

**PSYCHOANALYTIC THEORY
AND THERAPY
AFTER 100 YEARS,
AND ITS FUTURE**

ADOLF GRÜNBAUM

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*Opening Statement for Adolf Grünbaum's
Keynote Address at the Latsis Prize Laureates Ceremony;
Geneva, October 7, 1994.*

Je suis très reconnaissant à Monsieur Thorens pour les mots si gentils par lesquels il vient de me présenter. Je tiens à vous dire que c'est un très grand honneur pour moi de vous parler à cette occasion si importante.

Je dois m'excuser parce que ma connaissance de la langue française ne me permet pas de m'adresser à vous en français. Mon discours sera donc en anglais, comme cela est annoncé dans le Programme.

Je suis très touché par le geste exquis de mon ami Spiro Latsis, qui a bien voulu m'inviter ici, aujourd'hui. Je le remercie vivement, comme je remercie Madame Latsis, pour avoir invité aussi ma femme Thelma. Leur hospitalité est parfaite, ainsi que leur amitié l'a été pendant si longtemps.

Psychoanalytic Theory and Therapy After 100 Years, and Its Future

Introduction

The most basic ideas of psychoanalytic theory were initially enunciated in Josef Breuer's and Sigmund Freud's "Preliminary Communication" of 1893, which introduced their *Studies in Hysteria*. But the first published use of the word "psychoanalysis" occurred in Freud's 1896 French paper on "Heredity and the Etiology of the Neuroses" (S.E. 1896, 3: 151¹). Therein Freud designated Breuer's method of clinical investigation as "a new method of psychoanalysis". 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.

¹ All citations of Freud's writings in English will be from the *Standard Edition of the Complete Psychological Works of Sigmund Freud*, trans. by J. Strachey et al. London: Hogarth Press, 1953-1974, 24 volumes. Each reference will use the abbreviation "S.E.," followed by the year of first appearance, volume number, and page(s).

The cathartic *lifting* of the repression yielded relief from the particular hysterical symptom. Breuer and Freud believed that they could therefore hypothesize that the *repression itself* was the crucial cause for the development of the patient's psychoneurosis (S.E. 1893, 2: 6-7; 1893, 3: 29-30).

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 psycho-analysis (S.E. 1924, 19: 194).

In a 1924 historical retrospect (S.E. 1924, 19: 194), Freud acknowledged the pioneering role of Breuer's cathartic method:

The cathartic method was the immediate precursor of psycho-analysis; and, in spite of every extension of experience and of every modification of theory, is still contained within it as its nucleus.

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 (S.E. 1924, 19: 195).

These extensive elaborations have earned Freud the mantle of being the *father* of psychoanalysis.

Last year was the centenary of Breuer and Freud's "Preliminary Communication". The end of the first century of psychoanalysis was marked here in Geneva by an international congress on the topic *100 Years of Psychoanalysis*. Its proceedings were published this year in a Special Issue of the *Cahiers Psychiatriques Genevois* (Haynal & Falzedex, 1994). As shown by its subtitle, this volume is

devoted to the *history* of psychoanalysis, and its contributors are either practicing psychoanalysts or intellectual supporters of Freud's legacy.

But, in my view, at this centenary juncture, the time has also come to take thorough **critical** stock of the past performance of the psychoanalytic enterprise qua theory of human nature and therapy, as well as to have a look at its prospects. And I wish to take this welcome opportunity to do so, though, of course, only in broad strokes. French-speaking members of the audience interested in the details of my earlier critiques can find them in French translation in two of my books: One is *La Psychanalyse à l'Épreuve*, published in 1993 in Paris by *Editions de L'Éclat*, and the other is *Les Fondements de la Psychanalyse, une Critique Philosophique*, which is about to be published in Paris by Presses Universitaires de France.

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

Although Freud was the creator of the full-blown theory of psychoanalysis, even well-educated people often don't 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 in order to explain conscious thought and overt behavior for which they could find no other explanation (S.E. 1915, 14: 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. And 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 merely restoring it to his conscious memory.

At the turn of the 18th century, Leibniz gave psychological arguments for the occurrence of *sub-threshold* sensory perceptions, and for the existence of unconscious mental contents or motives that manifest themselves in our behavior (Ellenberger, 1970, p. 312). Moreover, Leibniz pointed out (1981, p. 107) 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 he put it (1981, p. 107):

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.

As Rosemarie Sand has pointed out, Leibniz's notion anticipates Freud's dictum that "*The interpretation of dreams is the royal road to a knowledge of the unconscious activities of the mind*" (S.E. 1900, 5: 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 can experience an object as moving in our visual field from one point to another, when a stroboscope successively flashes an *image* of it at a suitable rate at these points. Yet actually no such object has moved. It would seem that the viewer *unconsciously inferred* the illusory motion from the stroboscopic data, but without being aware at all of any such inference.

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 (S.E. 1914, 14: 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.

As background for 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, or 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 re-entry 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 (S.E. 1900, 5: 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 them. Thus, as Eagle (1987, p. 162) 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 (Eagle, 1993, pp. 374-376). 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 Czarina's lover Rasputin when I am asked for it. Helmholtz's process of "unconscious inference" of illusory stroboscopic movement illustrates the same inability. 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.

Other 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, very recently the Chicago analyst Michael Franz Basch (1994, p. 1) reasoned in vain that since 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.

In appraising psychoanalysis, we must also beware of yet another logical blunder that has recently become fashionable: The bizarre argument that the supposed pervasive influence of Freudian ideas in Western culture vouches for the validity of the psychoanalytic enterprise. Just this astonishing reasoning was put forward this year by the American philosopher Thomas Nagel (May 12, 1994) in an article entitled “Freud’s Permanent Revolution” in the *New York Review of Books*. His view is largely shared by the philosophers Richard Rorty and Ian Hacking. But, as I pointed out in my published reply to Nagel (Grünbaum, August 11, 1994), his argument is untenable.

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, 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 (S.E. 1916, 15: 40). And he added commendably that neither he nor his followers deserve any credit for the motivational explanations of such perspicuous slips (S.E. 1916, 15: 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 stifflingly 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, furthermore, 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* (S.E. 1916, 15: 41). As the father of psychoanalysis declared unambiguously (S.E. 1901, 6: 239): 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’”. 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 (S.E. 1901, 6: 9). *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 which *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. And, as long as good empirical support for this 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 genuinely Freudian slips indeed don't even exist, then Freud's theory of them is simply false. And if so, it would not contribute one iota to its validity, even if our entire culture unanimously believed in it, and made extensive explanatory use of it. When an ill-supported or false 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 living art historian Meyer Schapiro of Columbia University. Schapiro sees 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 subjects 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” (Solomon, 1994, p. 24). Reportedly, “his insights into ... the apples of Cézanne” (Solomon, 1994, 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 do likewise? Yet we learn that Schapiro’s 1968 publication “The Apples of Cézanne” is “His best known essay” (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, I must say that Schapiro’s psychodiagnosis is an instance of what Freud himself deplored as “Wild Psycho-Analysis” (S.E. 1910, 11: 221-227). In any case, *pace* Nagel, such art-historical invocation of Freud, however influential, does nothing, I claim, toward 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 “epilepsis” *for seizure*, and reflects etymologically the notion of being seized by a demon. Since exorcism is designed to drive out the devil, it is the supposed *therapy* for demonic possession. In the Roman Catholic exorcist ritual, which has been endorsed by the present Pope and by 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 or consecrating houses.

But 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*" (S.E. 1933, 22: 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" (S.E. 1933, 22: 152). Clearly, the psychoanalytic and theological accounts 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.

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".

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" (S.E. 1914, 14: 16). The three principal branches of the theory of repression are (1) sets of hypotheses as to the unconscious causation and psychoanalytic treatment of psychopathology, (2) the theory of dreams, and (3) the previously outlined theory of slips or parapraxes. In each of these three branches, the repression of mental contents is asserted to play a *causally necessary* role: It is crucial to the production of neuroses and psychoses by unconscious sexual motives, to the formation of dreams by latent infantile wishes, and to the generation of bungled actions by diverse hidden motives of displeasure.

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 (S.E. 1915, 14: 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" (S.E. 1925, 20: 45; 1917, 16: 301). By being only such compromises, rather than fulfillments of the instinctual impulses, these products of the unconscious afford only *substitutive* gratifications or outlets. For brevity, one can say, therefore, that Freud has offered a unifying "compromise-model" of neuroses, dreams and parapraxes.

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*. Further on, I shall have occasion to comment on the merits of Freud's more complicated account of the motives of repressions. Indeed, Freud claimed perennially that repression is the paragon among our *defense* mechanisms (Thomä & Kächele, 1987, vol. 1, pp. 107-111). As he put it dogmatically: "The tendency to forget what is disagreeable seems to me to be a quite universal one" (S.E. 1901, 6: 144), and "the recollection of distressing impressions and the occurrence of distressing thoughts are opposed by a resistance" (S.E. 1901, 6: 146).

Freud tries to disarm an important objection to his thesis that “distressing memories succumb especially easily to motivated forgetting” (S.E. 1901, 6: 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” (S.E. 1916, 15: 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 (S.E. 1900, 4: 216) that he “can remember very clearly”, from age seven or eight, 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”.

Since some painful mental states are vividly remembered while others are forgotten or even repressed, I claim contra Freud 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 on the tenability of the following pillar of his theory of repression: In his view, when painful or forbidden experiences are forgotten, the forgetting is often tantamount to their

repression *due to their negative affect*, and thereby produces neurotic symptoms or other compromise-formations.

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*, while 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 due to 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 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] (S.E. 1916, 15: 76) ...; there is room for both. It is only a question ... of what effects are produced by the one and the other" (p. 77).

Indeed, just that question cries out for an answer from Freud, if he is to make his case. Instead, he cavalierly left it dangle epistemologically in limbo.

Another basic difficulty, which besets all three 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 flood gates 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 two-fold major capability, which is both investigative and therapeutic: (i) It can *identify* the unconscious causes of human thoughts and behavior, both abnormal and normal, and (ii) By overcoming resistances and lifting repressions, it can remove the supposed 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 investigative 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 true*. Freud tells us very clearly (S.E. 1900, 5: 528) that his argument for his investigative tribute to free association is, at bottom, a *therapeutic* one going back to the cathartic method of treating hysteria. Let me articulate his argument.

First, Freud inferred that the therapeutic disappearance of the neurotic symptoms is *causally attributable* to the lifting of repressions **by means of the method free associations**. Relying on this key therapeutic hypothesis, he then drew two further major theoretical inferences: (i) The seeming removal of the neurosis by means of *lifting* repressions is good evidence for postulating that

repressions are themselves *causally necessary* for the very existence of a neurosis (S.E. 1893, 2: 6-7), and (ii) Granted that repressions are thus the essential causes of neurosis, *and* that the method of free association is uniquely capable of uncovering the repressions, this method is uniquely competent *to identify the causes* or pathogens of the neuroses. This, then, is Freud's argument for claiming that the investigative method he pioneered to improve upon hypnosis is etiologically probative. We saw that the crucial point of departure of this argument is a hypothesis pertaining to the dynamics of therapy in which the method of free association plays an indispensable role.

Having convinced himself of the causal probativeness of the method 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 – which he considered psychoanalytically *untreatable*.

But I claim that his therapeutic argument for the cogent etiologic probativeness of free associations fails multiply, no matter how revealing the associative contents may otherwise be in regard to the patient's psychological preoccupations or personality dispositions.

In the first place, the durable therapeutic success on which the argument was predicated did not materialize, as Freud was driven to admit both early and very late in his career (S.E. 1925, 20: 27; 1937, 23, 216-253). But even in so far as there was transitory therapeutic gain, 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. To rule out that rival hypothesis, it would have been necessary to show that in a control group of similar patients whose repressions are *not* lifted, the treatment-outcome was *worse* than in the psychoanalytically treated group. No such data have become available during the past century.

Thus, in a 55-page study “On the Efficacy of Psychoanalysis” (Bachrach *et al.*, 1991), published in 1991 in the official *Journal of the American Psychoanalytic Association*, the authors admit that their study of outcome from psychoanalytic treatment does “not enable meaningful comparisons of the effectiveness of psychoanalysis with that of other forms of treatment for specific kinds of patients experiencing specific kinds of illnesses” (p. 911). Evidently, this most recent and comprehensive study *did nothing* to discredit the hypothesis of placebo effect. And it also failed to control for the spontaneous remission of some disorders, or for the placebo effects from help given by *non*-professionals.

But Freud told us very clearly (S.E. 1900, 5: 528) that the etiologic probativeness of the method of free association rests on the attribution of treatment-gain to the insightful removal of repressions. And since the viability of the rival hypothesis of placebo effect rightly challenges that attribution, his stated argument for the etiologic probativeness of free association forfeits its foundation.

Yet, as we learn in 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 (S.E. 1900, 4: 100-101). 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 two-fold extrapolation to include slips or bungled actions. Having extrapolated the method of free association to interpret dreams, he then *also* offered a *non-therapeutic* argument for its cogency as a means of identifying their hidden motives. But, as I have shown elsewhere (Grünbaum 1984, ch. 5) that argument is, alas, no more than a piece of salesmanship.

But what do free associations tell us about our dreams? Whatever the manifest content of dreams, they are *purportedly wish-fulfilling* in two logically distinct ways as follows: For every dream D , there exists at least one normally unconscious infantile wish W such that (i) W is the motivational cause of D , and (ii) the manifest content of D graphically displays, more or less disguisedly, the state of affairs desired by W . But in a 1993 book (Grünbaum, 1993, ch. 10), I have argued that this theory should be presumed to be false at its core.

Well before then, the psychoanalysts Jacob Arlow and Charles Brenner (1964) had claimed, for reasons of their own, that “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 their 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 declare (1988, p. 8): “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 [psycho]analytic scrutiny”. By the same token, these analysts insouciantly announce: “Dreams are, in fact, compromise-formations

like any others” (pp. 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.

Indeed, 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 (Grünbaum, 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 (*New York Times*, May 31, 1994, Section B).

As a corollary of these multiple 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 serve to 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 that is recognized by the super-ego as illicit or dangerous (S.E. 1940, 23: 184-187; 1933, 22: 57, 89, 91, 94; 1937, 23: 227).

But since 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 (i) the relation of forgetting to repression, and (ii) 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*, (S.E. 1901, 6: 301), the General Index lists two subcategories, among others, under “Forgetting”: (i) “motivated by avoidance of unpleasure”, and (ii) “motivated by repression”. But

alas, Freud himself leaves us in a total quandary whether these two categories of Strachey's represent a distinction without a difference.

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 chemist Mendeleev 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 relying on the causal cogency of the method of free association, his compromise-model has 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-piecc 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* (S.E. 1901, 6). 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 French philosopher Paul Ricoeur (1970, p. 358), faced with quite different criticisms of psychoanalysis from philosophers of science during the 1950’s and 1960’s (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 counter-attack” against those who deplore this failure. In concert with the other 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, these philosophers misconstrue it as a *semantic* accomplishment by trading on the weasel word “meaning”. In Freud’s account, 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 intentionally 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. Yet, disastrously, the word “meaning” is used for both.

The “hermeneutic” reconstruction of psychoanalysis slides illicitly from one of two familiar senses of “meaning” encountered in ordinary discourse to another. When a parent is told by a pediatrician 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. But when a bus driver tells us that three rings of his bell “mean” that the bus is full, these rings – unlike the symptoms of measles or neurotic symptoms – are *intended* to communicate a certain state of affairs. Thus, the analyst Anthony Storr (1986, p. 260) confuses 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”. That statement is patently false. Similarly, Ricoeur erroneously credits Freud’s theory of repression with having provided, *malgré lui*, a veritable “semantics of desire.”

Yet the proposed 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 psychoanalytic 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 (Grünbaum, 1984, ch. 7)? Most recently, the 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, p. 374) 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 forty or fifty 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".

Previously (Section II), we had occasion to note that Heinz Hartmann's ego-psychology departed from Freud's instinctual anchorage of the cognitive functions. But, more importantly, both Heinz Kohut's self-psychology and the object relations theory of Otto Kernberg and the British school 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 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 one another. Thus, the orthodox psychoanalysts Arlow and Brenner speak ruefully of "the differences among all these theories, so apparent to every observer" (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 associations. How, for example, could a method of putting adults on the couch possibly have the epistemological resources to resolve the 3-way clash between the Freudian and two post-Freudian schools in regard to the *infantile* etiologies of psychopathology? 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 American medical schools (Reiser, 1989). No wonder that the subtitle of the 1988 book *Psychoanalysis* by the well-known analyst Marshall Edelson is "*A Theory in Crisis*" (Edelson, 1988).

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 (p. 404): "...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".

Finally, what are the prospects for the future of psychoanalysis in the 21st century? In their 1988 paper on that topic, the psychoanalysts Arlow and Brenner (1988, p. 13) reached the following sanguine conclusion about both its past and its future:

Of some things about the future of psychoanalysis we can be certain. Fortunately, they are the most important issues as well. Psychoanalysis will continue to furnish the most comprehensive and illuminating insight into the human psyche. It will continue to stimulate research and understanding in many areas of human endeavor. In addition to being the best kind of treatment for many cases, it will remain, as it has been, the fundamental base for almost all methods that try to alleviate human mental suffering by psychological means.

As I hope I have shown, in regard to the last 100 years, this partisan account is very largely and seriously ill-founded, if only because the lauded comprehensiveness of the core theory of repression is only a *pseudo*-unification, as I have argued. Among Arlow and Brenner's glowingly optimistic statements about the future, just one is plausible: The expectation of a continuing heuristic role for psychoanalysis. Such a function does *not* require the correctness of its current theories at all. One need only think of the unresolved heuristic issues I raised à propos of Freud's dubious account of the relation of affect to forgetting and remembering. These issues range well

beyond the concerns of psychoanalysis. As the Harvard psychoanalyst and schizophrenia researcher Philip Holzman sees it (Holzman, 1994, p. 190): “This view of the heuristic role of psychoanalysis, even in the face of its poor science, is beginning to be appreciated only now”.

Acknowledgement

Rosemarie Sand pointed out to me that especially after Freud had introduced the concept of the super-ego (1923), he repeatedly held that if a negative or painful experience is to be repressed, its unpleasure must involve the prohibition of an instinctual desire by the super-ego.

Her exegetical *caveat* prompted my comment on this view of Freud’s within the text.

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