IS SIGMUND FREUD’S PSYCHOANALYTIC EDIFICE RELEVANT TO THE 21ST CENTURY?

Adolf Grünbaum, PhD

University of Pittsburgh

To warrant the relevance, if any, of Freud’s psychoanalytic edifice to the 21st century, its supporters must endeavor, if at all possible, to find genuine evidence for its major pillars or to modify them significantly in response to emerging new evidence. Such a quest must begin with a clear understanding of the range and depth of the failure of Freud’s cardinal clinical arguments. I endeavor below to provide such comprehension by laying bare the epistemological gravamen in the case of each of his principal tenets. And I argue that neither the post-Freudian formulations of psychoanalysis nor its so-called “hermeneutic” reconstruction has succeeded in vindicating the psychoanalytic enterprise.

Keywords: Freud, psychoanalysis

Preface

Psychoanalytic theory and therapy are indeed increasingly in crisis, not only in the United States, but in much of the world, although Argentina and France are notable exceptions. Alarmèd by this threat in 2000, the American Psychoanalytic Association (APA) commissioned a Strategic Marketing Initiative in the forlorn hope of devising methods to recruit more psychoanalytic patients. Thus, as reported in 2003 in Time magazine, there are now only 5,000 such patients, amounting to just two per practicing member of the APA (Kirsner, 2004). That number is declining while the mean age of the analysts is 62 and increasing. Similarly, ominous numbers pertain to the British Psychoanalytic Society.

For well over a century, the bulk of the psychoanalytic community has complacently squandered and recklessly forfeited its erstwhile near monopoly: No genuine effort was made to procure the required cogent evidence for the credibility of Freud’s causal inferences and explanations in psychopathology, psychotherapy, dream theory, theory of psychosexual development, and theory of slips.
Alas, the recent responses of mainstream psychoanalysis and of its apologists contribute further to its stagnation and even decadence.

Thus, the philosopher Peter Caws tries to snatch victory from the jaws of scientific defeat. Speaking of my epistemological critique of psychoanalysis (Grünbau, 1984, 1993, 2002), Caws (2003) writes “...it might be that everything Grünbau said was completely brilliant and right, but that it was also completely beside the point” (p. 624). Instead, Caws offers the altogether elusive program of “reconceptualizing psychoanalysis as a set of local theories (metatheoretically linked) applicable to idiosyncratic cases. Every patient is a new world, whose laws it is the task of the analyst to establish and apply” (p. 618). But how, I ask, can the analyst possibly hope to “establish” the allegedly “idiosyncratic” causal “laws” governing the genesis of a singular patient’s psychopathology? Alas, Caws’ merely programmatic ipse dixit (p. 619) does not even begin to come to grips with this challenge and cries out for at least one specific example. Besides, this challenge is not predicated at all on Caws’ red herring of “physics envy” (p. 620). As the reader will see from my essay below, Caws irresponsibly caricatured my critique of psychoanalysis, when he manufactured a straw man evasively: “Grünbau (1993) takes the straight philosophy-of-physics line and demolishes Freud with heavy artillery” (p. 624).

As against Caws’ phantom, note that ever since Francis Bacon four centuries ago, it has been clear that there is nothing at all endemic to physics in the requirements for the validation of causal hypotheses, with which psychoanalytic theory and therapy are replete. And I demanded the fulfillment of just these entirely legitimate epistemological and methodological requirements when I argued that the principal tenets of the Freudian corpus, which are causal, have not been validated even a century after their enunciation. Besides, causal hypotheses are also avowedly indispensable to Caws’ fancied version of psychoanalysis. Moreover, his conception of psychoanalysis misguidedly patronizes Freud, when he tells us with complacent elusiveness: “...perhaps psychoanalysis wasn’t something built on a foundation by Freud, even though he thought it was...it could best be thought of as a scaffolding that might enable other people to build something” (Caws, 2003, p. 624).

Yet how, I ask, can Caws see himself as implementing his evisceration and emasculation of Freud’s edifice in the face of declaring: “The explanandum to be explained will be the symptom that brought the patient into analysis in the first place, the explanans [the explainor] will draw its material mainly from the unconscious” (p. 619). This excruciatingly unhelpful statement of an explanans as drawing “its material from the unconscious” plainly cries out for concrete illustration by at least one specific case history! How, for example, can Caws’ appeal to such an explanans hope to dispense at all with Freud’s generalization that repression is pathogenic? Indeed, as Freud rightly told us foundationally: “The theory of repression is the cornerstone on which the whole structure of psychoanalysis rests. It is the most essential part of it” (Freud, 1914/1957a, p. 16). It seems that with friends like Caws, psychoanalysis needs no enemies.

Equally unsuccessfully, in a 2004 article, the Australian philosopher Douglas Kirsner exhorts psychoanalysts to engage in “the exciting method of philosophical probing of the human mind and the nature of human nature” (p. 339). But how can analysts do such probing without empirical tests? As I have shown (Grünbau, 2002), Freud’s investigative method of free association does not possess the required probative capabilities at all. Thus, Kirsner’s utopian exhortation rests on quicksand.

Besides, despite Freud’s own clear commitment to validating his hypotheses by familiar standards of evidence, he is being misportrayed as a crypto-hermeneuticist and litterateur.
Alas, a low point in the contemporary psychoanalytic literature is Elisabeth Roudinesco’s book *Why Psychoanalysis?* (1999; 2001). Very regrettably, it is marked by a primitive incomprehension of the philosophy of science, by egregious straw men, as well as by personal abuse of those whose views she depletes, myself included, labeling me “the principal U.S. representative of scientistically inspired anti-Freudianism” (chap. 7, p. 74). But she does not deign to come to grips with any of my criticisms of Freud’s ideas. Nor is she equipped to do so.

What are the future prospects of psychoanalysis in the 21st century? Claiming predictive certainty, the American psychoanalysts Jacob Arlow and Charles Brenner declared Pollyannaishly: “Psychoanalysis will continue to furnish the most comprehensive and illuminating insight into the human psyche” (1988, p. 13). But the eminent American psychologist and psychoanalyst Paul Meehl was much more sober. Speaking of my own “core objection” to psychoanalysis as “the biggest single methodological problem that we [psychoanalysts] face,” Meehl declared: “If that problem cannot be solved, we will have another century in which psychoanalysis can be accepted or rejected, mostly as a matter of personal taste. Should that happen, I predict it will be slowly but surely abandoned, both as a mode of helping and as a theory of the mind” (1995, p. 1021).

**Introduction**

The most basic ideas of psychoanalytic theory were initially enunciated in Josef Breuer and Sigmund Freud’s “Preliminary Communication” of 1893, which introduced their *Studies on Hysteria* (1893/1955a). But the first published use of the word “psychoanalysis” occurred in Freud’s 1896 French paper on “Heredity and the Aetiology of the Neuroses” (Freud, 1896/1962a). Therein Freud designated Breuer’s method of clinical investigation as “a new method of psycho-analysis” (p.151). Breuer used hypnosis to revive and articulate a patient’s unhappy memory of a supposedly repressed traumatic experience. The repression of that painful experience had occasioned the first appearance of a particular hysterical symptom, such as a phobic aversion to drinking water. Thus, Freud’s mentor also induced the release of the suppressed emotional distress originally felt from the trauma. Thereby Breuer’s method provided a catharsis for the patient.

The cathartic lifting of the repression yielded relief from the particular hysterical symptom. Breuer and Freud (1893/1955a, pp. 6–7; 1893-1955b, pp. 29–30) believed that they could therefore hypothesize that the repression, coupled with affective suppression, was the crucial cause for the development of the patient’s psychoneurosis.

Having reasoned in this way, they concluded in Freud’s words: “Thus one and the same procedure served simultaneously the purposes of [causally] investigating and of getting rid of the ailment; and this unusual conjunction was later retained in psychoanalysis” (Freud, 1923/1961a, p. 194).

In a 1924 historical retrospect, Freud acknowledged the pioneering role of Breuer’s cathartic method: “The cathartic method was the immediate precursor of psychoanalysis; and, in spite of every extension of experience and of every modification of theory, is still contained within it as its nucleus” (Freud, S. p. 194).

Yet Freud was careful to highlight the contribution he made himself after the termination of his collaboration with Breuer. Referring to himself in the third person, he tells us: “Freud devoted himself to the further perfection of the instrument left over to him by his elder collaborator. The technical novelties which he introduced and the discoveries he made changed the cathartic method into psycho-analysis” (p.195).
These extensive elaborations have earned Freud the mantle of being the father of psychoanalysis.

By now, the psychoanalytic enterprise has completed more than its first century. Thus, the time has come to take thorough critical stock of its past performance qua theory of human nature and therapy, as well as to have a look at its prospects.

It is important to distinguish between the validity of Freud’s work qua psychoanalytic theoretician, and the merits of his earlier work, which would have done someone else proud as the achievement of a lifetime. Mark Solms has edited and translated a forthcoming four-volume series, *The Complete Neuroscientific Works of Sigmund Freud* (London: Karnac). One focus of these writings is the neurological representation of mental functioning; another is Freud’s discovery of the essential morphological and physiological unity of the nerve cell and fiber. They also contain contributions to basic neuroscience such as the histology of the nerve cell, neuronal function, and neurophysiology. As a clinical neurologist, Freud wrote a major monograph on aphasia (Solms & Saling, 1990). As the editors pointed out in the preview “An Introduction to the Neuroscientific Works of Sigmund Freud” in *The Pre-Psychoanalytic Writings of Sigmund Freud* (Van de Vijver, G. & Geerardyn, F., 2002), Freud wrote major papers on cerebral palsy that earned him the status of a world authority. More generally, he was a distinguished pediatric neurologist in the field of the movement disorders of childhood. Furthermore, Freud was one of the founders of neuropsychopharmacology. For instance, he did scientific work on the properties of cocaine that benefited perhaps from his own use of that drug. Alas, that intake may well also account for some of the abandon featured by the more bizarre and grandiose of his psychoanalytic forays.

As Solms has remarked (personal communication, 1995), it is an irony of history that Freud, the psychoanalyst who postulated the ubiquity of bisexuality in humans, started out by deeming himself a failure for having had to conclude that eels are indeed bisexual. In a quest to learn how they reproduce, one of Freud’s teachers of histology and anatomy assigned him the task of finding the hitherto elusive testicles of the eel as early as 1877, when he was 21 years old. After having dissected a lobular organ in about 400 specimens in Trieste, Freud found that this organ apparently had the properties of an ovary no less than those of a testicle. Being unable to decide whether he had found the ever elusive testicles, Freud inferred that he had failed, as he reported in a rueful 1877 paper.

In 1880, Freud published a (free) translation of some of J. S. Mill’s philosophical writings (Stephan, 1989, pp. 85–86). Yet he was often disdainful of philosophy (Assoun, 1995), despite clearly being indebted to the Viennese philosopher Franz Brentano, from whom he had taken several courses: The marks of Brentano’s (1995) quondam representationalist and intentionalist account of the mental are clearly discernible in Freud’s conception of ideation. And the arguments for the existence of God championed by the quondam Roman Catholic priest Brentano further solidified the thoroughgoing atheism of Freud, the “godless Jew” (Gay, 1987, pp. 3–4).

**History and Logical Relations of the “Dynamic” and “Cognitive” Species of the Unconscious**

Freud was the creator of the full-blown theory of psychoanalysis, but even well-educated people often do not know that he was certainly not at all the first to postulate the existence of some kinds or other of unconscious mental processes. A number of thinkers did so earlier to explain conscious thought and overt behavior for which they could find no other
explanation (Freud, 1915/1957c, p. 166). As we recall from Plato’s dialogue *The Meno*, that philosopher was concerned to understand how an ignorant slave boy could have arrived at geometric truths under mere questioning by an interlocutor with reference to a diagram. Plato argued that the slave boy had not acquired such geometric knowledge during his life. Instead, he explained, the boy was tapping prenatal but *unconsciously stored* knowledge, and restoring it to his conscious memory.

At the turn of the 18th century, Leibniz gave psychological arguments for the occurrence of *subthreshold* sensory perceptions and for the existence of unconscious mental contents or motives that manifest themselves in our behavior (Ellenberger, 1970, p. 312). Moreover, Leibniz (1981) pointed out that when the contents of some forgotten experiences subsequently emerge in our consciousness, we may *misidentify* them as *new* experiences, rather than recognize them as having been unconsciously stored in our memory. As Leibniz put it: “It once happened that a man thought that he had written original verses, and was then found to have read them word for word, long before, in some ancient poet. . . . I think that dreams often revive former thoughts for us in this way” (p. 107).

Rosemarie Sand (personal communication, March 1, 1996) has pointed out that Leibniz’s notion anticipates, to some extent, Freud’s dictum that “*The interpretation of dreams is the royal road to a knowledge of the unconscious activities of the mind*” (Freud 1900/1958c, p. 608).

Before Freud was born, Hermann von Helmholtz discovered the phenomenon of “unconscious inference” as being present in sensory perception (Ellenberger, 1970, p. 313). For example, we often unconsciously infer the *constancy* of the physical size of nearby objects that move away from us when we have *other* distance cues, although their visual images decrease in size. Similarly, there can be unconsciously inferred constancy of brightness and color under changing conditions of illumination when the light source remains visible. Such unconscious *inferential compensation* for visual discrepancies also occurs when we transform our *non*-euclidean (hyperbolic) binocular visual space into the “seen” euclidean physical space (Grübaum, 1973, pp. 154–157).

Historically, it is more significant that Freud also had other precursors who anticipated some of his key ideas with impressive *specificity*. As he himself acknowledged (Freud, 1914/1957a, pp. 15–16), Arthur Schopenhauer and Friedrich Nietzsche had speculatively propounded major psychoanalytic doctrines that he himself reportedly developed independently from his clinical observations only thereafter. Indeed, a new German book by the Swiss psychologist Marcel Zentner (1995) traces the foundations of psychoanalysis to the philosophy of Schopenhauer.

Preparatory to my critical assessment of the psychoanalytic enterprise, let me emphasize the existence of major differences between the unconscious processes hypothesized by current cognitive psychology, on the one hand, and the unconscious contents of the mind claimed by psychoanalytic psychology, on the other (Eagle, 1987). These differences will show that the existence of the *cognitive* unconscious clearly fails to support, and even may cast doubt on, the existence of Freud’s *psychoanalytic* unconscious. His so-called *dynamic* unconscious is the supposed repository of repressed forbidden wishes of a sexual or aggressive nature, whose reentry or initial entry into consciousness is prevented by the defensive operations of the ego. Though socially unacceptable, these instinctual desires are so imperious and peremptory that they recklessly seek immediate gratification, independently of the constraints of external reality.

Indeed, according to Freud (1900/1958c, pp. 566–567), we would not even have developed the skills needed to engage in cognitive activities, if it had been possible to
gratify our instinctual needs without reliance on these cognitive skills. Thus, as Eagle has pointed out:

Freud did not seem to take seriously the possibility that cognition and thought could be inherently programmed to reflect reality and could have their own structure and development—an assumption basic to cognitive psychology.

After World War II, the psychoanalyst Heinz Hartmann was driven, by facts of biological maturation discovered non-psychoanalytically, to acknowledge in his so-called ego psychology that such functions as cognition, memory, and thinking can develop autonomously by innate genetic programming, and independently of instinctual drive gratification. Thus, Hartman spoke of a “conflict-free sphere of ego-function” (1987, p. 162).

In the cognitive unconscious, there is great rationality in the ubiquitous computational and associative problem-solving processes required by memory, perception, judgment, and attention. By contrast, as Freud emphasized, the wish content of the dynamic unconscious makes it operate in a highly illogical way.

There is a further major difference between the two species of unconscious (Eagle, 1987, pp. 161–165): The dynamic unconscious acquires its content largely from the unwitting repression of ideas in the form they originally had in consciousness. By contrast, in the generation of the processes in the cognitive unconscious, neither the expulsion of ideas and memories from consciousness nor the censorious denial of entry to them plays any role at all. Having populated the dynamic unconscious by means of repressions, Freud reasoned that the use of his new technique of free association could lift these repressions of instinctual wishes, and could thereby bring the repressed ideas back to consciousness unchanged. But in the case of the cognitive unconscious, we typically cannot bring to phenomenal consciousness the intellectual processes that are presumed to occur in it, although we can describe them theoretically.

For example, even if my life depended on it, I simply could not bring into my phenomenal conscious experience the elaborate scanning or search process by which I rapidly come up with the name of the Russian czarina’s confidante Rasputin when I am asked for it. Helmholtz’s various processes of “unconscious inference” illustrate the same point. By glossing over the stated major differences between the two species of unconscious, some psychoanalysts have claimed their compatibility within the same genus without ado (Shevrin, et al., 1992, pp. 340–341). But Eagle (1987, pp. 166–186) has articulated the extensive modifications required in the Freudian notion of the dynamic unconscious if it is to be made compatible with the cognitive one.

More important, some Freudian apologists have overlooked that, even after the two different species of the genus “unconscious” are thus made logically compatible, the dynamic unconscious as such cannot derive any credibility from the presumed existence of the cognitive unconscious. Nonetheless, faced with mounting attacks on their theory and therapy, some psychoanalysts have made just that fallacious claim. Thus, the Chicago analyst Michael Franz Basch (1994, p. 1) reasoned in vain that because neurophysiological evidence supports the hypothesis of a generic unconscious, “psychoanalytic theory has passed the [epistemological] test with flying colors.” On the contrary, we must bear in mind that evidence for the cognitive unconscious does not, as such, also furnish support for the dynamic unconscious as such.
Has Psychoanalytic Theory Become a Staple of Western Culture?

In appraising psychoanalysis, we must also beware of yet another logical blunder that has recently become fashionable: the bizarre argument recently given by a number of American philosophers (e.g., Nagel, 1994) that the supposed pervasive influence of Freudian ideas in Western culture vouches for the validity of the psychoanalytic enterprise. This argument is demonstrably untenable (Grübaum, 1994).

Even its premise that Freudian theory has become part of the intellectual ethos and folklore of Western culture cannot be taken at face value. As the great Swiss scholar Henri Ellenberger (1970, pp. 547–549) has stressed in his monumental historical work *The Discovery of the Unconscious*, the prevalence of vulgarized *pseudo*-Freudian concepts makes it very difficult to determine reliably the extent to which genuine psychoanalytic hypotheses have actually become influential in our culture at large. For example, any slip of the tongue or other bungled action (parapraxis) is typically yet incorrectly called a “Freudian slip.”

But Freud himself has called attention to the existence of a very large class of lapses or slips whose psychological motivation is simply transparent to the person who commits them or to others (Freud, 1916/1961b, p. 40). And he added commendably that neither he nor his followers deserve any credit for the motivational explanations of such perspicuous slips (Freud, 1916/1961b, p. 47). In this vein, a psychoanalyst friend of mine provided me with the following example of a *pseudo*-Freudian slip that would, however, be wrongly yet widely called “Freudian”: A man who is at a crowded party in a stiflingly hot room starts to go outdoors to cool off but is confronted by the exciting view of a woman’s décolleté bosom and says to her: “Excuse me, I have to get a breast of flesh air.” Many otherwise educated people would erroneously classify this slip as Freudian for two wrong reasons: First, merely because it is motivated, rather than a purely mechanical lapsus linguae, and, second, because its theme is sexual.

Yet what is required for a slip or so-called parapraxis to qualify as *Freudian* is that it be motivationally *opaque* rather than transparent, precisely because its psychological motive is repressed (Freud, 1916/1961b, p. 41). As the father of psychoanalysis declared unambiguously: If psychoanalysis is to provide an explanation of a parapraxis, “we must not be aware in ourselves of any motive for it. We must rather be tempted to explain it by ‘inattentiveness,’ or to put it down to ‘chance’” (Freud, 1901/1960, p. 239). And Freud characterized the pertinent explanatory unconscious causes of slips as “motives of unpleasure.” Thus, when a young man forgot the Latin word *aliquis* in a quotation from Virgil, Freud diagnosed its interfering cause as the man’s distressing unconscious fear that his girlfriend had become pregnant by him (Freud, 1901/1960, pp. 9–11). If that latent fear was actually the motive of the slip, it was surely not apparent to anyone.

Once it is clear what is meant by a bona fide Freudian slip, we need to ask whether there actually exist any such slips at all, that is, slips that appear to be psychologically unmotivated, but are actually caused by repressed, unpleasant ideas. It is very important to appreciate how difficult it is to provide cogent evidence for such causation. K. Schüttauf, J. Bredenkamp, and E. K. Specht (1997) claim to have produced just such evidence. They note that, according to psychoanalytic etiologic theory, obsessive-compulsive neurosis is attributable to an unconscious conflict whose repressed component features anal-erotic and sadistic wishes, which are presumably activated by regression. Then they reason that when such conflict-laden material is to be verbalized by obsessive-compulsive neurotics, Freudian theory expects a higher incidence of misspeakings (slips
of the tongue) among them than among normal subjects. And these researchers report that all of their findings bore out that expectation.

This investigation by Schüttauf, Bredenkamp, and Specht differs from Bröder’s (1995) strategy, which was designed to inquire into “the possible influence of unconscious information-processing on the frequency of specific speech-errors in an experimental setting.” Thus, Bröder and Bredenkamp claim to have produced experimental support for the “weaker Freudian thesis” of verbal slip generation by unconscious, rather than repressed, thoughts: “Priming words that remain unconscious induce misspeaking errors with higher probability than consciously registered ones” (1996, p. 175).

As for the soundness of the design of Schüttauf, Bredenkamp, and Specht, Hans Eysenck (R. Sand, personal communication, March 1, 1996) has raised several objections: (1) “As the author [Schüttauf] himself acknowledges, this is not an experiment, as ordinarily understood; it is a simple correlational study. . .correlation cannot be interpreted as causation, which he unfortunately attempts to do”; (2) The members of the experimental group were severely neurotic while the control group were normals, but “the proper control group would have been severely [disturbed] neurotics suffering from a different form of neurosis than that of obsessive-compulsive behavior”; (3) “Freudian theory posits a causal relationship between the anal stage of development and obsessive-compulsive neurosis; the author does not even try to document this hypothetical relationship”; and (4) “Obsessive-compulsive neurotics suffer from fear of dirt and contamination, so that on those grounds alone they would be likely to react differentially to stimuli suggesting such contamination. . . . It is truly commonsensical to say that people whose neurosis consists of feelings of dirt will react differentially to verbal presentations of words related to dirt.”

Naturally, I sympathize with Schüttauf and his coworkers in their avowed effort to escape my criticism (Grünbaum, 1984, pp. 202–205) of an earlier purported experimental confirmation of Freud’s theory of slips by Motley (1980). I had complained that the independent variable Motley manipulated in his speech-error experiments did not involve unconscious antecedents—but only conscious ones. As Schüttauf, Bredenkamp, and Specht tell us, precisely to escape my criticism of Motley, they relied on Freud’s etiology of obsessive-compulsive neurosis to infer that subjects who exhibit the symptoms of that neurosis fulfill the requirement of harboring repressions of anal-sadistic wishes. Thus, only on that etiologic assumption does their manipulation of words pertaining to anal-sadistic themata warrant their expectation of a higher incidence of verbal slips in this group than among normals.

Surely one could not reasonably expect the authors themselves to have carried out empirical tests of the etiology on which their entire investigation is crucially predicated. Nonetheless, Eysenck’s demand for such evidence is entirely appropriate: Without independent supporting evidence for that etiology, their test is definitely not a test of Freud’s theory of slips of the tongue, let alone—as they conclude—a confirmation of it.

Thus, as long as good empirical support for the Freudian scenario is unavailable, we actually don’t know whether any bona fide Freudian slips exist at all. Just this lack of evidence serves to undermine Nagel’s thesis that cultural influence is a criterion of validity. After all, if we have no cogent evidence for the existence of genuinely Freudian slips, then Freud’s theory of bungled actions (parapraxes) might well be false. And if so, it would not contribute to its validity even if our entire culture unanimously believed in it and made extensive explanatory use of it: When an ill-supported theory is used to provide explanations, they run the grave risk of being bogus, and its purported insights may well be pseudo-insights.

A second example supporting my rejection of Nagel’s cultural criterion is furnished by
the work of the celebrated art historian Meyer Schapiro of Columbia University. Schapiro saw himself as greatly influenced by Freud in his accounts of the work of such painters as Paul Cézanne, who died in 1906 (Solomon, 1994). Of course, Schapiro never actually put Cézanne on the psychoanalytic couch. But he subjected artists indirectly “to his own [brand of speculative] couch treatment” (Solomon, 1994). In his best-known essay, Schapiro “turns the Frenchman into a case history.” Indeed, a recent tribute to Schapiro’s transformation of scholarship in art history (Solomon, 1994) says that his “accomplishment was to shake off the dust and open the field to a style of speculation and intellectual bravura that drew . . . most notably [on] psychoanalysis” (p. 24). Reportedly, “his insights into . . . the apples of Cézanne” (p. 24) make the point that Cézanne’s “depictions of apples contain [in Schapiro’s words] ‘a latent erotic sense.’”

But if apples are held to symbolize sex unconsciously for Cézanne or anyone else, why doesn’t anything else that resembles apples in some respect (e.g., being quasi-spherical) do likewise? Yet we learn that Schapiro’s, 1968 publication “The Apples of Cézanne” is “his best known essay” (Solomon, 1994, p. 25). Alas, if Schapiro’s claim that Cézanne was “unwillingly chaste” is to be a psychoanalytic insight gleaned from his art, rather than a documented biographical fact, Schapiro’s psychodiagnosis is an instance of what Freud himself deplored as “‘Wild’ Psycho-Analysis” (Freud, 1910/1957d, pp. 221–227). In any case, pace Nagel, such art historical invocation of Freud, however influential, does nothing, I claim, to enhance the credibility of psychoanalysis.

For centuries, even as far back as in New Testament narratives, both physical disease and insanity have been attributed to demonic possession in Christendom, no less than among primitive peoples. That demon theory has been used, for example, to explain deafness, blindness, and fever as well as such psychopathological conditions as epilepsy, somnambulism, and hysteria. Our contemporary medical term “epilepsy” comes from the Greek word ἐπιλέψις (“seizure”) and reflects etymologically the notion of being seized by a demon. Because exorcism is designed to drive out the devil, it is the supposed therapy for demonic possession. In the Roman Catholic exorcist ritual, which was endorsed by Pope John Paul II and by John Cardinal O’Connor of New York, the existence of death is blamed on Satan. And that ritual also survives in baptism as well as in blessing persons and consecrating houses.

How does the strength of the cultural influence of such religious beliefs and practices compare to that of Freud’s teachings? Though Freud characterized his type of psychotherapy as “primus inter pares” (Freud, 1933/1964b, p. 157), he conceded sorrowfully: “I do not think our [psychoanalytic] cures can compete with those of Lourdes. There are so many more people who believe in the miracles of the Blessed Virgin than in the existence of the unconscious” (p. 152). Clearly, the psychoanalytic and theological notions of etiology and of therapy clash, and their comparative cultural influence cannot cogently decide between them. But if it could, psychoanalysis would be the loser! This alone, I claim, is a reductio ad absurdum of the thesis that the validity of the psychoanalytic enterprise is assured by its wide cultural influence.

Nor can Nagel buttress that thesis by the dubious, vague declaration that psychoanalysis is an “extension” of common sense. As I have shown elsewhere (Grübaum, forthcoming), the term “extension” is hopelessly unable to bear the weight required by his thesis if actual psychoanalytic theory is to square with it. What, for example is common-sensical about the standard psychoanalytic etiologic explanation of male diffidence and social anxiety by repressed adult “castration anxiety” (Fenichel, 1945, p. 520), or of a like explanation of a male driver’s stopping at a green traffic light as if it were red (Brenner, 1982, pp. 182–183)? Common sense rightly treats such explanations incredulously as
bizarre, and rightly so: As I have shown (Grübaum, 1997), these etiologic explanations rest on quicksand, even if we were to grant Freud’s Oedipal scenario that all adult males unconsciously dread castration by their father for having lusted after their mother.

Critique of Freudian and Post-Freudian Psychoanalysis

Let me now turn to my critique of the core of Freud’s original psychoanalytic theory and to a verdict on its fundamental modifications by two major post-Freudian sets of hypotheses called self psychology and object relations theory.

The pillars of the avowed “cornerstone” of Freud’s theoretical edifice comprise several major theses: (1) Distressing mental states induce the operation of a psychic mechanism of repression, which consists in the banishment from consciousness of unpleasurable psychic states (Freud, 1915/1957b, p. 147); (2) Once repression is operative (more or less fully), it not only banishes such negatively charged ideas from consciousness, but plays a further crucial multiple causal role: It is causally necessary for the pathogens of neuroses, the production of our dreams, and the generation of our various sorts of slips (bungled actions); and (3) The “method of free association” can identify and lift (undo) the patient’s repressions; by doing so, it can identify the pathogens of the neuroses, and the generators of our dreams, as well as the causes of our motivationally opaque slips; moreover, by lifting the pathogenic repressions, free association functions therapeutically, rather than only investigatively. It is to be understood that the neurotic symptoms above are linked to unsuccessful repressions, being “compromise” formations as explained below.

Freud provided two sorts of arguments for his cardinal etiologic doctrine that repressions are the pathogens of the neuroses: His earlier one, which goes back to his original collaboration with Josef Breuer, relies on purported therapeutic successes from lifting repressions; the later one, designed to show that the pathogenic repressions are sexual, is drawn from presumed reenactments (“transferences”) of infantile episodes in the adult patient’s interactions with the analyst during psychoanalytic treatment.

It will be expositorily expeditious to deal with Freud’s earlier etiologic argument below, and to appraise the subsequent one, which goes back to his “Dora” case history of 1905, after that. But also for expository reasons, it behooves us to devote an introduction to his account of the actuation of the hypothesized mechanism of repression by “motives of unpleasure.”

Negative Affect and Forgetting

As Freud told us, “The theory of repression is the cornerstone on which the whole structure of psycho-analysis rests. It is the most essential part of it” (1914/1957a, p. 16). The process of repression, which consists in the banishment of ideas from consciousness or in denying them entry into it, is itself presumed to be unconscious (1915/1957b, p. 147). In Freud’s view, our neurotic symptoms, the manifest contents of our dreams, and the slips we commit are each constructed as “compromises between the demands of a repressed impulse and the resistances of a censoring force in the ego” (Freud, 1924/1959, p. 45; 1915/1963, p. 301). By being only such compromises, rather than fulfillments of the instinctual impulses, these products of the unconscious afford only substitution gratifications or outlets. For brevity, one can say, therefore, that Freud has offered a unifying “compromise model” of neuroses, dreams, and parapraxes. Since the repressed impulse
made a compromise with the repressing ego, compromise-formations are products of unsuccessful repressions.

But what, in the first place, is the motive or cause that initiates and sustains the operation of the unconscious mechanism of repression before it produces its own later effects? Apparently, Freud assumes axiomatically that distressing mental states, such as forbidden wishes, trauma, disgust, anxiety, anger, shame, hate, guilt, and sadness—all of which are unpleasurable—almost always actuate, and then fuel, forgetting to the point of repression. Thus, repression regulates pleasure and unpleasure by defending our consciousness against various sorts of negative affect. Indeed, Freud claimed perennially that repression is the paragon among our defense mechanisms (Thomä & Kächele, 1987, pp. 107–111). As Freud put it dogmatically: “The tendency to forget what is disagreeable seems to me to be a quite universal one” (1901/1960, p. 144), and “The recollection of distressing impressions and the occurrence of distressing thoughts are opposed by a resistance” (p. 146).

Freud tries to disarm an important objection to his thesis that “distressing memories succumb especially easily to motivated forgetting” (p. 147). He says:

The assumption that a defensive trend of this kind exists cannot be objected to on the ground that one often enough finds it impossible, on the contrary, to get rid of distressing memories that pursue one, and to banish distressing affective impulses like remorse and the pangs of conscience. For we are not asserting that this defensive trend is able to put itself into effect in every case. (p. 147, italics added)

Indeed, he acknowledges as “also a true fact” that “distressing things are particularly hard to forget” (Freud, 1916/1961b, pp. 76–77).

For instance, we know from Charles Darwin’s autobiography that his father had developed a remarkably retentive memory for painful experiences (cited in Grünbaum, 1994), and that a half century after Giuseppe Verdi was humiliatingly denied admission to the Milan Music Conservatory, he recalled it indignantly (Walker, 1962, pp. 8–9). Freud himself told us as an adult that he “can remember very clearly,” from age 7 or 8, how his father rebuked him for having relieved himself in the presence of his parents in their bedroom. In a frightful blow to Freud’s ego, his father said: “The boy will come to nothing” (Freud, 1900/1958b, p. 216).

But Freud’s attempt here to uphold his thesis of motivated forgetting is evasive and unavailing: Because some painful mental states are vividly remembered while others are forgotten or even repressed, I claim that factors different from their painfulness determine whether they are remembered or forgotten. For example, personality dispositions or situational variables may in fact be causally relevant. To the great detriment of his theory, Freud never came to grips with the unfavorable bearing of this key fact about the mnemonic effects of painfulness on the tenability of the following pillar of his theory of repression: When painful or forbidden experiences are forgotten, the forgetting is tantamount to their repression because of their negative affect, and thereby produces neurotic symptoms or other compromise formations. Thomas Gilovich, a professor of psychology at Cornell University, is now doing valuable work on the conditions under which painful experiences are remembered and on those other conditions under which they are forgotten.

The numerous and familiar occurrences of vivid and even obsessive recall of negative experiences pose a fundamental statistical and explanatory challenge to Freud that neither he nor his followers have ever met. We must ask (Grünbaum, 1994): Just what is the ratio of the forgetting of distressing experiences to their recall, and what other factors determine
that ratio? Freud gave no statistical evidence for assuming that forgetting them is the rule and remembering them is the exception. Yet, as we can see, his theory of repression is devastatingly undermined from the outset if forgettings of negative experiences do not greatly outnumber rememberings statistically. After all, if forgetting is not the rule, then what other reason does Freud offer for supposing that when distressing experiences are actually forgotten, these forgettings are instances of genuine repression because of affective displeasure? And if he has no such other reason, then, a fortiori, he has no basis at all for his pivotal etiologic scenario that forbidden or aversive states of mind are usually repressed and thereby cause compromise formations.

Astonishingly, Freud (1916/1961b) thinks he can parry this basic statistical and explanatory challenge by an evasive dictum, as follows: “Mental life is the arena and battle-ground for mutually opposing purposes [of forgetting and remembering]. . .there is room for both. It is only a question. . .of what effects are produced by the one and the other” (pp. 76-77). Indeed, just that question cries out for an answer from Freud if he is to make his case. Instead, he cavalierly left it to dangle epistemologically in limbo.

The Epistemological Liabilities of the Psychoanalytic Method of Free Association

Another basic difficulty, which besets all three major branches of the theory of repression alike, lies in the epistemological defects of Freud’s so-called fundamental rule of free association, the supposed microscope and X-ray tomograph of the human mind. This rule enjoins the patient to tell the analyst without reservation whatever comes to mind. Thus, it serves as the fundamental method of clinical investigation. We are told that by using this technique to unlock the floodgates of the unconscious, Freud was able to show that neuroses, dreams, and slips are caused by repressed motives. Just as in Breuer’s cathartic use of hypnosis, it is a cardinal thesis of Freud’s entire psychoanalytic enterprise that his method of free association has a twofold major capability, which is both investigative and therapeutic: (1) It can identify the unconscious causes of human thoughts and behavior, both abnormal and normal; and (2) By overcoming resistances and lifting repressions, it can remove the unconscious pathogens of neuroses and thus provide therapy for an important class of mental disorders.

But on what grounds did Freud assert that free association has the stunning investigatory capability to be causally probative for etiologic research in psychopathology? Is it not too good to be true that one can put a psychologically disturbed person on the couch and fathom the etiology of her or his affliction by free association? As compared to fathoming the causation of major somatic diseases, that seems almost miraculous, if at all true. Freud tells us very clearly (Freud 1900/1958c, p. 528) that his argument for his investigative tribute to free association as a means of uncovering the causation of neuroses is, at bottom, a therapeutic one going back to the cathartic method of treating hysteria. Let me state and articulate his argument.

One of Freud’s justifications for the use of free association as a causally probative method of dream investigation leading to the identification of the repressed dream thoughts, he tells us, is that it “is identical with the procedure [of free association] by which we resolve hysterical symptoms; and there the correctness of our method [of free association] is warranted by the coincident emergence and disappearance of the symptoms” (p. 528) But, as I have pointed out elsewhere (Grünbaum, 1993, pp. 25–26), his original German text here contains a confusing slip of the pen. As we know, the patient’s
symptoms hardly first emerge simultaneously with their therapeutic dissipation. Yet Strachey translated Freud correctly as having spoken of “the coincident emergence and disappearance of the symptoms.” It would seem that Freud means to speak of the resolution (German: Auflösung), rather than of the emergence (German: Auftauchen), of the symptoms as coinciding with their therapeutic dissipation. Now, for Freud, the “resolution of a symptom,” in turn, consists of using free association to uncover the repressed pathogen that enters into the compromise formation that is held to constitute the symptom. This much, then, is the statement of Freud’s appeal to therapeutic success to vouch for the “correctness of our method” of free association as causally probative for etiologic research in psychopathology.

To articulate the argument adequately, however, we must still clarify Freud’s original basis for claiming that (unsuccessful) repression is indeed the pathogen of neurosis. Only then will he have made his case for claiming that free association is etiologically probative because it is uniquely capable of uncovering repressions. The pertinent argument is offered in Breuer and Freud’s “Preliminary Communication” (1893/1955a, pp. 6–7).

There they wrote:

For we found, to our great surprise at first, 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, and when the patient had described that event in the greatest possible detail and had put the affect into words. Recollection without affect almost invariably produces no result. The psychical process which originally took place must be repeated as vividly as possible; it must be brought back to its status nascendi and then given verbal utterance. (p. 6)

Breuer and Freud then make an important comment on their construal of this therapeutic finding:

It is plausible to suppose that it is a question here of unconscious suggestion: the patient expects to be relieved of his sufferings by this procedure, and it is this expectation, and not the verbal utterance, which is the operative factor. This, however, is not so. (p. 7)

And their avowed reason is that, in 1881, that is, in the “‘presuggestion’ era,” the cathartic method was used to remove separately distinct symptoms, “which sprang from separate causes” such that any one symptom disappeared only after the cathartic (“abreactive”) lifting of a particular repression. But Breuer and Freud do not tell us why the likelihood of placebo effect should be deemed to be lower when several symptoms are wiped out seriatim than in the case of getting rid of only one symptom. Thus, as I have pointed out elsewhere (Grunbaum, 1993, p. 238), to discredit the hypothesis of placebo effect, it would have been essential to have comparisons with treatment outcome from a suitable control group whose repressions are not lifted. If that control group were to fare equally well, treatment gains from psychoanalysis would then be placebo effects after all.

In sum, Breuer and Freud inferred that the therapeutic removal of neurotic symptoms was produced by the cathartic lifting of the patient’s previously ongoing repression of the pertinent traumatic memory, not by the therapist’s suggestion or some other placebo factor (see Grunbaum, 1993, pp. 69–107 for a very detailed analysis of the placebo concept). We can codify this claim as follows:

“T. Therapeutic Hypothesis: Lifting repressions of traumatic memories cathartically is causally relevant to the disappearance of neuroses.”
As we saw, Breuer and Freud (1893/1955a, p. 6) reported the immediate and permanent disappearance of each hysterical symptom after they cathartically lifted the repression of the memory of the trauma that occasioned the given symptom. They adduce this “evidence” to draw an epoch-making inductive etiologic inference, which postulates “a causal relation between the determining [repression of the memory of the] psychical trauma and the hysterical phenomenon” (p. 6). Citing the old scholastic dictum “Cessante causa cessat effectus” (When the cause ceases, its effect ceases), they invoke its contrapositive (p. 7), which states that as long as the effect (symptom) persists, so does its cause (the repressed memory of the psychical trauma). And they declare just that to be the pattern of the pathogenic action of the repressed psychical trauma. This trauma, we learn, is not a mere precipitating cause. Such a mere “agent provocateur” just releases the symptom, “which thereafter leads an independent existence.” Instead, “the [repressed] memory of the trauma... acts like a foreign body which long after its entry must continue to be regarded as an agent that is still at work” (p. 6, Freud, 1983/1962b).

The upshot of their account is that their observations of positive therapeutic outcome from the abreactive lifting of repressions, which they interpret in the sense of their therapeutic hypothesis, spelled a paramount etiologic moral as follows:

“E. Etiologic Hypothesis: An ongoing repression accompanied by affective suppression is causally necessary for the initial pathogenesis and persistence of a neurosis” (Breuer & Freud, 1893/1955a, pp. 6–7).

(This formulation of the foundational etiology of psychoanalysis supersedes the one I gave at the suggestion of Carl Hempel and Morris Eagle [in Grünbaum, 1984, p. 181]. The revised formulation here is faithful to Breuer and Freud’s reference to “accompanying affect” [1893/1955a, p. 6] apropos of the traumatic events whose repression occasioned the symptoms.)

Clearly, this etiologic hypothesis E permits the valid deduction of the therapeutic finding reported by Breuer and Freud as codified in their therapeutic hypothesis T: The cathartic lifting of the repressions of traumatic memories of events that occasion symptoms engendered the disappearance of the symptoms (1893/1955a, p. 6). And, as they told us there explicitly, this therapeutic finding is their “evidence” for their cardinal etiologic hypothesis E.

But I maintain that this inductive argument is vitiated by what I like to call the “fallacy of crude hypothetico-deductive (H-D) pseudoconfirmation.” Thus, note that the remedial action of aspirin consumption for tension headaches does not lend H-D support to the outlandish etiologic hypothesis that a hematolytic aspirin deficiency is a causal sine qua non for having tension headaches, although such remedial action is validly deducible from that bizarre hypothesis. Twenty-five years ago, Wesley Salmon called attention to the fallacy of inductive causal inference from mere valid H-D deducibility by giving an example in which a deductively valid pseudoexplanation of a man’s avoiding pregnancy can readily give rise to an H-D pseudoconfirmation of the addle-brained attribution of his nonpregnancy to his consumption of birth control pills. Salmon (1971, p. 34) states the fatuous pseudoexplanation: “John Jones avoided becoming pregnant during the past year, for he had taken his wife’s birth control pills regularly, and every man who regularly takes birth control pills avoids pregnancy.”

Plainly, this deducibility of John Jones’ recent failure to become pregnant from the stated premises does not lend any credence at all to the zany hypothesis that this absence of pregnancy is causally attributable to his consumption of birth control pills. Yet it is
even true that any men who consume such pills in fact never do become pregnant. Patently, as Salmon notes, the fly in the ointment is that men just do not become pregnant, whether they take birth control pills or not.

His example shows that neither the empirical truth of the deductively inferred conclusion and of the pertinent initial condition concerning Jones, nor the deductive validity of the inference can provide bona fide confirmation of the causal hypothesis that male consumption of birth control pills prevents male pregnancy: That hypothesis would first have to meet other epistemic requirements, which it manifestly cannot do.

Crude H-D confirmationism is a paradise of spurious causal inferences, as illustrated by Breuer and Freud’s unsound etiologic inference. Thus, psychoanalytic narratives are replete with the belief that a hypothesized etiologic scenario embedded in a psychoanalytic narrative of an analysand’s affliction is made credible merely because the postulated etiology then permits the logical deduction or probabilistic inference of the neurotic symptoms to be explained.

Yet some apologists offer a facile excuse for the fallacious H-D confirmation of a causal hypothesis. We are told that the hypothesis is warranted by an “inference to the best explanation” (Harman, 1965, pp. 85–95). But in a careful new study, Salmon (2001, p. 79) has argued that “the characterization of non-demonstrative inference as inference to the best explanation serves to muddy the waters. . .by fostering confusion” between two sorts of why-questions that Hempel had distinguished: Explanation-seeking questions as to why something is the case, and confirmation-seeking why-questions as to why a hypothesis is credible. Thus, a hypothesis that is pseudoconfirmed by some data cannot be warranted qua being “the only [explanatory] game in town.” Alas, “best explanation”—sanction was claimed for psychoanalytic etiologies to explain and treat the destructive behavior of sociopaths to no avail for years (cf. Cleckley, 1988, esp. pp. 238–239 and 438–439).

I can now demonstrate the multiple failure of Freud’s therapeutic argument for the etiologic probativeness of free association in psychopathology, no matter how revealing the associative contents may otherwise be in regard to the patient’s psychological preoccupations and personality dispositions. Let us take our bearings and first encapsulate the structure of his therapeutic argument.

First, Freud inferred that the therapeutic disappearance of the neurotic symptoms is causally attributable to the cathartic lifting of repressions by means of the method of free association. Relying on this key therapeutic hypothesis, he then drew two further major theoretical inferences: (1) The seeming removal of the neurosis by means of cathartically lifting repressions is good inductive evidence for postulating that repressions accompanied by affective suppression are themselves causally necessary for the very existence of a neurosis (Breuer & Freud 1893/1955a, pp. 6–7); and (2) granted that such repressions are thus the essential causes of neurosis, and that the method of free association is uniquely capable of uncovering these repressions, this method is uniquely competent to identify the causes or pathogens of the neuroses. (Having convinced himself of the causal probative-ness of the method of free association on therapeutic grounds in the case of those neuroses he believed to be successfully treatable, Freud also felt justified in deeming the method reliable as a means of unearthing the etiologies of those other neuroses—the so-called narcissistic ones, such as paranoia—that he considered psychoanalytically untreatable.)

But the argument fails for the following several reasons. In the first place, the durable therapeutic success on which it was predicated did not materialize (Borch-Jacobsen, 1996), as Freud was driven to admit both early and very late in his career (1924/1959, p. 27; 1937/1964a, pp. 216–253). But even insofar as there was transitory therapeutic gain, we saw that Freud failed to rule out a rival hypothesis that undermines his attribution of
such gain to the lifting of repressions by free association: The ominous hypothesis of placebo effect, which asserts that treatment ingredients other than insight into the patient’s repressions—such as the mobilization of the patient’s hope by the therapist—are responsible for any resulting improvement (Grünbaum, 1993, chap. 3). Nor have other analysts ruled out the placebo hypothesis during the past century. A case in point is a 45-page study “On the Efficacy of Psychoanalysis” (Bachrach et al., 1991), published in the official Journal of the American Psychoanalytic Association. Another is the account of analytic treatment process by Vaughan and Roose (1995).

Last, but not least, the repression etiology is evidentially ill founded, as we saw earlier and will see further in the next section. It is unavailing to the purported etiologic probativeness of free associations that they may lift repressions because Freud failed to show that the latter are pathogenic. In sum, Freud’s argument has forfeited its premises.

Freud’s Etiologic Transference Argument

Now let us consider Freud’s argument for his cardinal thesis that sexual repressions in particular are the pathogens of all neuroses, an argument he deemed “decisive.” Drawing on my earlier writings (Grünbaum, 1990, pp. 565–567; 1993, pp. 152–158), we shall now find that this argument is without merit.

According to Freud’s theory of transference, the patient transfers onto his or her psychoanalyst, feelings and thoughts that originally pertained to important figures in his or her earlier life. In this important sense, the fantasies woven around the psychoanalyst by the analysand, and quite generally the latter’s conduct toward his or her doctor, are hypothesized to be thematically recapitulatory of childhood episodes. And by thus being recapitulatory, the patient’s behavior during treatment can be said to exhibit a thematic kinship to such very early episodes. Therefore, when the analyst interprets these supposed reenactments, the ensuing interpretations are called “transference interpretations.” But since the analyst typically has no independent epistemic access to the adult patient’s childhood episodes, they have to be inferred retrodictively in the first place from their supposed thematic recapitulation in the analytic treatment sessions.

Freud and his followers have traditionally drawn the following highly questionable causal inference: Precisely in virtue of being thematically recapitulated in the patient-doctor interaction, the hypothesized earlier scenario in the patient’s life can cogently be held to have originally been a pathogenic factor in the patient’s affliction. For example, in his case history of the “Rat-Man,” Freud (1909/1955) infers that a certain emotional conflict had originally been the precipitating cause of the patient’s inability to work, merely because this conflict had been thematically reenacted in a fantasy the “Rat-Man” had woven around Freud during treatment.

Thus, in the context of Freud’s transference interpretations, the thematic reenactment is claimed to show that the early scenario had originally been pathogenic. According to this etiologic conclusion, the patient’s thematic reenactment in the treatment setting is also asserted to be pathogenically recapitulatory by being pathogenic in the adult patient’s here and now, rather than only thematically recapitulatory. Freud (1914/1957a, p. 12) extols this dubious etiologic transference argument in his History of the Psycho-Analytic Movement, claiming that it furnishes the most unshakable proof for his sexual etiology of all the neuroses:

The fact of the emergence of the transference in its crudely sexual form, whether affectionate or hostile, in every treatment of a neurosis, although this is neither desired nor induced by
either doctor or patient, has always seemed to me the most irrefragable proof [original 
German: unerschütterlichste Beweis] that the source of the driving forces of neurosis lies in 
sexual life [sexual repressions]. This argument has never received anything approaching the 
degree of attention that it merits, for if it had, investigations in this field would leave no other 
conclusion open. As far as I am concerned, this argument has remained the decisive one, over 
and above the more specific findings of analytic work. (p. 12)

On the contrary, the patient’s thematically recapitulatory behavior toward his or her 
doctor does not show that it is also pathogenically recapitulatory. How, for example, does 
the reenactment during treatment of a patient’s early conflict show at all that the original 
conflict had been pathogenic in the first place? Quite generally, how do transference 
phenomena focusing on the analyst show that a presumed current replica of a past event 
is pathogenic in the here and now? Elsewhere (Grünbaum, 1984, p. 55; 1993, pp. 
136–138, item 9), I have examined the unsound inference of a causal connection from a 
more thematic connection between some events, under the rubric of “thematic affinity 
fallacy.”

Therefore, I submit, the purportedly “irrefragable proof” of which Freud spoke 
deserves more attention not because its appreciation “would leave no other conclusion 
open,” as he would have it; instead, I contend that the “Rat-Man” case and other such case 
histories show how baffling it is that Freud deemed the etiologic transference argument 
cogent at all, let alone unshakably so.

Marshall Edelson (1984) has offered a rebuttal to my denial of the cogency of the 
etiologic transference argument:

...in fact, in psychoanalysis the pathogen is not merely a remote event, or a series of such 
 events, the effect of which lives on. The pathogen reappears in all its virulence, with 
increasing frankness and explicitness, in the transference—in a new edition, a new version, a 
reemergence, a repetition of the past pathogenic events or factors. (p. 150)

And Edelson elaborates:

The pathogen together with its pathological effects are, therefore, under the investigator’s eye, 
so to speak, in the psychoanalytic situation, and demonstrating the causal relation between 
them in that situation, by experimental or quasi-experimental methods, surely provides 
support, even if indirect, for the hypothesis that in the past the same kind of pathogenic factors 
were necessary to bring about the same kind of effects. (p. 151)

But how does the psychoanalyst demonstrate, within the confines of his or her clinical 
setting, that the supposed current replica of the remote, early event is presently the 
virulent cause of the patient’s neurosis, let alone that the original pathogen is replicated 
at all in the transference? Having fallaciously identified a conflict as a pathogen because 
it reappears in the transference, many Freudians conclude that pathogens must reappear in 
the transference. And in this way, they beg the key question I have just asked. How, for 
example, did Freud show that the “Rat-Man’s” marriage conflict depicted in that patient’s 
transference fantasy was the current cause of his ongoing death obsessions? Neither 
Edelson’s book nor his 1986 paper offers a better answer. Thus, in the latter, he declares:

“The psychoanalyst claims that current mental representations of particular past events or 
fantasies are constitutive (i.e., current operative) causes of current behavior, and then goes on 
to claim that therefore past actual events or fantasies are etiological causes of the analysand’s 
symptoms.”
And Edelson concludes:

“Transference phenomena are...on question-begging evidence for...inferences about causally efficacious psychological entities existing or occurring in the here and now” (p. 110).

In sum, despite Edelson’s best efforts, the etiologic transference argument on which both Freud and he rely is ill founded: (1) They employ epistemically circular reasoning when inferring the occurrence of infantile episodes from the adult patient’s reports and then claiming that these early episodes are thematically recapitulated in the adult analysand’s conduct toward the analyst; (2) they beg the etiologic question by inferring that, qua being thematically recapitulated, the infantile episodes had been pathogenic at the outset; and (3) they reason that the adult patient’s thematic reenactment is pathogenically recapitulatory such that the current replica of the infantile episodes is pathogenic in the here and now.

Freud went on to build on the quicksand of his etiologic transference argument. It inspired two of his further fundamental tenets: first, the investigative thesis that the psychoanalytic dissection of the patient’s behavior toward the analyst can reliably identify the original pathogens of his or her long-term neurosis; second, the cardinal therapeutic doctrine that the working through of the analysand’s so-called transference neurosis is the key to overcoming his or her perennial problems.

Free Association as a Method of Dream Interpretation

Yet, as we learn from Freud’s opening pages on his method of dream interpretation, he extrapolated the presumed causally probative role of free associations from being only a method of etiologic inquiry aimed at therapy, to serving likewise as an avenue for finding the purported unconscious causes of dreams (Freud, 1900/1958b, pp. 100–101; see also 1900/1958c, p. 528). And in the same breath, he reports that when patients told him about their dreams while associating freely to their symptoms, he extrapolated his compromise model from neurotic symptoms to manifest dream contents. A year later, he carried out the same twofold extrapolation to include slips or bungled actions.

But what do free associations tell us about our dreams? Whatever the manifest content of dreams, they are purportedly wish-fulfilling in at least two logically distinct specific ways: For every dream $D$, there exists at least one normally unconscious infantile wish $W$ such that (1) $W$ is the motivational cause of $D$, and (2) the manifest content of $D$ graphically displays, more or less disguisedly, the state of affairs desired by $W$. As Freud opined: “When the latent dream-thoughts that are revealed by the analysis [via free association] of a dream are examined, one of them is found to stand out from among the rest...the isolated thought is found to be a wishful impulse” (1924/1959, p. 44). But Freud manipulated the free associations to yield a distinguished wish motive (Glymour, 1983).

Quite independently of Freud’s abortive therapeutic argument for the causal probativeness of free association, he offered his analysis of his 1895 “Specimen Irma Dream” (1900/1958b, pp. 96–121) as a nontherapeutic argument for the method of free association as a cogent means of identifying hypothesized hidden, forbidden wishes as the motives of our dreams. But in my detailed critique of that unjustly celebrated analysis (Grünbaum, 1984, chap. 5, pp. 216–239), I have argued that Freud’s account is, alas, no more than a piece of false advertising: (1) It does not deliver at all the promised vindication of the probativeness of free association; (2) it does nothing toward warranting his foolhardy dogma that all dreams are wish-fulfilling in his stated sense; (3) it does not even pretend
that his alleged “Specimen Dream” is evidence for his compromise model of manifest-dream content; and (4) the inveterate and continuing celebration of Freud’s analysis of his “Irma Dream” in the psychoanalytic literature as the paragon of dream interpretation is completely unwarranted hero-worship, because it is mere salesmanship.

Alas, Freud’s 1895 neurobiological wish-fulfillment theory of dreaming was irredeemably flawed from the outset (Grünonbaum, forthcoming). Furthermore, astonishingly, he did not heed a patent epistemological consequence of having abandoned his 1895 project’s neurological energy model of wish-driven dreaming: By precisely that abandonment, he himself had forfeited his initial biological rationale for claiming that at least all “normal” dreams are wish fulfilling. A fortiori, this forfeiture left him without any kind of energy-based warrant for then universalizing the doctrine of wish fulfillment on the psychological level to extend to any sort of dream. Yet, unencumbered by the total absence of any such warrant, the universalized doctrine, now formulated in psychological terms, rose like a phoenix from the ashes of Freud’s defunct energy model.

Once he had clearly chained himself gratuitously to the universal wish monopoly of dream generation, his interpretations of dreams were constrained to reconcile wish-contravening dreams with the decreed universality of wish fulfillment. Such reconciliation demanded imperiously that all other parts and details of his dream theory be obligingly tailored to the governing wish dogma so as to sustain it. Yet Freud artfully obscured this dynamic of theorizing while begging the methodological question (1900/1958b, p. 135). Wish-contravening dreams include anxiety dreams, nightmares, and the special, so-called “counterwish dreams” (p. 157). As an example of the latter, Freud reports a trial attorney’s dream that he had lost all of his court cases (p. 152).

Freud’s initial 1900 statement of his dual wish fulfillment in dreams had been: “Thus, its content was the fulfillment of a wish and its motive was a wish” (p. 119). But the sense in which dreams are wish fulfilling overall is purportedly threefold rather than only twofold: One motivating cause is the universal preconscious wish to sleep, which supposedly provides a generic causal explanation of dreaming as such and, in turn, makes dreaming the guardian of sleep (1900/1958b, pp. 233–234; 1900/1958c, p. 680). Another is the individualized repressed infantile wish, which is activated by the day’s residue and explains the particular manifest content of a given dream; furthermore, as already noted, that manifest content of the dream graphically displays, more or less disguisedly, the state of affairs desired by the unconscious wish. The disguise is supposedly affected by the defensive operation of the “dream-distortion” of the content of forbidden unconscious wishes.

But this theorized distortion of the hypothesized latent content must not be identified with the very familiar phenomenological bizarreness of the manifest dream content. That bizarreness stands in contrast to the stable configurations of ordinary waking experiences. By achieving a compromise with the repressed wishes, the postulated distortion makes “plausible that even dreams with a distressing content are to be construed as wish fulfillments” (Freud, 1900/1958b, p. 159). Accordingly, Freud concedes: “The fact that dreams really have a secret meaning which represents the fulfillment of a wish must be proved afresh in each particular case by analysis” (p. 146).

But in a 1993 book (Grünonbaum, chap. 10; and in Grünonbaum, forthcoming), I have argued that this dream theory of universal wish fulfillment should be presumed to be false at its core rather than just ill founded.

More conservatively, the psychoanalysts Jacob Arlow and Charles Brenner (1964) claimed, for reasons of their own, “A dream is not simply the visually or auditorily hallucinated fulfillment of a childhood wish” (Arlow & Brenner, 1988, p. 7). And they
countenanced a range of dream motives other than wishes, such as anxiety, though ultimately still rooted in childhood (p. 8).

But this modification did not remedy the fundamental epistemological defect in the claim that the method of free association can reliably identify dream motives. Undaunted, Arlow and Brenner (1988) declare: “The theory and technique of dream analysis [by free association] in no way differs from the way one would analyze...a neurotic symptom...a parapraxis...or any other object of [psychoanalytic scrutiny]” (p. 8). By the same token, these analysts insouciantly announce: “Dreams are, in fact, compromise formations like any others” (p. 7–8). Yet this ontological conclusion is predicated on the ill-founded epistemological thesis that free associations reliably identify repressions to be the causes of symptoms, dreams, and slips.

Careful studies have shown that the so-called free associations are not free but are strongly influenced by the psychoanalyst’s subtle promptings to the patient (Grünbau, 1984, pp. 211–212). And recent memory research has shown further how patients and others can be induced to generate pseudo-memories, which are false but deemed veridical by the patients themselves (Goleman, 1994).

As a corollary of the latter epistemological defects of the method of free association, it appears that such associations cannot reliably vouch for the contents of presumed past repressions that are lifted by them. Thus, the products of such associations cannot justify the following repeated claim of the later (post-1923) Freud: The mere painfulness or unpleasurableness of an experience is not itself the prime motive for its repression; instead, its negativity must involve the conscious emergence of an instinctual desire recognized by the superego as illicit or dangerous (1933/1964c, lecture XXXII, esp. pp. 89–90, 94).

But because Freud had also stressed the well-nigh universal tendency to forget negative experiences per se, his later view of the dynamics of repression disappointingly leaves dangling theoretically (1) the relation of forgetting to repression, and (2) why some forgettings, no less than repressions, supposedly cannot be undone without the use of the controlled method of free association. In James Strachey’s Standard Edition (1901/1960), the general index lists two subcategories, among others, under “Forgetting”: (1) “motivated by avoidance of unpleasure” and (2) “motivated by repression” (p. 301). But alas, Freud himself leaves us in a total quandary whether these two categories of Strachey’s represent a distinction without a difference.

The Explanatory Pseudo-Unification Generated by Freud’s Compromise Model of Neuroses, Dreams, and Slips

My indictment of the compromise model, if correct, spells an important lesson, I claim, for both philosophical ontology and the theory of scientific explanation. Advocates of psychoanalysis have proclaimed it to be an explanatory virtue of their theory that its compromise model gives a unifying account of such prima facie disparate domains of phenomena as neuroses, dreams, and slips, and indeed that the theory of repression also illuminates infantile sexuality and the four stages hypothesized in Freud’s theory of psychosexual development. In fact, some philosophers of science, such as Michael Friedman, have hailed explanatory unification as one of the great achievements and desiderata of the scientific enterprise. Thus, one need only think of the beautiful way in which Newton’s theory of mechanics and gravitation served all at once to explain the
motions of a pendulum on earth and of binary stars above by putting both terrestrial and celestial mechanics under a single theoretical umbrella.

Yet, in other contexts, unification can be a vice rather than a virtue. Thales of Miletus, though rightly seeking a rationalistic, rather than mythopoetic, picture of the world, taught that everything is made of water. And other philosophical monists have enunciated their own unifying ontologies. But the Russian chemist Dmitry Mendeleyev might have said to Thales across the millennia in the words of Hamlet: “There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy” (Shakespeare, Hamlet, Act I, Scene V).

As I have argued, the same moral applies to Freud: By invoking the alleged causal cogency of the method of free association as a warrant for his compromise model, he generated a pseudo-unification of neurotic behavior with dreaming and the bungling of actions. This dubious unification was effected by conceiving of the normal activities of dreaming and occasionally bungling actions as mini-neurotic symptoms, of a piece with abnormal mentation in neuroses and even psychoses. To emphasize this monistic psychopathologizing of normalcy, Freud pointedly entitled his magnum opus on slips The Psychopathology of Everyday Life (1901/1960). To this I can only say in metaphorical theological language: “Let no man put together what God has kept asunder,” a gibe that was used by Wolfgang Pauli, I believe, against Einstein’s unified field theory.

The “Hermeneutic” Reconstruction of Psychoanalysis

The French philosopher Paul Ricoeur (1970, p. 358), faced with quite different criticisms of psychoanalysis from philosophers of science during the 1950s and 1960s (von Eckardt, 1985, pp. 356–364), hailed the failure of Freud’s theory to qualify as an empirical science by the received standards as the basis for “a counterattack” against those who deplore this failure. In concert with the so-called hermeneutic German philosophers Karl Jaspers and Jürgen Habermas, Ricoeur believed that victory can be snatched from the jaws of the scientific failings of Freud’s theory by abjuring his scientific aspirations as misguided. Claiming that Freud himself had “scientistically” misunderstood his own theoretical achievement, some hermeneuts misconstrue it as a semantic accomplishment by trading on the multiply ambiguous word “meaning” (Grünbaum, 1984; 1990; 1993, pp. 109–166). In Freud’s theory, an overt symptom manifests one or more underlying unconscious causes and gives evidence for its cause(s), so that the “sense” or “meaning” of the symptom is constituted by its latent motivational cause(s). But this notion of “meaning” is different from the one appropriate to the context of communication, in which linguistic symbols acquire semantic meaning by being used deliberately to designate their referents. Clearly, the relation of being a manifestation, which the symptom bears to its cause, differs from the semantic relation of designation, which a linguistic symbol bears to its object.

The well-known academic psychoanalyst Marshall Edelson (1988) is in full agreement with this account and elaborates it lucidly:

For psychoanalysis, the meaning of a mental phenomenon is a set of unconscious psychological or intentional states (specific wishes or impulses, specific fears aroused by these wishes, and thoughts or images which might remind the subject of these wishes and fears). The mental phenomenon substitutes for this set of states. That is, these states would have been present in consciousness, instead of the mental phenomenon requiring interpretation, had they not encountered, at the time of origin of the mental phenomenon or repeatedly since then,
obstacles to their access to consciousness. If the mental phenomenon has been a relatively enduring structure, and these obstacles to consciousness are removed, the mental phenomenon disappears as these previously unconscious states achieve access to consciousness. (pp. 246–249)

That the mental phenomenon substitutes for these states is a manifestation of a causal sequence (Edelson, 1988, pp. 247–248). And drawing on Freud’s compromise model of symptoms in which symptoms are held to provide substitutive outlets or gratifications, Edelson continues:

Suppose the question is: “Why does the analysand fear the snake so?” Suppose the answer to that question is: “A snake stands for, or symbolizes, a penis.” It is easy to see that by itself this is no answer at all; for one thing, it leads immediately to the question: “Why does the analysand fear a penis so?” The question is about an inexplicable [unexplained] mental phenomenon (i.e., “fearing the snake so”) and its answer depends on an entire causal explanation. . . . “A snake stands for, or symbolizes, a penis” makes sense as an answer only if it is understood as shorthand for a causal explanation. . . . Correspondingly, “the child stands for, or symbolizes, the boss” is not a satisfactory answer (it does not even sound right) to the question, “Why does this father beat his child?” (p. 249)

For my part, in this context I would wish to forestall a semantic misconstrual of the perniciously ambiguous term “symbol” by saying: In virtue of the similarity of shape, the snake causally evokes the unconscious image of a feared penis; thereby the snake itself becomes a dreaded object.

Speaking of Freud’s writings, Edelson (1988) says illuminatingly:

Certain passages (occasional rather than preponderant) allude, often metaphorically, to symbolizing activities in human life. I think it could be argued that these indicate an effort on Freud’s part to clarify by analogy aspects of the subject matter he is studying, including in some instances aspects of the clinical activity of the psychoanalyst—while at the same time perhaps he paid too little attention to disanalogies—rather than indicate any abandonment on his part of the [causally] explanatory objectives he so clearly pursues. There is no more reason to suppose that just because Freud refers to language, symbols, representations, and symbolic activity (part of his subject matter), he has rejected, or should have rejected, canons of scientific method and reasoning, than to suppose that just because Chomsky studies language (his subject matter), his theory of linguistics cannot be a theory belonging to natural science and that he cannot be seeking causal explanations in formulating it. (p. 247)

The “hermeneutic” reconstruction of psychoanalysis slides illicitly from one of two familiar senses of “meaning” encountered in ordinary discourse to another. When a pediatrician says that a child’s spots on the skin “mean measles,” the “meaning” of the symptom is constituted by one of its causes, much as in the Freudian case. Yet, the analyst Anthony Storr (1986, p. 260), when speaking of Freud’s “making sense” of a patient’s symptoms, conflates the fathoming of the etiologic “sense” or “meaning” of a symptom with the activity of making semantic sense of a text (Grünbaum, 1986, p. 280), declaring astonishingly: “Freud was a man of genius whose expertise lay in semantics.” And Ricoeur incongruously credits Freud’s theory of repression with having provided, malgré lui, a veritable “semantics of desire.”

Achim Stephan (1989, pp. 144–149) takes issue with some of my views (Grünbaum, 1990, 1993, chap. 4). (Quotations from Stephan below are my English translations of his German text.) He does not endorse Ricoeur’s “semantics of desire” (p. 123). But he objects to my claim that “In Freud’s theory, an overt symptom manifests one or more
underlying unconscious causes and gives evidence for its cause(s), so that the ‘sense’ or ‘meaning’ of the symptom is constituted by its latent motivational cause(s)” (p. 146, item [3]).

As Stephan recognizes (1989, p. 27), Freud (1913/1958a, pp. 176–178) avowedly “overstepped” common usage when he generalized the term “language” to designate not only the verbal expression of thought but also gestures “and every other method...by which mental activity can be expressed” (p. 176). And Freud declared that “the interpretation of dreams [as a cognitive activity] is completely analogous to the decipherment of an ancient pictographic script such as Egyptian hieroglyphs” (p. 177). But surely this common challenge of problem solving does not license the assimilation of the psychoanalytic meaning of manifest dream content to the semantic meaning of spoken or written language (Grünbaum, 1993, p. 115).

Stephan does countenance (1989, p.148) my emphasis on the distinction between the relation of manifestation, which the symptom bears to its cause, and the semantic relation of designation, which a linguistic symbol bears to its object. Yet, his principal objection to my view of the psychoanalytic “sense” of symptoms as being causal manifestations of unconscious ideation is that I assign “exclusively nonsemantic significance” to them by denying that they also have “semiotic” significance like linguistic symbols (pp. 148–149). He grants that Freud did not construe the sense or meaning of symptoms as one of semantic reference to their causes. Yet according to Stephan’s own reconstruction of Freud’s conception, “He did assume that the manifest phenomena [symptoms] semantically stand for the same thing as the (repressed) ideas for which they substitute”; that is, “they stand semantically for what the repressed (verbal) ideas stand (or rather would stand, if they were expressed verbally)” (p. 149).

Searle (1990, pp. 161–167) has noted illuminatingly (p. 175) that, unlike many mental states, language is not intrinsically “intentional” in Brentano’s directed sense; instead, the intentionality (aboutness) of language is extrinsically imposed on it by deliberately “decreeing” it to function referentially. Searle (pp. 5, 160, 177) points out that the mental states of some animals and of “prelinguistic” very young children do have intrinsic intentionality but no linguistic referentiality.

I maintain that Stephan’s fundamental hermeneuticist error was to slide illicitly from the intrinsic, nonsemantic intentionality of (many, but not all) mental states to the imposed, semantic sort possessed by language. Moreover, some of the neurotic symptoms of concern to psychoanalysts, such as diffuse depression and manic, undirected elation even lack Brentano intentionality.

Finally, the aboutness (contents) of Freud’s repressed conative states is avowedly different from the intentionality (contents) of their psychic manifestations in symptoms (compromise formations). But Stephan erroneously insists that they are the same.

Yet some version of a hermeneutic reconstruction of the psychoanalytic enterprise has been embraced with alacrity by a considerable number of analysts no less than by professors in humanities departments of universities. Its psychoanalytic adherents see it as buying absolution for their theory and therapy from the criteria of validation mandatory for causal hypotheses in the empirical sciences, although psychoanalysis is replete with just such hypotheses. This form of escape from accountability also augurs ill for the future of psychoanalysis, because the methods of the hermeneuts have not spawned a single new important hypothesis. Instead, their reconstruction is a negativistic ideological battle cry whose disavowal of Freud’s scientific aspirations presages the death of his legacy from sheer sterility, at least among those who demand the validation of theories by cogent evidence.
But what have been the contemporary post-Freudian developments insofar as they still qualify as psychoanalytic in content rather than only in name? And have they advanced the debate by being on firmer epistemological ground than Freud’s original major hypotheses (Grüñbaum, 1984, chap. 7)? Most recently, the noted clinical psychologist and philosopher of psychology Morris Eagle (1993) has given a comprehensive and insightful answer to this question on which we can draw.

Eagle (1993) begins with a caveat:

> It is not at all clear that there is a uniform body of thought analogous to the main corpus of Freudian theory that can be called contemporary psychoanalytic theory. In the last 40 or 50 years there have been three major theoretical developments in psychoanalysis: ego psychology, object relations theory, and self-psychology. If contemporary psychoanalytic theory is anything, it is one of these three or some combination, integrative or otherwise, of the three. (pp. 374)

Eagle makes no mention of Lacan’s version of psychoanalysis, presumably because he does not take it seriously, as Lacanians have avowedly forsaken the need to validate their doctrines by familiar canons of evidence, not to mention Lacan’s deliberate, irresponsible obscurity and notorious cruelty to patients (Green, 1995/1996).

Previously, we had occasion to note that Heinz Hartmann’s ego psychology departed from Freud’s instinctual anchorage of the cognitive functions. But, more important, both Heinz Kohut’s self psychology and the object relations theory of Otto Kernberg and the British school more fundamentally reject Freud’s compromise model of psychopathology. Indeed, self psychology has repudiated virtually every one of Freud’s major tenets (Eagle, 1993, p. 388). Thus, Kohut supplants Freud’s conflict model of psychopathology, which is based on the repression of internal sexual and aggressive wishes, by a psychology of self-defects and faulty function caused by hypothesized *environmental events* going back to the first two years of infancy. Relatedly, Kohut (1984) denies, contra Freud, that insight is curative, designating instead the analyst’s empathic understanding as the operative therapeutic agent. Again, the object relations theorists deny that the etiology of pathology lies in Freudian (Oedipal) conflicts and traumas involving sex and aggression, claiming instead that the quality of maternal caring is the crucial factor.

Yet these two post-Freudian schools not only diverge from Freud but also disagree with each other. Thus, the orthodox psychoanalysts Arlow and Brenner speak ruefully of “the differences among all these theories, so apparent to every observer” (1988, p. 9), hoping wistfully that refined honing of the psychoanalytic method of free association will yield a common body of data, which “would in the end resolve the conflict among competing theories” (p. 11). But their hope is utopian, if only because of the severe probative limitations of the method of free association. How, for example, could a method of putting adults on the couch possibly have the epistemological resources to resolve the three-way clash among the Freudian and two post-Freudian schools in regard to the *infantile* etiologies of psychopathology? Otto Kernberg’s (1993) account of the “Convergences and Divergences in Contemporary Psychoanalytic Technique” does not solve that problem. And, as other psychoanalysts themselves have documented, there are several clear signs that the future of the sundry clinical and theoretical enterprises that label themselves “psychoanalytic” is now increasingly in jeopardy. For example, the pool of patients seeking (full-term) psychoanalytic treatment in the United States has been steadily shrinking, and academic psychoanalysts are becoming an endangered species in

But what about the evidential merits of the two post-Freudian developments that are usually designated as “contemporary psychoanalysis”? Do they constitute an advance over Freud? The answer turns largely, though not entirely, on whether there is better evidential support for them than for Freud’s classical edifice. But Eagle (1993, p. 404) argues that the verdict is clearly negative: “The different variants of so-called contemporary psychoanalytic theory...are on no firmer epistemological ground than the central formulations and claims of Freudian theory.... There is no evidence that contemporary psychoanalytic theories have remedied the epistemological and methodological difficulties that are associated with Freudian theory.”

**Conclusion**

Since the psychoanalytic enterprise is now in its second century, it behooved us to take thorough stock of its past performance qua theory of human nature and therapy, as well as to assess its prospects. In this essay, I have offered such a critical appraisal from a philosophy of science perspective.

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