s first point against Habermas effectively undercuts this whole line of argumentation. For, according to Grünbaum (1984), careful examination of the Freudian text (Freud, 1914, p. 77; 1915a, p. 117, 1925, p. 32) reveals clearly

that when Freud unswervingly claimed natural science status for his theoretical constructions throughout his life, he did so first and foremost for his evolving clinical theory of personality and therapy, rather than for the metapsychology. For he had been chastened in his early reductionistic exuberance by the speedy demise of his Project. And, once he had repudiated his ephemeral neurobiological model of the psyche after 1896, he perennially saw himself entitled to proclaim the scientificity of his clinical theory *entirely on the strength of a secure and direct epistemic warrant from the observations he made on his patients and on himself*. In brief, during all but the first few years of his career Freud’s criterion of scientificity was *methodological* and *not* ontologically reductive, (p. 6)

The consequence of Grünbaum's exegetical position here is that he simply passes over Habermas' first set of arguments, presumably on the grounds that they are simply irrelevant to the issue at hand. Implicitly, Grünbaum's reasoning seems to be something like this: When Freud claimed that psychoanalysis was scientific, what he chiefly had in mind was that the clinical theory was scientific. And since the status of the clinical theory does not depend in any essential way on the status of the metapsychology, any argument that assumes that the scientificity of the clinical theory *depends* on that of the metapsychology is irrelevant to the question of whether the theory in general is scientific. Grünbaum therefore turns his attention to Habermas' second set of arguments.

To be in a position to understand this second set of arguments and to appreciate Grünbaum's replies, we must briefly consider Habermas' conception of the clinical theory. The standard reading of Freud is that the clinical theory consists of a large number of universal generalizations about the human psyche. Habermas' view is somewhat different. Rather than viewing the theory of psychosexual development, say as a set of universal claims about the ontogenesis of human personality, Habermas (1971) takes it to consist of a set of *narrative schemata*. He writes:

A general interpretation...has the form of a narrative, because it is to aid subjects in reconstructing their own life history in narrative form. But it can serve as the background of *many* such narrations only because it does not hold merely for an individual case. It is a *systematically generalized history*, because it provides a scheme for many histories with foreseeable

alternative courses [p. 263],

Furthermore, in keeping with his (misplaced) emphasis on the centrality of the therapy to the psychoanalytic enterprise as a whole, Habermas takes the primary function of Freud's general interpretations to be their role in self-reflection. For it is by the application of such general interpretations to the individual case that patient and physician together create the interpretative *constructions*, by means of which the self-reflective process takes place. Habermas (1971) states:

Only the...*systematically generalized history* of infantile development with its typical developmental variants puts the physician in the position of so combining the fragmentary information obtained in analytic dialogue that he can reconstruct the gaps of memory and hypothetically anticipate the experience of reflection of which the patient is at first incapable [p. 260].

On the basis of this rather one-sided conception of Freud's clinical theory, Habermas offers us a number of arguments that the clinical theory ought not to be regarded as science of the empirical-analytic sort. I label these "the argument from therapeutic application," "the argument from explanation," and "the argument from validation."

1. *The Argument from Therapeutic Application.* I pointed out earlier that, in Habermas' view, empirical-analytic theories are always associated with a technical interest in manipulating nature. The argument from therapeutic application relies heavily on the further assumption that such manipulation

always occurs by means of the exploitation of causal laws. We can reconstruct the argument as follows:

1. If psychoanalytic theory were scientific (empirical-analytic), its application would consist in the manipulation of its domain by the exploitation of causal laws.
2. The application of psychoanalytic theory consists in the doing of psychoanalytic therapy.
3. However, psychoanalytic therapy does not work by the exploitation of causal laws; rather “it owes its efficacy to overcoming causal connections themselves” (Habermas, 1971, p. 271).
4. Thus, psychoanalytic theory cannot be scientific.

Habermas (1971) defends the key third premise as follows:

Psychoanalysis does not grant us a power of technical control over the sick psyche comparable to that of biochemistry over a sick organism. And yet it achieves more than a mere treatment of symptoms, because it certainly does grasp causal connections, although not at the level of physical events—at a point “which has been made accessible to us by some very remarkable circumstances” [Freud, 1971, p. 436], This is precisely the point where language and behavior are pathologically deformed by the causality of split-off symbols and repressed motives. Following Hegel we can call this the causality of fate, in contrast to the causality of nature. For the causal connection between the original scene, defense, and symptom is not anchored in the invariance of nature according to natural laws but only

in the spontaneously generated invariance of life history, represented by the repetition compulsion, which can nevertheless be dissolved by the power of reflection [p. 271].

Habermas' point seems to be that the power of reflection can "overcome" the causal connections responsible for the patient's neurosis, because these causal connections are of a different sort than those posited by the empirical-analytic sciences. They constitute the "causality of fate" rather than the "causality of nature." What Habermas has in mind by this term is far from clear, although I suspect that it is, in some way, a consequence of his reading of the clinical theory as consisting of narrative schemata. Whatever it is, however, it is irrelevant. For, as Grünbaum argues, the kind of causality avowed by psychoanalytic etiologic and therapeutic theory does not permit this kind of "dissolution." In addition, careful examination of the causal assertions made by the theory exhibits the complete folly of this sort of talk of dissolution. In other words, Habermas has a case only by blatantly misconceptualizing psychoanalytic theory.

To be more precise, Habermas' account, in Grünbaum's view, "*flatly repudiates* the psychoanalytic *explanation* for the patient's therapeutic transition from unconsciously driven behavior to more consciously governed conduct" (Grünbaum, 1984, p. 10). This psychoanalytic explanation, first articulated in Breuer and Freud's (1893, pp. 6-7) "Preliminary Communication," rests on the etiological principle that repression is *causally necessary* not only for the initial development of a neurotic disorder, but also for its maintenance. The explanation

of why therapy is efficacious then is as follows:

1. Repression of type R is the causal sine qua non of a neurosis of kind N.
2. Therapy largely consists of ridding the patient of R.
3. Therefore, therapy has the effect of obliterating N.

Grünbaum points out that, in this explanation, *therapy involves the instantiation or exemplification* of the etiologic causal relationship rather than its dissolution. For it is precisely because after the fulfillment of the second condition the patient no longer satisfies the sine qua non state that the symptoms are claimed (predicted) to disappear. Paradoxically, Habermas appears to accept both the etiological principle and the explanation; thus, he is guilty not only of contradicting the foundational postulate of Freudian theory but also of confusing the dissolution of the neurosis with the dissolution of its causal link to its original pathogen.

To further bring home his objection, Grünbaum (1984, p. 14) offers us a *reductio ad absurdum* argument to show that if Habermas' reasoning were legitimate, then thermal elongation in physics could also be shown to rest on the dissolution rather than the instantiation of a causal law:

For consider a metal bar that is isolated against all but thermal influences. It is subject to the law $\Delta L = \alpha \Delta T \cdot L_0$, where L_0 is its length at the fixed standard temperature, ΔT the length increase or decrease due to this

temperature change, and a the coefficient of linear thermal expansion characteristic of the particular material composing the metal bar. Now suppose that the bar, initially at the standard temperature, is subjected to a “pathogenic” temperature increase ΔT , which produces the elongation ΔL as its “pathological” effect. In addition to supplying this “aetiology,” the law of linear thermal elongation also provides a basis for a corresponding “therapy”: It tells us that if the bar’s temperature is reduced to its “healthy” standard value, the “pathological” effect ΔL will be wiped out. Thus, we can correlate the “therapeutic intervention” of temperature reduction with the patient’s remedial lifting of his own repressions. Similarly, we correlate the bar’s “neurotic symptom” ΔL with the patient’s repetition compulsion.

By parity with Habermas’ reasoning, we could then draw the following ludicrous conclusion: When the temperature reduction “therapeutically” wiped out the endurance of the “pathological” effect ΔL generated by the “pathogenic” temperature increase, this thermal termination also “dissolved” the stated law of thermal elongation.

What is overcome here is clearly the “pathological” effect, *not* the causal connection itself. And the same is true, according to Grünbaum, in the psychoanalytic case (that is, assuming the Freudian story is correct, as Habermas does). In sum, Habermas’ claim that psychoanalytic therapy owes its efficacy to “overcoming causal connections” rather than “making use” of them is totally unsubstantiated.

2. *The Argument from Explanation.* Habermas’ (1971) second argument concerns the kind of explanation that results from the application of Freud’s clinical theory to a specific case:

In its logical form...explanatory understanding differs in one decisive way from explanation rigorously formulated in terms of the empirical sciences.

Both of them have recourse to causal statements that can be derived from universal propositions by means of supplementary conditions: that is, from derivative interpretations (conditional variants) or lawlike hypotheses. Now the content of theoretical propositions remains unaffected by operational application to reality. In this case we can base explanations on context-free laws. In the case of hermeneutic application, however, theoretical propositions are translated into the narrative presentation of an individual history in such a way that a causal statement does not come into being without this context...Narrative explanations differ from strictly deductive ones in that the events or states of which they assert a causal relation is [sic] further defined by their application. Therefore general interpretations do not make possible context-free explanations [pp. 272-273].

The passage is somewhat confusing because Habermas uses the term 'theoretical propositions' in both a narrow and broad sense. I assume that the first reference to such theoretical propositions is meant to refer to the theoretical propositions of empirical-analytic science, whereas the second reference includes also those that can have a "hermeneutic application." Given this reading, the basic structure of the argument seems to be the following:

1. The explanation of a particular phenomenon by means of the causal laws of an empirical-analytic science always results in a "context-free" explanation.
2. However, this is not the case for the application of the general interpretations of psychoanalytic theory; "general interpretations do not make possible context-free explanations."

3. Therefore, these general interpretations cannot be part of an empirical-analytic science.

Recall that, in responding to Habermas' first argument, Grünbaum took issue with Habermas' grasp of Freudian theory, in particular, his failure to see that the therapeutic conquest of a neurosis *instantiates* rather than dissolves its etiologic linkage to its pathogen. In this case, he objects that Habermas relies on a false view of natural science. In particular, Grünbaum offers an array of counterexamples from physics to the first premise of our reconstruction of Habermas' argument. In Grünbaum's view, Habermas is simply wrong that explanations in the natural science are never context dependent; thus, this cannot be used as a reason for distinguishing the Freudian enterprise from that of natural science. In arguing his point, Grünbaum (1984) again draws on his knowledge of physics, specifically, the physical theory of classical electrodynamics. He writes: "For that major physical theory features laws that embody a far more fundamental dependence on the history and/or context of the object of knowledge than was ever contemplated in even the most exhaustive of psychoanalytic explanatory narratives..." (p. 17; for a briefer version of this argument, see Grünbaum, 1983c). Grünbaum's (1984) specific counterexample is the following:

Consider an electrically charged particle having an arbitrary velocity and acceleration. We are concerned with the laws governing the electric and magnetic fields produced by this point charge throughout space at any one fixed time t . In this theory, the influence of the charge on any other test charge in space is postulated to be propagated with the finite velocity of

light rather than instantaneously, as in Newton's action-at-a-distance theory of gravitation. But this *non*-instantaneous feature of the propagation of the electrodynamic influence contributes to an important consequence as follows: At any space point P , the electric and magnetic fields at a given time t depend on the position, velocity and acceleration that the charge had at an earlier time t_0 . That earlier time has the value $t - r/c$, where r is the distance traversed by the influence arriving at P at time t after having traveled from the charge to P with the velocity c of light.

Clearly, the greater the distance r that was traversed by the influence by the time t of its arrival at point P , the earlier its origination time t_0 . Thus, for space points at ever larger such distances r in infinite space, the origination time $t_0 = t - r/c$ will be ever more remotely past. In short, as the distance r becomes infinitely large, the origination time goes to past infinity.

It follows that at ANY ONE INSTANT t the electric and magnetic fields produced throughout infinite space by a charge moving with arbitrary acceleration depend on its own PARTICULAR ENTIRE INFINITE PAST KINEMATIC HISTORY! (p. 17).

This is not at all a unique case, according to Grünbaum. There are other cases that exhibit "hysteresis" in the sense that "a property of a physical system induced by a given present influence upon it depends not only on that present influence, but also on the *past history* of variation of that influence" (Grünbaum, 1984, p. 18; see also 1983c for a briefer discussion). These cases include the hysteresis behavior of highly magnetizable metals (e.g., iron, cobalt, nickel, etc.), the *elastic* hysteresis of certain solids, the electric hysteresis exhibited by dielectric substances in electric fields, and the hysteresis of a radiation counter tube. Even rubber bands exhibit like behavior, and metal fatigue in airplanes is a

similar phenomenon. These cases clearly show, in Grünbaum's view, that some of the important laws of nature, and, hence, any explanation that makes use of them, exhibit context dependence. On the basis of these considerations, Grünbaum's (1983c) summary judgment of Habermas' second argument is a harsh one: Habermas (as well as Gadamer (1975) who echoes Habermas' view) have simply succeeded in "parlay[ing] the severe limitations of their own personal scientific horizons into a *pseudo*-contrast between the humanistic disciplines and the natural sciences" (p. 11).

3. *The Argument from Validation*. What I call "the argument from validation" consists of two subarguments—one concerning supposed differences between how psychoanalytic theory and empirical-analytic theories are confirmed; the other concerning how they are disconfirmed. The first subargument rests on the fact that, according to Habermas, there is the following "specific difference" between empirical-analytic theories and the general interpretations of psychoanalysis (that is, Freud's clinical theory):

In the case of testing theories through observation...the application of assumptions to reality is a matter for the inquiring subject. In the case of testing general interpretations through self-reflection...this application becomes *self-application* by the object of inquiry, who participates in the process of inquiry. The process of inquiry can lead to valid information only via a transformation in the patient's self-inquiry. When valid, general interpretations hold for the inquiring subject and all who can adopt its position only to the degree that those who are made the object of individual interpretations *know and recognize themselves* in these interpretations. The subject cannot obtain knowledge of the object unless

it becomes knowledge for the object—and unless the latter thereby emancipates itself by becoming a subject [pp. 261-262].

I take it that Habermas is here assuming that the general interpretative schemata of the clinical theory are confirmed only to the extent that they are inductively supported by valid individual constructions. The claim, then, is that the latter are confirmed, in turn, *only* if they become a part of the self-reflection of the analysand. That is, the analysand has, as Grünbaum puts it, complete “epistemic privilege” with respect to these constructions, even as against the analyst him- or herself. In contrast, according to Habermas, the objects of standard empirical-analytic inquiry do not have this kind of epistemic privilege. Here confirmation occurs on the basis of observations of the object by the scientist (the so-called subject of inquiry). We can reconstruct the argument thus:

1. Statements relevant to the confirmation of clinical psychoanalytic theory (for example, individual constructions) can be accepted by the researcher only if they have first been accepted as valid by the subject.
2. No such requirement holds for statements relevant to the confirmation of empirical-analytic theories, which are typically accepted on the basis of observation by the researcher.
3. Therefore, clinical psychoanalytic theory is not an empirical-analytic theory.

In replying to this argument, Grünbaum again attacks Habermas' conception of psychoanalysis, this time on the grounds that the thesis of privileged epistemic access expressed in Habermas' first premise is ill-founded. His first point is that the only argument Habermas supplies for his first premise is a question-begging one. For in the above quote, Habermas construes the "otherwise innocuous phrase 'testing through self-reflection' so as to *stipulate* that only the patient's own appraisal can carry out the application of general interpretations to his particular life situation." (Grünbaum, 1984, p. 23) Second, the epistemic privilege that Habermas assigns to the analysand does not accord with Freud's own views concerning when an individual construction ought to be regarded as true. In particular, Freud (1937) explicitly rejects recollection by the patient as essential.

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 [the patient] an assured conviction of the truth of the construction which achieves the same therapeutic result as a recaptured memory [pp. 265-266].

Habermas might reply at this point that perhaps he was wrong about the need for recollection; however, this quote from Freud shows that the patient's *conviction* is necessary, which is enough to maintain some form of an epistemic privilege doctrine. This reply is inadequate, however. For, as Grünbaum also points out, in Freud's paper on "Constructions in Analysis" (1937), he argues (from the confluence of clinical induction) that the analyst could justify an individual construction on the basis of the totality of the patient's productions,

even in the face of the patient's denial. (See also Freud, 1920, on the treatment of a young lesbian as a case in point.) Finally, Grünbaum points out that Habermas' attribution of epistemic privilege to the analysand has also been impugned by the contemporary psychoanalysts Thomä and Kächele (1973, pp. 315-316) and Eagle (1973) as being untrue to the psychoanalytic situation (treatment setting).

One might expect the subargument from disconfirmation to run exactly parallel to that from confirmation. That is, one might expect Habermas to argue that the difference between the disconfirmation of psychoanalytic theory and empirical-analytic theory is that the former relies on the failure of self-reflection whereas the latter relies on the failure of observable prediction. But this is not the case for the following reason: Although Habermas regards the acceptance of a construction C by the analysand during self-reflection to be sufficient for the correctness of C, the *absence* of self-reflection in the face of C does not falsify it. The patient's resistances might simply be too strong. Thus, Habermas focuses instead on the logic of disconfirmation in the two cases and claims that there is a fundamental contrast between them on the purported grounds that an unsuccessful prediction in the natural sciences automatically refutes the hypothesis used to make it. In fact, Habermas (1971) takes the existence of an alternative to disconfirmation in the face of apparently disconfirming evidence to be the distinguishing feature of the psychoanalytic case. He argues as follows:

General interpretations do not obey the same criteria of refutation as general theories. If a conditional prediction deduced from a lawlike

hypothesis and initial conditions is falsified, then the hypothesis may be considered refuted. A general interpretation can be tested analogously if we derive a construction from one of its implications and the communications of the patient. We can give this construction the form of a conditional prediction. If it is correct, the patient will be moved to produce certain memories, reflect on a specific portion of forgotten life history, and overcome disturbances of both communication and behavior. But here the method of falsification is not the same as for general theories. For if the patient rejects a construction, the interpretation from which it has been derived cannot yet be considered refuted at all...[T]here is still an alternative: either the interpretation is false (that is, the theory or its application to a given case) or, to the contrary, the resistances, which have been correctly diagnosed, are too strong [p. 266].

But, as Grünbaum points out, it has become a commonplace of the philosophy of science, ever since Pierre Duhem's work before World War I, that precisely the same ambiguity of refutation holds for science in general. By and large, it is not theories alone that are at issue in prediction but theories in conjunction with a statement of initial conditions and various collateral hypotheses. This means that if a prediction is not borne out, the blame cannot be pinned on the theory with certainty. Thus, again, the alleged difference between psychoanalysis and empirical-analytic science rests on a false view of the latter.

IS FREUDIAN PSYCHOANALYSIS GOOD SCIENCE?

In considering the merits of psychoanalytic theory as a scientific theory, Grünbaum has been concerned with the extent to which Freud's theoretical claims are supported by the available evidence. He has focused, in particular, on the sort

of evidence that Freud invoked, namely, evidence obtained “from the couch.” In making his assessment, Grünbaum has relied both on logical considerations and on various canons of inductive support that have become standard since the time of Bacon. He makes three basic claims:

1. The therapeutic effectiveness of the characteristic constituent factors of Freudian psychoanalytic therapy is in serious question.
2. Clinical data are subject to so many epistemological liabilities as to render them virtually useless in supporting the cardinal hypotheses as Freudian theory.
3. Even if clinical data were not epistemologically contaminated and could be taken at face value, they would fail to sustain any of the central postulates of Freud’s clinical theory as well as the investigative utility of the method of free association.

Let us consider each of these claims in turn.

THE QUESTION OF THERAPEUTIC EFFECTIVENESS

The effectiveness of Freudian therapy has been under attack at least since Eysenck (1952, 1966) published his classic challenge. Contending that available evidence does not adequately support the claim that psychoanalysis is

therapeutically effective, Eysenck claimed to have telling evidence that psychoanalysis did no better than simply having people go on about their lives without therapy. Erwin (1980) has reconstructed Eysenck's argument as follows:

1. If there is no adequate study of psychoanalytic therapy showing an improvement rate of better than two thirds or better than that of a suitable no-treatment control group, then there is no firm evidence that the therapy is therapeutically effective.
2. There is no adequate study showing either rate of improvement.
3. Therefore, there is no firm evidence that the therapy is therapeutically effective.

Originally, Eysenck made use of an overall spontaneous remission rate across all varieties of neurotic disorder. In response to criticism, however, Eysenck (1977) has recently emphasized that different types of neurotic disorder have different incidences and/or time courses of spontaneous remission. He now claims that any comparative evaluation must focus on a particular diagnostic grouping and a diagnostically matched untreated control group.

In the light of much subsequent literature, Grünbaum proceeds on the assumption that the superiority of the outcome of analytic treatment over that of rival treatment modalities has not been demonstrated. However, in his essay,

“How Scientific is Psychoanalysis?” (Grünbaum, 1977), he stresses the following additional fact which is frequently overlooked: If psychoanalytic treatment outcomes do exceed the spontaneous remission rate, this alone does not suffice to establish that psychoanalytic treatment gains are due to mediation of analytic insight. It would not rule out an important rival hypothesis, namely, that such treatment gains are due to an *inadvertent placebo effect*. In defining this term, Grünbaum (1981, 1983a) notes that of the various constituent factors that make up a treatment process, we can distinguish those that are characteristic, that is, claimed by the theory to be remedial, from others it regards as incidental. Grünbaum (1980) continues:

A treatment process *t* characterized by having constituents *F*, will be said to be an *inadvertant placebo* with respect to target disorder *D* and dispensing physician *P* just in case each of the following conditions is satisfied: (a) none of the characteristic treatment factors *F* are remedial for *D*, but (b) *P* credits these very factors *F* with being therapeutic for *D* and indeed he deems at least some of them to be causally *essential to the remedial efficacy of t*, and (c) the patient believes that *t* derives remedial efficacy for *D* from constituents belonging to *t*'s *characteristic factors* [p. 330].

The point is that in assessing the effectiveness of psychoanalytic therapy or of any of its rivals, “one must try to disentangle from one another (i) the effects, if any, indeed due to those factors that the relevant therapeutic theory postulates as being genuinely remedial, and (ii) purportedly lesser changes due to the expectations aroused in both patients and physicians by their belief in the therapeuticity of the treatment” (Grünbaum, 1977, p. 238). As Grünbaum reads

the relevant literature on treatment effectiveness (in particular, Fisher & Greenberg, 1977; Luborsky, Singer, & Luborsky, 1975; Meltzoff & Komreich, 1970; Sloan et al., 1975), there is good reason to suspect that insofar as Freudian therapy is effective, it is, in fact, “placebogenic.” The studies seem to point to two conclusions; (1) psychotherapy of a wide variety of types and for a broad range of disorders is better than nothing, but (2) there is either no difference between different treatment modalities or the behavioral treatment is better.

EPISTEMOLOGICAL LIABILITIES OF CLINICAL DATA

Eysenck (1963) not only impugned the effectiveness of Freudian therapy, he also raised serious questions about the epistemic validity of clinical data as had Wilhelm Fliess (see Freud, 1954) before him. In contrast, Freud himself, as well as most of his advocates (see Luborsky & Spence, 1978, for a recent statement) have regarded clinical evidence as *the* basis for the claims of psychoanalytic theory to truth.

In considering how clinical material is supposed to bear evidentially on Freudian theory, it is important to distinguish three levels of clinical material. At the lowest level, we have what we can call the patient’s *productions*. These include their dream reports, slips of the tongue, memory reports, and free associations as well as assents or dissents to interpretations offered by the analyst. In addition, we have facts concerning the presence or absence of behaviors or bodily states

that are regarded as symptoms. At the second level, we have the *interpretations* provided either by the analyst or by patients themselves of these productions and symptoms as expressions of unconscious wishes, resistance, and so forth. Finally, we have what Freud (1937) later called a *construction*, a whole psychoanalytic story about the patient's psyche from the patient's early infantile history to the present state, including, of course, an etiological account of the symptoms. Although it is possible to maintain that the patient's productions bear directly on Freud's universal theoretical claims, a more plausible epistemological reconstruction is roughly as follows: Most productions, such as dream reports, slips of the tongue, free associations, and expressions of feeling toward the analyst during transference are taken to be relevant insofar as they provide the raw material for interpretations, which, in turn, provide the building blocks for the ultimate construction. Some productions may also be taken to attest to therapeutic success. In contrast, others, such as the patient's assent to or protest against a proposed construction, are often taken as direct evidence for the truth of that construction. The constructions themselves, clearly, are supposed to bear on the theory in the way that a particular instantiation of a universal claim bears on the universal claim.

The principal epistemological liability to which clinical data are subject is that the analyst, who presumably is committed to the truth of Freudian theory, unwittingly influences both patients' productions and the course of the analysis. This point has been recognized for some time (by Fliess, as is clear from Freud,

1954; as well as Christiansen, 1964; Glover, 1952; Martin, 1964a; Nagel, 1959). What appears not to have been recognized, as Grünbaum (1983b) points out, is that Freud himself was aware of this problem and, in addition, had a very sophisticated, albeit unsuccessful, strategy for dealing with it. Freud (1917) acknowledges the so-called problem of suggestion in his *Introductory Lectures*:

It must dawn on us that in our technique we have abandoned hypnosis only to rediscover suggestion in the shape of transference.

But here I will pause, and let you have a word; for I see an objection boiling up in you so fiercely that it would make you incapable of listening if it were not put into words: "Ah! so you've admitted it at last! You work with the help of suggestion, just like the hypnotists! That is what we've thought for a long time. But, if so, why the roundabout road by way of memories of the past, discovering the unconscious, interpreting and translating back distortions—this immense expenditure of labour, time and money—when the one effective thing is after all only suggestion? Why do you not make direct suggestions against the symptoms, as the others do—the honest hypnotists? Moreover, if you try to excuse yourself for your long detour on the ground that you have made a number of important psychological discoveries which are hidden by direct suggestion—what about the certainty of these discoveries now? Are not they a result of suggestion too, of unintentional suggestion? Is it not possible that you are forcing on the patient what you want and what seems to you correct in this field as well?" [pp. 446-447].

By this time in his career, Freud had clearly recognized the importance of transference as a motive force in therapy. Thus, the challenge was that, as Freud (1917) so nicely put it, "what is advantageous to our therapy is damaging to our researches" for "the influencing of our patient may make the objective certainty of our findings doubtful" (p. 452). His reply was as follows:

Anyone who has himself carried out psycho-analyses will have been able to convince himself on countless occasions that it is impossible to make suggestions to a patient in that way. The doctor has no difficulty, of course, in making him a supporter of some particular theory and in thus making him share some possible error of his own. In this respect the patient is behaving like anyone else—like a pupil—but this only affects his intelligence, not his illness. *After all, his conflicts will only be successfully solved and his resistance overcome if the anticipatory ideas he is given tally with what is real in him* [italics added]. Whatever in the doctor's conjectures is inaccurate drops out in the course of the analysis; it has to be withdrawn and replaced by something more correct (p. 452).

Grünbaum has dubbed the underlined statement the “necessary condition thesis,” NCT for short. (Elsewhere, Grünbaum, 1983c, calls it—more honorifically —“Freud’s master proposition”.) This assertion plays the key role in Freud’s attempted solution to the problem of suggestion. What he is claiming, according to Grünbaum (1983c), is tantamount to the following: “(1) only the psychoanalytic method of interpretation and treatment can yield or mediate to the patient correct insight into the unconscious pathogens of his psychoneurosis, and (2) the analysand’s correct insight into the etiology of his affliction and into the unconscious dynamics of his character is, in turn, *causally necessary* for the therapeutic conquest of this neurosis” (p. 184). NCT can then be used to vindicate the validity of the clinical data furnished by patients in analysis by means of what Grünbaum dubs the “tally argument” (referring to Freud’s assumption that ideas given patients *tally* with what is real in them). The argument runs as follows:

1. The analysis of patient P was therapeutically successful.

2. NCT.

3. Therefore, the psychoanalytic interpretations of the hidden causes of Ps behavior given to him by his analyst are indeed correct.

Freud's strategy was brilliant, according to Grünbaum. But was it successful? It should be clear from our discussion of the therapeutic efficacy question, that Grünbaum does not think so. For, although the tally argument is logically valid, there is a serious question concerning the truth of its premises, in particular, the crucial NCT. NCT claims that therapeutic success is mediated *only* by psychoanalytic insight. Insofar as there is either spontaneous remission of symptoms or there exist rival successful treatment modalities, NCT is false. As Grünbaum (1980a) argues—after extensive review of the relevant literature—there appears to be strong evidence for both. (Interestingly enough, Freud himself explicitly conceded the existence of spontaneous remission [Grünbaum, 1983c]). Grünbaum (1983b) concludes: “Since the Tally Argument is thus gravely undercut, any therapeutic successes scored by analysts, even if spectacular, have become *probatively* unavailing to the validation of psychoanalytic theory via that argument” (p. 208).

Grünbaum (1980a) considers one possible alternative to the use of the tally argument. This is to make use of a patient's introspections *once he or she has been successfully analyzed*. It might be thought that, if reliable, such introspections

could provide the needed validation for two sorts of claims: (1) claims concerning the etiology of the patient's affliction, and (2) claims concerning the necessary role of the analyst's constructions in the therapeutic process. The validation of such claims could, in turn, provide direct evidence for Freud's psychogenetic theory as well as help to discredit the rival therapeutic hypothesis of placebogenesis. Unfortunately, however, these "hopeful speculations" are "fundamentally impugned" in Grünbaum's view by the findings reported by Nisbett and Wilson (1977) on the extent to which we have introspective access to the dynamics of our mental life. Nisbett and Wilson do not apply the results of their findings to the case of psychoanalysis. Grünbaum (1980a) believes, however, that they are directly relevant and that "they marshal telling empirical support" for the following conclusions:

1. Far from justifying the prevalent belief in privileged access to the dynamics of our psychic responses, the findings strongly indicate the following: Purportedly introspective self-perception of causal connections between one's own mental states is just as liable to theory-induced errors as is drawing causal inferences about connections between purely external events from apparent covariations among their properties....
2. When asked how, if at all, a particular stimulus influenced a given response, the persons in the experimental studies, and ordinary people in their daily lives did not and do not even attempt to interrogate their memories of the mediating causal process. Although it may *feel* like introspection, what they actually do is draw on the causal *theories* provided by their culture or pertinent intellectual subculture for a verdict as to the effect, if any, of that kind of stimulus on that kind of response....

3. As N & W remark: "Subjective reports about higher mental processes are sometimes correct, but even the instances of correct report are not due to direct introspective awareness. Instead, they are due to the incidentally correct employment of a priori causal theories" [Nisbett & Wilson, 1977, p. 233] [p. 363-364].

(See Rothstein, 1980, for some criticisms of Grünbaum's discussion of the epistemological liabilities of patient introspection and Grünbaum, 1981, for a reply.)

Grünbaum's point, then, is that neither the tally argument nor the use of patients' introspective judgments subsequent to successful analysis can be used to guard against the very real possibility that both patients' productions and therapeutic outcomes are due more to the suggestive influence of the analyst than to the causal mechanisms and states of affairs posited by Freudian theory. Grünbaum (1983b) considers the suggestion hypothesis to be more than a mere logical possibility. He discusses in detail three of the major kinds of clinical findings that Freud deemed either initially exempt from contamination or, at least, unmarred when gathered with proper precautions. These are the products of "free" association, the patient's assent to analytic interpretations that were initially resisted, and memories recovered from early life. Grünbaum finds "solid" evidence in the psychological literature that each of these instances is subject to "considerable epistemic contamination." Grünbaum (1983b) concludes:

Thus, generally speaking, clinical findings—in and of themselves—forfeit the probative value that Freud claimed for them, although their potential heuristic merits may be quite substantial. To assert that the contamination

of intraclinical data is *ineradicable* without extensive and essential recourse to *extraclinical* findings is *not*, of course, to declare the automatic falsity of any and every analytic interpretation that gained the patient's assent by means of prodding from the analyst. But it *is* to maintain—to the great detriment of intraclinical testability!—that, in general, the epistemic devices confined to the analytic setting cannot reliably *sift* or decontaminate the clinical data so as to *identify* those that qualify as probative [p. 270].

THE LOGICAL FOUNDATIONS OF THE THEORY OF REPRESSION

The problem of the contamination of clinical evidence is not the only epistemic problem to which Freudian theory is subject. In his most recent work, Grünbaum (1983b, 1984) has charted a number of further, even more serious, difficulties, the upshot of which is that *even if clinical data could be taken at face value, they would not support the basic tenets of Freud's theoretical structure.*

Grünbaum argues for this conclusion by considering the reasoning that Freud used at various stages of his career to support “the cornerstone” of his theoretical edifice. This is the hypothesis that it is *repressed material* that initially causes and continues to maintain psychoneurotic symptoms as well as other psychic phenomena such as dreams and parapraxes. Grünbaum begins by considering the reasoning used by Freud and Breuer to support the original version of this “repression hypothesis” for psychoneurosis. Although the evidence they adduced to support their theory was not completely unflawed, it did come up to a relatively high standard, according to Grünbaum. As it turned out, Freud

himself discovered that this evidence was spurious. Rather than abandoning the repression hypothesis at this point, however, Freud substituted a new version. The difficulty with this—and the basis of Grünbaum’s complaint—is that Freud never succeeded in providing new evidence that was anywhere near as cogent as his original observations with Breuer. In addition, he proceeded to extrapolate from his repression hypothesis of the psychoneuroses to a more general repression hypothesis covering both parapraxes and dreams. But in neither case was there any new, compelling, evidence that would warrant the extrapolation.

The original Freud-Breuer hypothesis was that (1) the therapeutic conquest of hysterical symptoms is effected by the abreactive lifting of the repression of a traumatic memory, and (2) this posited therapeutic efficacy can be explained deductively by the etiologic hypothesis that the repression of the traumatic event was causally necessary for the formation and maintenance of the given hysterical symptom. Freud and Breuer’s (1893) evidence for these claims was that “each individual hysterical symptom immediately and permanently disappeared when we had succeeded in bringing clearly to light the memory of the event by which it was provoked and in arousing its accompanying affect” (p. 6; emphasis in original). Grünbaum (1983b) reconstructs their reasoning as follows:

First, they attributed their positive therapeutic results to the lifting of repressions. Having assumed such a *therapeutic connection*, they wished to *explain* it. Then they saw it would indeed be explained deductively by the following etiological hypothesis: the particular repression whose undoing removed a given symptom S is *causally necessary* for the initial formation

and maintenance of S. Thus, the nub of their inductive argument for inferring a repression etiology can be formulated as follows: the removal of a hysterical symptom S *by means of lifting* a repression R is *cogent evidence* that the repression R was *causally necessary* for the formation of the symptom S [p. 218].

The beauty of their appeal to separate symptom removal was this. To support their hypothesis, Freud and Breuer had to show that removal of the repression was *sufficient* for the removal of the symptom. This would count as cogent inductive grounds for the claim that the repression was a causally *necessary condition* of the symptom. The difficulty was that given merely the conjunction of removal of the repression with removal of the symptom, there was a rival explanation—namely, that the therapeutic efficacy of the cathartic method was placebogenic. But, they argued, if the symptom removal were a placebo effect wrought by suggestion, one would expect all the symptoms to be removed at once. Thus, in their view, the fact that they were removed one by one was evidence against the rival placebo hypothesis and in support of their own view.

Although Freud and Breuer deserve considerable credit for realizing the importance of the alternative rival placebo hypothesis and attempting to rule it out, their line of reasoning was not totally successful, in Grünbaum's (1983a) view. Precisely because of the analyst's evident focus on a specific memory for *each* symptom, the patient's conquest of the given symptom might be affected by suggestion. That is, on the basis of the analyst's behavior, the patient might come to *believe* that uncovering a memory associated with a given symptom would

cause that symptom to disappear. Thus, *as a consequence of this belief*, rather than the state of affairs posited by the Freud-Breuer hypothesis, the symptom might then actually disappear.

As it turned out, the Freud-Breuer hypothesis had a far more serious problem to contend with—namely, that the crucial evidence concerning therapeutic success was spurious. As Freud (1925) put it:

Even the most brilliant [therapeutic] results were liable to be suddenly wiped away if my personal relation with the patient became disturbed. It was true that they would be reestablished if a reconciliation could be effected; but such an occurrence proved that the personal emotional relation between doctor and patient was after all stronger than the whole cathartic process [p. 27].

Freud, however, continued to maintain a version of the repression hypothesis, substituting repression of infantile sexual wishes for the Freud-Breuer repression of a traumatic event in adulthood and calling on the full array of clinical material in support of his claims.

We have already seen that, according to Grünbaum, this clinical material is probatively hopeless because of the failure of the tally argument to protect against the ever-present problem of suggestion and because of the unavailability of any other vindication of the probity of clinical data. Let us suppose, however, that this is not the case. In the face of the demise of the *therapeutic* vindication of the tally argument, can clinical data nevertheless provide the support that Freudian theory

so badly needs *without* relying on *therapeutic* success? According to Grünbaum, the answer is no; Freud's clinical data suffer from serious epistemic limitations as support for causal hypotheses, even if they are regarded as uncontaminated. Consider, for example, products of the method of free association. According to Grünbaum (1983b), the epistemic legitimation of free association as a reliable means of identifying and certifying pathogenic causes as such collapsed with the demise of the Breuer-Freud cathartic method. Thus, the most that the method of free association can come up with is the expression of a thought or wish that was previously repressed. But this is a far cry from the etiologic claim that the pertinent repression had been the *pathogen* P of the patient's neurosis N on the strength of its emergence as an association to the symptom. For, Grünbaum (1983b) argues:

to support Freud's etiologic hypothesis that P is causally necessary for N, evidence must be produced to show that being a P *makes a difference* to being an N. But such causal relevance is *not* attested by mere instances of Ns that were Ps, i.e., by patients who are both Ps and Ns. For a large number of such cases does not preclude that just as many *non*-Ps would also become Ns, if followed in a horizontal study from childhood onward! Thus, instances of Ns that were Ps may just *happen* to have been Ps. Then being a P has no etiologic role at all in becoming an N....Thus, to provide evidence for the causal relevance claimed by Freud, we need to *combine* instances of Ns that were Ps with instances of non-Ps who are *non*-Ns. Indeed, since he deemed P to be causally necessary for N—rather than just causally relevant—his etiology requires that the class of non-Ps should not contain *any* Ns whatever, and the class of Ps is to have a positive (though numerically unspecified) incidence of Ns [p. 277].

Furthermore, for the purpose of supporting *etiologic* (causal) hypotheses,

the absence of such controls undermines the probative value of not only data collected by the method of free association, but also evidence based on memories such as those discussed by Glymour (1974) in Freud's Rat Man case.

Freud's causal explanations of dreams and parapraxes fare no better, as it turns out (see Grünbaum, 1983b, pp. 222-265). There are two basic difficulties. First, Freud's claim that dreams and parapraxes are like neurotic symptoms in the sense of being compromises between the demands of our unconscious and conscious life is simply an extrapolation from his theory of psychoneurosis. In fact, Grünbaum (1983b) argues that it is a *misextrapolation* because there is nothing akin to the therapeutic base of the latter. With respect to parapraxes, for example, "Freud did not adduce any evidence that the permanent lifting of a repression to which he had attributed a parapraxis will be 'therapeutic' in the sense of enabling the person himself to correct the parapraxis *and* to avoid its repetition in the future" (Grünbaum, 1983b, p. 225). Second, the method Freud used to identify the particular unconscious determinants of dreams and parapraxes is simply the method of free association. Hence, even assuming that it is free from epistemic contamination, the method is powerless to provide support for any *causal* hypothesis, including those pertinent to dreams and parapraxes.

EXAMINATION OF A RADICAL CRITIQUE OF GRÜNBAUM'S VIEWS

Because most of Grünbaum's work on psychoanalysis is so recent, there has,

as yet, been little time for critical reply. One exception is the strongly negative reaction of Flax (1981), who argues that “neither Popper nor Grünbaum offers an adequate philosophy of science by which psychoanalysis may be judged” (p. 561). Furthermore, she chastises Grünbaum for restricting his discussion to Freud, contending that “this is like confining a discussion of physics to Newton because contemporary physics is in such disarray and then throwing out physics because there are unresolved problems in Newton’s theory” (p. 564). More specifically, she seems to believe that the more contemporary version of psychoanalysis embodied in object relations theory is immune from the epistemic difficulties Grünbaum attributes to Freud. In fact, she makes the astounding claim that “all the phenomena that Grünbaum counts as the clinical liabilities of psychoanalysis on empiricist grounds—epistemic contamination (i.e., intersubjectivity), suggestion, the placebo effect, etc.... are evidence that object-relations theory is correct” (p. 567). Since these points would, if correct, strike at the heart of Grünbaum’s work, I will conclude my discussion of Grünbaum with a consideration of Flax’s principal contentions.

Flax’s strategy of attack involves isolating a number of assumptions “suppressed within this debate” that she takes to be problematic. To make her case, then, she must show both that these assumptions are problematic and that they are, in fact, *essential* to Grünbaum’s arguments. I suggest that she does neither, with the failure on the second count the more serious. It is to this point that I will direct my remarks, for it suggests that she has seriously misunderstood

the character of Grünbaum's arguments. Let me give a few examples.

"Empiricism," Flax (1981) claims, "is simply untenable as a methodology of philosophy of science. A datum is never observed as it is in itself...Thus fact and theory cannot be totally distinct. Empirical experience loses its special status as the most privileged and unproblematic evidence... All data are 'epistemically contaminated' " (p. 563). I take it that the assumption of concern here is that data gathered on the basis of observation are somehow epistemically privileged and independent of theory. Suppose, however, we take this to be false. Suppose we agree that observation is always "theory laden" in the sense that it always involves interpretation, and such interpretation is relative to a person's conceptual apparatus, beliefs, expectations, and so forth. Furthermore, we take it that observational claims, like any other, are subject to controversy and revision and must be supported if contested. Does it then follow that Grünbaum's epistemological liabilities arguments fail? Flax's reasoning seems to be that if all observation is theory laden, then all data are "epistemically contaminated," including the data of our most esteemed scientific theories. Hence, any argument based on the implicit assumption that a theory cannot be scientific if it is based on contaminated data will be an argument based on a totally unreasonable demarcation principle.

The difficulty with this line of reasoning is that it is perfectly possible to agree that all observation is theory laden and still maintain a distinction between

data that are *biased* in a damagingly relevant sense and those that are not so biased. The ideal of objective data is possible at least to the extent that data relevant to a given theory T can be collected by someone whether he or she believes in T or even, in fact, whether he or she has knowledge of T. Scientists have become increasingly aware of the ways in which experimenters' bias toward their pet hypotheses can affect the outcome of experiments. With animal subjects, bias often operates in the recording of observations; with human subjects, it can be unintentionally conveyed in the communication of experimental instructions. But is it important to note that the result of this increasing knowledge about the potential pitfalls of experimenter bias has not been despair over the inevitable irrationality and arbitrariness of scientific theorizing. Rather, it has been the adoption of new and more stringent *controls* to minimize or eliminate such bias. For example, the use of so-called "double blind" experimental procedure has become standard for experimentation with human subjects in drug and other medical research.

Grünbaum's quarrel with the use of clinical evidence as support for psychoanalytic theory is precisely that it consists of data subject to the charge of investigator bias. Not only are the data being gathered in the clinical setting obtained by someone firmly committed to the truth of the theory, but they are gathered in such a way—during the course of a therapeutic process in which transference plays a major role—that even Freud (1917, pp. 446-447) worried about the charge of suggestion.

Furthermore, as Grünbaum (1983d) points out, Flax fails to distinguish between data that is merely theory-laden and data generated by the self-fulfilling use of the theory in their production. As Merton's (1949) studies of self-fulfilling and self-defeating predictions in the social sciences have shown, "*identifiable alterations* of the presumed initial conditions, rather than *mere* theory-ladenness, generate phenomena that furnish demonstrably spurious confirmations and disconfirmations" (p. 50). Again, it's not mere theory ladenness but the occurrence of precisely such alterations of the presumed initial conditions that is the object of Grünbaum's concern. In Grünbaum's (1983d) view, this occurrence "has been tellingly demonstrated *experimentally* in studies [reported in Marmor, 1970] of the purportedly "free" associations produced by patients in analysis" (p. 50). Grünbaum can perfectly well grant that all data are theory laden and still maintain that *certain* forms of theory-ladenness are epistemically unacceptable.

Another one of Flax's (1981) objections is that "a purely internal philosophical analysis of theories and theory shifts is not adequate...[to] explain why a theory is accepted as "credible" or when this acceptance occurs" (p. 563). The problematic assumption she has in mind is obviously that such a purely internal philosophical analysis of theories and theory shifts is adequate for such purposes. What Flax means by such a "purely internal philosophical analysis" can be gleaned from the sorts of considerations she thinks are left out of account. She writes: "At least equally important and under dispute is what counts as a fact, how data are to be interpreted and which data must be explained" (p. 563). I take it

then that such a purely internal analysis, then, is one that focuses solely on the relationship of theory to evidence as the basis of theory choice. And the putatively problematic assumption would be that it is possible to give a complete account of why scientists in fact accept theories as credible at particular moments in the history of science solely in terms of the logical relations between theory and evidence. Now, I have grave doubts as to whether most normative philosophers of science, including Grünbaum, would accept this assumption. But the main point again is: What of it? Suppose we agree that the assumption is wrong. It seems to me that Grünbaum's discussion of the problems inherent in the use of clinical data rests on no assumptions whatsoever concerning the sorts of considerations that must be invoked to explain particular historical occurrences of accepting particular theories. Someone interested in the *assessment* of the evidential grounds claimed to provide support for a theory is simply interested in a different question than someone interested in explaining why those who have accepted the theory did so. The former is a question about epistemic merit, the second about human psychology. Comments by Flax (1981) such as "Neither Grünbaum's nor Popper's philosophy, can provide an adequate account of the scientific process" (p. 563) indicate that she has no real understanding of the normative project. Certainly one can argue that the facts of scientific practice bear on one's choice of normative principles. And given certain views about what down-to-earth normative philosophy of science should be like, one can fault a particular exercise in appraisal for using utopian standards. However, all this in no way affects the

point that normative philosophy of science is not concerned with giving a psychological or political or sociological or historical explanation of why particular episodes in the history of science occurred as they did, and, therefore, ought not be criticized if it does not do so.

Perhaps Flax in some sense realizes this, for her last criticism concerning the putatively problematic nature of Grünbaum's grounds goes for the jugular. Flax (1981) writes:

Some of the greatest weaknesses of both Popper's and Grünbaum's accounts of science stem from the attempt rationally and arbitrarily to reconstruct the nature of scientific practice. Integrally connected with rationalization is their claim to legitimately legislate what counts as science and to evaluate how well it is done. Neither Popper nor Grünbaum give a scientific or philosophic justification for this claim, and there are good philosophical grounds for questioning its validity [p. 564].

Flax does not tell us here what those grounds are but simply refers us to Rorty (1979). Surely, one might think, I cannot charge Flax with irrelevance here. For certainly, Grünbaum's criticism of Freudian theory does at least presuppose that normative philosophy of science is a legitimate enterprise. My reply is, yes and no. Normative philosophy of science is not just one sort of thing, but many. I believe that what Flax is attacking is a far more ambitious form of the enterprise than the one Grünbaum undertakes in his recent writings. The ambitious form aims at a global rational reconstruction of at least those parts of scientific practice that seem to be governed by reason. This involves an attempt to find a set of

principles that serve to *rationalize* the decisions, acts, and heuristic rules that belong to actual scientific practice.

This ambitious form of normative philosophy may well not be possible. But the enterprise can be made more modest in a number of ways. First, rational reconstruction can be done in a piecemeal rather than a global way. Second, the philosopher of science can engage in the appraisal of specific scientific contributions not as an external critic, invoking, as Scheffler (1967) puts it, norms based on “an abstract epistemological ideal” but rather as a *participant* whose norms are “an ideal which, regulating the characteristic activities of science, may enter into its very description” (p. 73). Flax fails to understand that in Grünbaum’s various epistemological liabilities arguments, he is playing it very close to the ground. The normative principles he invokes do not stem from any philosophical rational reconstruction of scientific practice. They are part of that practice itself. This is particularly true of the inductivist principles he marshals in his criticism of Freud’s attempts to establish his causal claims. Furthermore, Grünbaum mounts a persuasive case that these normative principles are ones Freud himself explicitly avowed. Thus, it seems quite true, as Grünbaum (1983c) himself says, that the verdict he reaches concerning the scientific merit of psychoanalysis “is hardly predicated on the imposition of some extraneous methodological purism” (p. 13). The point, then, is this. Even if Flax were to convince us that a global rational reconstruction of science were impossible, I do not see how this would undercut Grünbaum’s critique in any way.

Grünbaum (1983d) himself has replied to Flax's other charges. He is particularly insistent that the critique he has offered of Freud's claims is equally applicable to more contemporary psychoanalytic theorists such as Heinz Kohut and the object relations school. These latter-day theorists

all claim clinical sanction for the generic repression-aetiology of neuroses. And they hold that free association has the *epistemic capability* of identifying the unconscious *causes* of all kinds of thought and behavior, such as dream content and parapraxes. Moreover, qua being psychoanalytic, the post-Freudian versions also deem the successful lifting of repressions to be the decisive agency in the postulated insight dynamics of the therapy [p. 47].

Eagle, a psychoanalytically oriented clinical psychologist, has recently voiced full support for these claims of Grünbaum's. After examining recent formulations in psychoanalytic object relations theory and self psychology, Eagle (1983) concludes:

Contrary claims notwithstanding, Grünbaum's criticisms of Freudian theory are neither vitiated nor undone by these recent developments. In no way do current formulations somehow manage to weaken or even constitute a response to these criticisms. The clinical data generated by an object relations theory or self psychology approach are as epistemologically contaminated as data generated by the more traditional approach. There is a little, or perhaps even less, evidence available on therapeutic process and therapeutic outcome. And finally, the etiological claims made in more current formulations are perhaps even more logically and empirically flawed than Freud's etiological formulations [pp. 49-50].

Thus, Flax's analogy to the case of physics completely misfires, Grünbaum

(1983d) asserts, “if only because the much vaunted post-Freudian versions have not remedied a single one of the methodological defects” (p. 48) that Grünbaum charges against the psychoanalytic method of clinical investigation.

SUMMARY

In his recent work on psychoanalytic epistemology, Grünbaum has exhibited an extremely impressive command of both the psychoanalytic literature and the philosophy of science. His views thus ought to be taken very seriously by anyone interested in the epistemic status of psychoanalytic theory. I have attempted here to extract the principal points and arguments contained in that body of work.

Grünbaum addresses himself to two fundamental questions: (1) What sorts of standards of assessment ought we to invoke in evaluating psychoanalysis? and (2) How does psychoanalysis measure up relative to those standards? Because Freud himself insisted that psychoanalysis was a natural science, and because, in Grünbaum’s view, there are no good arguments to the contrary, Grünbaum has insisted that psychoanalysis ought to be assessed as an empirical science. To support his position, he has engaged in debate with Popper, Habermas, Ricoeur, and George Klein, although we have restricted our attention here to his consideration of the views of Popper and Habermas.

Popper’s famous contention that psychoanalysis is not scientific because it is unfalsifiable was historically important not only because it raised interesting

questions about the epistemic status of psychoanalysis but also because it raised fundamental issues about what makes something scientific. In the context of replying to Popper's challenge, Grünbaum has argued that (1) falsifiability is a meaningful notion in science, although it is not the touchstone of scientific rationality as Popper maintains; (2) in particular, Popper is completely wrong in claiming that inductivism is powerless to impugn the scientific credentials of a theory like psychoanalysis; (3) Popper has no good arguments for his claim that psychoanalysis is unfalsifiable; and (4) in fact, psychoanalysis is falsifiable, if one applies a scientifically reasonable notion of falsifiability.

Habermas has argued that psychoanalysis ought to be regarded as a *critical* science rather than as an empirical-analytic one. Habermas rests his case on two sets of arguments. The first concerns the relationship of the clinical theory to the metapsychology. The second concerns the epistemic properties of the clinical theory itself. Grünbaum has addressed these arguments as follows: First, Freud rightly saw that the scientific status of the clinical theory is not dependent on that of the metapsychology; hence, any argument which assumes that there is such a dependence is irrelevant to the question of whether the clinical theory is scientific. Second, the *specific* arguments that Habermas advances to show that the clinical theory is not appropriately regarded as an empirical science fail, either because Habermas does not correctly understand psychoanalysis or because he is ignorant of certain features of the natural sciences.

In considering the *merits* of psychoanalysis as a scientific theory—that is, in reply to the second of the questions he sets himself—Grünbaum has argued for three points: (1) the therapeutic effectiveness of the characteristic constituent factors of psychoanalytic treatment is seriously in question; (2) all known attempts to save clinical data from the charge of contamination from suggestion fail, so that such data are virtually useless in providing support for the cardinal hypotheses of Freudian theory; and (3) even if clinical data were not epistemologically contaminated, they would not support the basic tenets of Freud's theoretical structure, *because Freud's major clinical arguments are basically flawed.*

The most explicit critique of Grünbaum's views to date is to be found in the work of Flax (1981). Flax argues that Grünbaum's discussion of psychoanalysis makes use of a number of implicit assumptions regarding the nature of science which, in her view, are seriously questionable. To make her case, she must show both that these assumptions are problematic and that they are essential to Grünbaum's arguments. I have argued that, in fact, she does neither. The second failing is the more serious because it indicates that Flax does not clearly understand the sort of normative philosophy of science in which Grünbaum is engaged.

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Notes

- [1\]](#) We are confronted, unfortunately, with a terminological difficulty concerning the word “science.” English renditions of Habermas use the word ‘science’ as the translation of the German ‘Wissenschaft.’ Hence, it is used in the more inclusive sense, which encompasses not only the natural sciences but also the hermeneutic and critical sciences. In contrast, when we ask Grünbaum whether psychoanalysis is a science, we are using the term to refer paradigmatically to what physicists, chemists, and biologists do, and it becomes an open question whether the so-called cultural and critical “sciences” in fact count as science. I alert the reader to this fact so as to minimize possible confusion. I will try to make it clear in context which sense is intended.

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