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The Psychoanalytic Enterprise in Scientific Perspective

1. Introduction

It is well known that Freud was hardly the first thinker who postulated *unconscious* processes in order to explain much conscious psychic life and overt conduct. In Plato's dialogue the *Meno,* we encounter a slave boy who had never studied geometry. Yet by just showing him a diagram and asking him appropriate questions, his interlocutor was able to elicit geometric truths from him. This phenomenon is then used by Plato to interpret the acquisition of conscious knowledge as the recall of information, which the soul had unconsciously stored in the meantime.

In the early nineteenth century, the German philosopher Johann Herbart, who died fifteen years before Freud was born, taught that conscious mental life is affected by subliminal processes, which function like ideas, except for being beneath the threshold of focal awareness (Fancher 1973, 12). Moreover, Arthur Schopenhauer had claimed, *before* Freud, that consciousness resists the intrusion of unpleasant thoughts and perceptions. Indeed, Schopenhauer was Freud's precursor in the field of psychopathology even to the extent of enunciating, in very general terms, that repressed ideation is pathogenic (Ellenberger 1970, 209)! No wonder that Thomas Mann had a sense of déjà vu when he read Freud, after delving into Schopenhauer. Furthermore, Eduard von Hartmann published his *Philosophy of the Unconscious* when Freud was thirteen years of age. There, von Hartmann gave wide explanatory scope to unconscious processes (Ellenberger 1970, 210).

The founding father of psychoanalysis died in 1939. Several decades thereafter, new experimental findings have prompted cognitive psychologists to conclude that unconscious ideation plays a cognitive role in mental life that even Freud had not envisioned. In fact, despite some similarity between the *cognitive* unconscious of recent psychology and Freud's *dynamic* unconscious, there are also important differences between them that should not be glossed over. For example, the psychoanalytic unconscious is *affect*-laden, and its contents are deemed to be recoverable by lifting their repression. By contrast, the implicit problem-solving capabilities of the cognitive unconscious are neither repressed nor conscious. Thus, therapists who have used subliminal techniques to increase self-esteem, or to induce weight loss, are feeling reassured by the new recognition that a substantial portion of *cognitive* activity is, in fact, unconscious.

But one factor that may have made psychoanalytic theory so extraordinarily influential in some segments of our culture was Freud's particular articulation of the assumed *causal* role of unconscious processes. It is a measure of this influence, at least in the United States, that the Science section of the *New York Times* (January 24, 1984), no less than the *New Yorker, Time,* and other such magazines, gave wide publicity to Jeffrey Masson's book (1984) about Freud, which purports to contain highly derogatory revelations about his lack of intellectual integrity. To boot, Janet Malcolm (1981) gave prominence to Peter Swales' allegation that one of Freud's paradigmatic examples of a memory lapse pertained to a shady episode involving Freud himself, who had supposedly impregnated his own sister-in-law, and had then taken her to Italy for an abortion. Indeed, the intellectual and cultural historian Peter Gay (1985, 1988)—to name only one—invokes psychoanalytic theory *uncritically,* as if it were holy writ. And he applies it to generate so-called psychohistory (Lifton 1980).

Yet, in my view, Freud's massive elaboration of *clinical observations* into his own doctrine of hidden motives still cries out for further careful scrutiny. By the same token, I contend, such scrutiny is at least equally imperative in the case of the various *post*-Freudian, revisionist versions of psychoanalysis. Although the specific content of their theories of psychic conflict is more or less different, they also rely on Freud's clinical methods of validating causal inferences (Eagle, 1983; Grünbaum, 1984, chapter 7). I shall be challenging just these causal inferences. And it will be an immediate corollary of my challenge that it applies not only to Freud's own original hypotheses, but also to any and all post-Freudian versions of psychoanalysis that rely on his clinical methods of justifying causal claims. After all, the changes made by post-Freudians in the specific content of the founding father's theory of psychic conflict (repression) hardly make the *validation* of the revisionist versions more secure!

Therefore, as Morris Eagle documented in a recent publication (1983), those analysts who have objected to my critique as anachronistic have simply not come to grips with it. For example, such inadequate engagement is present, in my view, in the recent Freud Anniversary Lecture "Psychoanalysis as a Science: A Response to the New Challenges," given by Robert Wallerstein (1986), the current president of the International Psychoanalytical Association. As he tells us (1988, 6, n.1), "The Freud Anniversary Lecture was intended primarily as response to Grünbaum." Yet he does not come to grips at all with the gravamen of my challenge: *Even if clinical data could be taken at face value as being uncontaminated epistemically*, the inability of the psychoanalytic method of clinical investigation by free association to warrant causal inferences leaves the major pillars of the clinical theory of repression ill-supported.

The heart of Freud's distinctive theory of repression is not just that we harbor repressed memories, thoughts, desires, and feelings. Instead, it is that sexual repressions are the *crucial pathogens* of mental disorders, that repressed infantile wishes are the generators of our dreams, and that various sorts of repressed, unpleasant thoughts *engender* our slips of memory, of the tongue, the ear, the eye, the pen, etc.

Freud referred to these various sorts of unsuccessful, bungled actions collectively in German as *Fehlleistungen,* or misbegotten performances. And James Strachey, the principal translator of Freud's psychological works into English, coined the new term *parapraxes* to denote them *generically* in English. But, the Vienna-born American psychoanalyst Bruno Bettelheim (1982), deplored this translation. The German word *Fehlleistungen,* he tells us, has a familiar, mellifluously humanistic ring, even smacking of poetry. By contrast, according to Bettelheim, the technical term *parapraxes* allegedly has a coldly scientific tang. Thus, as he would have it, the Englishman Strachey has misportrayed the psychoanalytic enterprise to the English-speaking public by giving a misleading scientific twist to it. Freud, we are asked to believe, wanted psychoanalysis to be a branch of the humanities, but deplorably Strachey made it appear as if Freud worshiped the natural sciences and idolatrously intended psychoanalysis to be a natural science! Strachey created this allegedly false impression by insidiously mistranslating Freud's key German vocabulary. Let me just say very briefly that Bettelheim's indictment of Strachey is, I believe, an unfortunate exegetical fabrication which, alas, grasps at straws. For example, one need only read the German original of Freud's 1933 lecture "Über eine Weltanschauung," which appeared only six years before he died, to see that Bettelheim's complaint is a hermeneutic red herring. In that lecture, Freud declared that "psychoanalysis has a special right to speak for the scientific *Weltanschauung*" (S.E. 1933, 22:159),[1] the word *scientific* being intended in the sense of the *natural* sciences.

The contemporary philosophic spokesmen for the so-called hermeneutic reconstruction of psychoanalysis have even gone further than Bettleheim by condemning Freud for an alleged "scientistic" misunderstanding of his own clinical theory. As we know, the word *scientism* is a derogatory term, used to refer to a misguidedly utopian, intellectually imperialistic worship of science. Thus the hermeneutic philosophers, such as Jürgen Habermas and Paul Ricoeur, claim that even Freud's *aspiration* to build a scientific depth psychology was misguided from the outset. In my book (Grünbaum 1984), I argued that these hermeneutic criticisms are based on misconceptions of both the content and the methods of the natural sciences. Furthermore, I have contended (1988) that Karl Jaspers and the hermeneutic philosophers have mishandled so-called "meaning connections" between mental events vis-à-vis causal connections between such states. As against their claim that Freud assigned much too little explanatory significance to thematic kinships ("meaning connections"), I maintain that he fallaciously gave much too much explanatory weight to them. Therefore, here, I shall try to appraise Freud's own principal arguments for the *cornerstone* of his entire edifice: the theory of repression, or psychic conflict. His theory of psychopathology was the logical and historical foundation of his entire theory of repression. Thus, I shall now turn to it.

2. The Theory of Psychopathology

The central causal and explanatory significance of unconscious ideation *throughout* the psychoanalytic theoretical edifice rests, I claim, on two cardinal inductive inferences. They were drawn by Freud in collaboration with his senior mentor, Josef Breuer. As we are told in their joint "Preliminary Communication" of 1893 (S.E. 1893, 2:6–7), they began with an observation made after having administered their cathartic treatment by hypnosis to patients suffering from various symptoms of hysteria. In the course of such treatment, it had turned out that, for each distinct symptom *S* afflicting such a neurotic, the victim had *repressed* the memory of a trauma that had closely preceded the onset of *S and* was thematically cognate to this particular symptom. Besides repressing this traumatic memory, the patient had also strangulated the affect induced by the trauma. In the case of each symptom, our two therapists tried to lift the ongoing repression of the pertinent traumatic experience, and to effect a release of the pent-up affect by expressing the previously suppressed feelings verbally. When their technique succeeded in implementing this cognitive and cathartic objective, they reportedly observed the dramatic disappearance of the given symptom. Furthermore, the symptom removal *seemed* to be durable, although the presumed cures later turned out to be ephemeral, especially after Freud had begun to practice without Breuer.

Impressed by the positive treatment outcome while it lasted, Breuer and Freud drew their first momentous *causal* inference. Thus they enunciated the following fundamental therapeutic hypothesis: The dramatic improvements observed *after* treatment were produced by none other than the cathartic lifting of the pertinent repressions. But before the founders of psychoanalysis credited the undoing of repressions with remedial efficacy, they had been keenly alert to a rival hypothesis, which derived at least prima facie credibility from the known achievements of the admittedly suggestive therapies. On that alternative explanation of the positive outcome after cathartic treatment, its therapeutic benefit was actually wrought by the patient's credulous expectation of symptom relief, or at any rate *not* by achieving insight into his or her repressions. In this perspective, the quest for such insight is only a particular treatment ritual, which serves to fortify the patient's therapeutic expectations. But Breuer and Freud believed that they could *rule out* such a contrary account of the treatment gains, a challenge to which I shall refer as "the hypothesis of *placebo* effect" (Grünbaum 1986a). In an attempt to counter it, they pointed out that the distinct symptoms had been removed separately – one at a time – such that any one symptom disappeared only after lifting a *particular* repression (S.E. 1893, 2:7).

But, even such separate removals, I submit, may not be due at all to the lifting of repressions; instead they may be a *placebo effect* after all, generated by the patient's awareness that the therapist was intent upon uncovering a thematically particular episode *E* when focusing attention upon the initial appearance of the distinct symptom *S*. Thus, it was presumably communicated to the patient that his or her doctor attached potential therapeutic significance to the recall of *E* with respect to *S*. Indeed, 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* (one at a time), than in the case of getting rid of only one symptom. 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 were *not* lifted. If that control group were to fare equally well therapeutically, treatment gains from psychoanalysis would then presumably be placebo effects, since such a result would then *not* have been wrought by psychoanalytic insight. Hence the attribution of remedial efficacy to the cathartic lifting of repressions was devoid of adequate evidential warrant.

Let me be clear on what I understand here by a "placebo effect" (Grünbaum 1986a). I do not rely on the vague notion of suggestion at all to characterize such an effect. Instead, I say essentially the following: A treatment gain is a "placebo effect" with respect to a particular target disorder, and also with respect to a particular therapeutic theory, just when that positive effect is produced by treatment factors *other than* those designated as the efficacious ones by the given theory.

At the time, Breuer and Freud believed that their therapeutic results *had* ruled out the dangerous competing hypothesis of placebo effect. Thus they credited the gains made by their hysterics to the resurrection of buried painful memories. And they thought furthermore that this supposed therapeutic potency of lifting repressions spelled a paramount etiologic moral as follows: A coexisting ongoing repression is *causally necessary* for the *maintenance* of a neurosis *N*, and an original act of repression was the causal *sine qua non* for the origination of *N*. This second groundbreaking causal inference was animated by the fact that the inferred etiology then yielded a *deductive* explanation of the supposed remedial efficacy of cathartic recall. How so? Clearly, if an ongoing repression *R* is causally *necessary* for the pathogenesis *and* persistence of a neurosis *N*, then it follows that the removal of *R* will actually engender the disappearance of *N*. As we can see, it was not the observed therapeutic gain itself that had prompted the inference of the repression etiology. Instead, it was only the *causal attribution* of the patient's gain to the lifting of repressions *in particular* that had furnished this motivation. Thus, without reliance on the presumed dynamics of their therapeutic results, Breuer and Freud could never have propelled their clinical data into repression etiologies. Freud often put this very briefly by saying that the attempt to unravel the *cause* of a disorder by means of free association was *simultaneously* a therapeutic maneuver (S.E. 1893, 3:35). Or he made the same point by declaring that therapy and etiologic research coincide in psychoanalysis (S.E. 1909, 10: 104–5; 1926, 20: 256).

Freud saw hypnosis, and then free association, as means for the authentic restoration of forgotten traumatic experiences to conscious awareness. Hence one might wonder how he thought he could rule out the *fancied* recall of events that never occurred. Thus, the question is how *pseudo*memories could be distinguishable from genuine ones among the emerging associations. I shall not pursue this issue here. Suffice it to say that it derives added poignancy from recent experimental studies (Dywan & Bowers 1983), which have cast much doubt on the authenticity of hypnotically enhanced remembering. These investigations have shown that hypnotically increased recall is achieved largely at the expense of reliability: When highly hypnotizable subjects recalled twice as many new items hypnotically as the control subjects, they introduced three times as many *new* errors, despite their intense conviction that the new memories were trustworthy! Thus, hypnotic memory is less, not more, reliable than normal prehypnotic recall. It would seem that, by being more suggestible, the hypnotized person translates some of his or her own beliefs and/or those of the hypnotist into *pseudo*-memories.[2] Yet in the 1918 case history of the Wolf Man, who suffered from obsessional neurosis (S.E. 1918, 17:57–60, 95–97), Freud (S.E. 1918, 17: 33–4) gave credence to the adult patient's report of a dream that had occurred between the ages of three and five, which Freud then used to retrodict the following event: When the Wolf Man was a mere infant of eighteen months, he witnessed his parents engage in sexual intercourse *a tergo*. Alleged hypnotic findings have also figured in contexts *removed* from psychoanalysis.

More recently, we have it on the authority of illustrious Hollywood actors such as Glenn Ford and Shirley MacLaine that, under hypnosis, they discovered having each gone through several prior incarnations. For example, it was reported[3] that Glenn Ford was once a Christian martyr who was eaten by a lion. *Mirabile dictu,* Shirley MacLaine reports having been beheaded by Louis XV, and claims to have watched her own head rolling on the floor, but landing face up, a big tear coming out of one eye. This experience, she explains, cured her of stage fright during her current reincarnation. Moreover, her own daughter was her mother in one life, and her sister in another. Recently, the Phil Donahue show on television featured a hypnotherapist from California who blithely reported the following statistic: Two-thirds of his patients retrieve memories from their *current* incarnation, but fully *one-third* recall experiences from *several prior incarnations.* In each case, personal identity remains stunningly intact, unencumbered by the death of the brain.

Even police departments in Los Angeles and elsewhere have made use of hypnotists as detectives in "Svengali squads." But recently, a number of state supreme courts in the United States have declared hypnotically induced testimony inadmissible in criminal trials as inherently unreliable, because a hypnotized person is as likely to concoct wild hallucinations as to achieve veridical recall.[4] I do *not* say that free association should be simply equated with hypnosis. I do claim that the pitfalls of hypnosis at least spell a sobering caveat for the credibility of recall under free association.

But Freud went much beyond crediting free association with the authentic restoration of memories when he drew a major methodological inference from his repression-etiology. As we saw, the outcome and presumed dynamics of successful therapy had been the original evidential basis for the pathogenic role of repression. And once Freud had thus convinced himself of the etiologic role of (unsuccessful) repression on *therapeutic* grounds, he drew a crucial, momentous *investigative* lesson: Free association is a tool of etiologic research into pathogenesis, precisely because it excavates repressions (psychic conflict). In sum, it was only the therapeutically inferred etiology *itself* that gave the license for supposing that when previous repressions are uncovered by free associations, then these emerging repressions are indeed pathogens (S.E. 1900, 5: 528). Hence, at least as far as clinical evidence goes, the credibility of free association as a means of indentifying pathogens depends crucially on whether the therapeutic results do, in fact, support the alleged pathogenic role of repression.

In fact, though it is widely overlooked, *the attribution of therapeutic success to the removal of repressions not only was but, to this day, remains the sole epistemological underwriter of the purported ability of the patient's free associations to ascertain causes.* Analysts such as Strachey (S.E. 1955, 2:xvi) – the translator and editor of the *Standard Edition* of Freud's works – and Kurt Eissler (1969, 461) have hailed free association as an instrument comparable to the microscope and the telescope. More recently, it has been compared to x-ray tomography or CAT scanning. And it is asserted to be a trustworthy means of etiologic inquiry in the sense of licensing a specific major *causal inference.* To state the inference, consider a set of previously repressed traumatic memories, wishes, or fantasies, whose associative emergence is triggered by one of the patient's neurotic symptoms; then, it is claimed, precisely this surfacing in the causal chain of ensuing associations is strong evidence that the prior ongoing repressions of these memories or wishes contributed *pathogenically* to the formation of the symptom.

Whereas all Freudians presumably champion this causal inference, a number of influential ones have explicitly renounced its legitimation by the presumed *therapeutic dynamics* of undoing repressions. To them I say: Without this therapeutic vindication, or some as yet unknown other warrant, no rational person should believe that free associations can ascertain pathogens or any other causes! For without the stated *therapeutic* foundation, this epistemic tribute to free associations rests on nothing to date but a glaring causal fallacy of causal inversion (Grünbaum 1984, 186–87 and 233–34; 1986b, 277). And even that therapeutic foundation will turn out to be quite flimsy. Therefore, it is unavailing to extol the method of clinical investigation by free association as a trustworthy resource of etiologic inquiry, while issuing a disclaimer as to the therapeutic efficacy of psychoanalytic treatment. In brief, those who have made it fashionable nowadays to dissociate the clinical credentials of Freud's theory of personality—the so-called science—from the merits of psychoanalytic therapy are stepping on thin ice indeed.

But let me caution against a possible serious misunderstanding. I do claim that insofar as the credentials of psychoanalytic hypotheses avowedly rest on the purported ability of free associations to ascertain causes, these credentials are ultimately predicated on the therapeutic efficacy of lifting repressions. In concert with Freud himself, the vast majority of his followers continue to maintain that the use of free association as a touchstone of causal certification is crucial for the validation of their *etiologic* hypotheses, as well as of the psychoanalytic theory of dreams and of slips. Hence I contend that these advocates cannot dispense with a therapeutic foundation. But, for my own part, I abjure as unwarranted the invocation of free association as a hallmark of causal certification. Thus, unlike these Freudians, who insist on clinical validation via free association, I am not at all constrained to grant the consequences of such an insistence. Insofar as I can envisage potentially cogent tests of psychoanalytic hypotheses, I see no reason to assign any special or privileged role to therapeutic results. For example, I can envision an epidemiologic test of one of Freud's etiologies such that therapeutic results are irrelevant to the test (Grünbaum 1984, 38–39, 110–11).

Within the clinical confines of traditional psychoanalysis, the foundational role of the presumed dynamics of the therapy to which I have called attention has an important corollary: Namely, that the whole structure of clinical hypotheses based on the therapeutic foundation is in serious jeopardy from the ominous threat posed by the hypothesis of placebo effect. Mind you, the thus endangered hypotheses comprise not only the asserted therapeutic efficacy of lifting repressions, and the repression etiologies of the psychoneuroses, but also the theory of dreams and of sundry sorts of "slips," which Freud extrapolated from his etiologic theory, as we shall see.

Even before he had developed his theory of dreams, and of slips, the menace of placebogenic gains in the therapy was driven home to him. By his own account, soon after he had begun to practice without Breuer, it became devastatingly plain that both of them had been all too hasty in rejecting the rival hypothesis of placebo effect. The remissions achieved by additional patients whom Freud himself treated cathartically turned out *not* to be durable. And these symptom relapses showed him that his treatment had *not* uprooted the cause of the symptoms. Indeed, the ensuing pattern of relapses, additional treatment, ephemeral remissions, and further relapses undermined the attribution of therapeutic credit to the lifting of those repressions that Freud had uncovered. Ironically, he began to be haunted by the triumph of the hypothesis of placebo effect over the fundamental therapeutic tenet that Breuer and he had originally enunciated. As he recognized, the vicissitudes of his personal relations to the patient were highly correlated with the pattern of symptom relapses and intermittent remissions. And, in his own view, this correlation "proved that the personal emotional relation between doctor and patient was after all stronger than the whole cathartic process" (S.E. 1925, 20:27). But, once the repression etiology was thus bereft of therapeutic support, the very cornerstone of psychoanalysis had been completely undermined. Hence at that point, the new clinical psychoanalytic structure tumbled down and lay in shambles. So also then did free association as a method of etiologic certification!

Nonetheless Freud was undaunted. He took courage in 1896, because he thought that, in a new *sexual* version going back to childhood, the repression etiology could be rehabilitated on secure therapeutic foundations after all. And his strenuous effort to achieve such a rehabilitation culminated in his 1917 paper on "Analytic Therapy" (S.E. 1917, 16:448–63). There he tried to offer an explicitly *therapeutic* vindication of the psychoanalytic method and theory of personality, including its specific etiologies of the psychoneuroses, and even its general hypotheses about psychosexual development. But, as I have argued in detail in my book (1984, chapter 2), his attempt to vindicate the rehabilitated etiology therapeutically in 1917 fared no better empirically than his and Breuer's original reliance on cathartic success in the mid-1890s. In particular, this 1917 endeavor foundered, if only because Freud again failed to rule out the opposing placebo hypothesis. Indeed, to this day, psychoanalytic treatment process research has failed to discredit the altogether reasonable challenge posed by this rival account of gains from its therapy. Hence I claim that the whole of the clinical psychoanalytic enterprise continues to be haunted by it.

Moreover, no empirically viable surrogate for Freud's discredited 1917 effort seems to be even remotely in sight. Nor, to my knowledge, are there any other cogent *therapeutic* defenses of the sexual repression etiology of the neuroses. Hence this etiology should now be regarded as devoid of significant therapeutic support, just like Breuer's nonsexual cathartic etiologies, which Freud himself had disavowed as clinically dubious.

But that is not all. As I have already pointed out, the credibility of free association as a method of etiologic certification has rested entirely on the cogency of Freud's *therapeutic* argument for his theory of pathogenesis (S.E. 1900, 5: 528). Hence the collapse of precisely that argument, even in its mature version of 1917, also undermines the etiologic credibility of the fundamental rule of free association. After all, as we saw, Freud had enunciated this rule as a maxim of research in psychopathology, because he thought—on therapeutic grounds—that associations governed by it can reliably certify the unconscious pathogens of the neuroses.

In fact, *it was too good to be true at the outset, I think, that psychoanalysis should have been able to make reliable etiologic determinations just by having someone lie on a couch and associate freely,* even for years. At least prima facie, this skeptical attitude appears justified, if only because the validation of etiologic hypotheses in somatic medicine, for example, normally requires controls of one sort or another. And there is typically no counterpart to such controls in Freud's fundamental rule of free association. The well-known psychoanalyst Erik Erikson seems to be cognizant of this doubt. Thus, he speaks very defensively, when he claims "the necessity to abandon well-established methods of sober investigation (invented to find out a few things exactly and safely to overlook the rest) for a method of self-revelation apt to open the floodgates of the unconscious" (Erikson 1954, 54). This brings us to the theory of dreams and of slips.

3. The Dream Theory and the Theory of Slips

Freud did not limit his investigative esteem for free associations to etiologic research. When he found that his patients reported their dreams while freely associating to their neurotic symptoms, he drew a very weighty but highly risky conclusion as follows: Manifest dream contents can be causally *assimilated* to the status of neurotic symptoms, and thus are likewise presumed to be generated by repressions. And he saw neurotic symptoms, in turn, as *substitutive* gratifications and outlets, or as "compromises between the demands of a repressed impulse and the resistance of a censoring force in the ego" (S.E. 1925, 20:45). And when he extended this notion of compromise formation to manifest dream content, he carried out a bold extrapolation of his repression etiology from neurotic symptoms to manifest dream content. By the same token, he felt entitled to enlarge the scope of free association from being a method of etiologic research *aimed at therapy,* to serving likewise as an avenue for finding and certifying the purported unconscious causes of dreaming (S.E. 1900, 4:101; 5:528).

Mutatis mutandis, he also assimilated slips and bungled actions (*Fehlleistungen*) to his compromise conception of neurotic symptoms. And, by parity with his previous reasoning, he again resorted to free association not merely heuristically, as a means of generating causal explanations of slips, but also probatively, as a basis for validating the explanatory hypotheses. For example, he conceived of a slip of the tongue as a compromise between a repressed motive that crops out in the form of a disturbance, on the one hand, and the conscious intention to make a certain utterance, on the other.

In short, once Freud had postulated *by sheer extrapolation* that dreams and slips are indeed compromise formations, no less than neurotic symptoms themselves, it seemed evident that dream production and slip generation are also due to repressed motives. By the same token, *if* it be granted that the method of free association can show repressions to be the pathogens of neuroses, that method seems eminently capable of reliably ascertaining as well the unconscious motives of other purported compromise formations, notably of dreams and slips.

But Freud's assimilation of dreams and slips to compromise formations without ado was an audacious, if not just foolhardy, extrapolation from the etiologic role that he had attributed to repression in psychopathology. The more so, since just that purported etiologic role is itself in grave jeopardy—as I have argued—from lack of cogent therapeutic support.

Indeed, I contend that even if the original *therapeutic* defense of the repression etiology of neuroses had actually turned out to be empirically viable, Freud's compromise models of parapraxes and of manifest dream content would still be *misextrapolations* of that etiology, precisely because they lacked any corresponding *therapeutic* base at the outset. For example, he did not adduce any evidence that the permanent lifting of a repression to which he had attributed a slip will be "therapeutic" in the sense of enabling the person himself or herself to correct the slip and/or to avoid its repetition in the future. Thus, it turns out that whereas he claimed to have therapeutic evidence for postulating the pathogenic role of repressions, he never produced any *independent* clinical support for his two daredevil extrapolations of the compromise model to dreams and to slips—nor for believing that free associations can ascertain the *causes* of dreams and of slips. Let me emphasize that when I speak of evidence as "clinical," I adopt its technical usage as referring to the observations made by psychoanalysts of their patients' productions in the treatment setting of the couch.

Hence, within the confines of his clinical evidence, where free association is invoked as a hallmark of causal validation, the epistemic fortunes of Freud's extrapolations of the compromise model are dependent on those of his theory of psychopathology. As a consequence of just this epistemic dependence in the clinical context, the ravages from the clinical collapse of Freudian psychopathology, and of free association as a tool of etiologic certification, turn out to extend, with a vengeance, to the psychoanalytic theory of dreams and of sundry sorts of slips. Mind you, when I speak of the clinical collapse, I mean the clinical unfoundedness, not the clinical refutation.

Thus, for instance, I *allow* that there may be slips that are engendered by repressions. But I maintain that if there are such slips, Freud did not give us any good reason to think that his clinical methods can identify and validate their causes as such, no matter how interesting the elicited "free" associations might otherwise be.

Thus, the failure of his clinical arguments for the protean causal role of repressions does not itself betoken the falsity of his theory of psychopathology, or of his dream theory, or yet of his account of slips. But my discreditation of his arguments so far does, I claim, undermine the foundations of his theoretical edifice, since he rested it on just the clinical reasoning I have challenged. And the vast majority of his disciples nowadays also rest their case on *clinical* evidence from the analytic treatment setting.

But even if we confine ourselves solely to clinical considerations and disregard experimental ones (Hobson 1988), there are additional good grounds for deeming Freud's dream theory to be false rather than just ill supported. These further grounds for claiming falsity are of two sorts.

1. As I shall now argue, in the context of the remainder of Freud's theory of repression and psychoanalytic therapy, his dream theory predicts a reduction in the *frequency* of dreaming among extensively psychoanalyzed patients. But there is no evidence for such a reduction. More precisely, Freud's assimilation of manifest dream content to minineurotic symptoms has the consequence that *either* extensively analyzed patients should be "cured" of dreaming, or free association fails as a means of lifting presumably repressed infantile wishes.

It will be recalled that just as sexual repressions, in particular, are postulated to be causally necessary for neurosogenesis, so also sundry sorts of repressed infantile wishes are hypothesized to be the causal sine qua non of dream generation. Breuer and Freud had told us in 1893 that if particular repressions are, in fact, causally necessary for psychopathology, then it follows that the lifting (undoing) of these pathogenic repressions or conflicts will issue in the conquest of the patient's affliction. By parity of reasoning, if *repressed* infantile wishes are the sine qua non of dream formation, the patient's achievement of conscious awareness of these wishes will rob them of their previous causal role as dream generators. It emerges, therefore, that in proportion as the patient's free associations do succeed in bringing his or her buried infantile wishes to light, the analysand should experience—and presumably exhibit neurophysiologically—a noticeable reduction in dream formation. Evidently this reduction should be a diminution in the *frequency* of dream generation, as distinct from a mere change in dream content. But whereas changes in dream content are commonplace as a function of the thematic content of analytic sessions, even protractedly analyzed patients do not report any remarkable subjective diminution of their recalled dream experiences. Nor, to my knowledge, have analysts been aware that the theoretical expectation of a reduction in the frequency of dreaming is a consequence of Freud's dream theory, which makes the assumption that free associations of sufficient duration normally retrieve at least some buried infantile wishes, at least among extensively analyzed patients.

We now see that if neurophysiological indicators (perhaps REM sleep) bear out that, among such psychoanalytic patients, the expected decline in dream activity *fails* to materialize, then an important indictment would seem to follow: Either their free associations are chronically unsuccessful in retrieving their buried infantile wishes, or, if there is such retrieval, then Freud's account of dream generation is false. But if free association were to fail chronically even in just lifting repressions, that would be a much greater threat to the psychoanalytic enterprise than the mere demise of Freud's dream theory.

In response to my development of this argument, Philip Holzman suggested that I comment on the retort that no reduction of dream frequency is to be expected, because the impulse behind the emerging wishes remains undiminished in the unconscious. I reply that this retort cannot obviate the discreditation of Freud's account of dream formation, precisely because of the parity of the reasoning in my argument with the basic rationale of psychoanalytic therapy. If we grant Freud's assimilation of manifest dream content to his compromise model of neurotic symptoms, then I ask: Why shoud the *therapeutic* import of this model not hold alike for dream production and ordinary symptom formation? What is sauce for the goose is sauce for the gander. If lifting (and working through) the sexual repressions that are deemed pathogenic more or less cures the neuroses, then lifting (and working through) the repressions of infantile wishes should "cure" dreaming to the same extent, as it were. On the other hand, if the impulse behind the previously repressed wishes generates ever new ones that, in turn, engender dreams even as the patient becomes conscious of the earlier ones, why does the pathogenic action of sexual repressions not also remain equally undiminished after *they* are lifted? If psychoanalytic theory is taken to assert that we have an inexhaustible store of the impulses that beget dreams, how can it claim that we do not also have a like store of pathogenic impulses? By the same token, if psychoanalytic therapy is not doomed to fail at the outset in the case of neuroses, then the recourse to undiminished dream generation, even as repressed wishes are made conscious, is impermissibly ad hoc.

2. I contend that, contrary to Freud (1900, pp. 151–59), the so-called "counter-wish dreams" cannot be reconciled with his wish-fulfillment theory. Freud claims compatibility on the grounds that the contents of these dreams do fulfill one of two wishes: (1) the purported wish to prove his psychoanalytic theory wrong, or (2) the masochistic wish for humiliation and mental torture. As shown by his examples, Freud makes the sound assumption that imputations of the desire to prove him wrong or of a masochistic disposition require *independent* evidence, which does not rely on the de facto occurrence of counter-wish dreams.

As Freud (p. 151) reports, after he had explained his wish-fulfillment theory to "the cleverest of all my dreamers," she dreamt that "she was traveling down with her mother-in-law to the place in the country where they were to spend their holidays together." Yet he "knew that she had violently rebelled against the idea of spending the summer near her mother-in-law and that a few days earlier she had successfully avoided the propinquity she dreaded by engaging rooms in a far distant resort." Thus, "now her dream had undone the solution she had wished for."

To deal with this seemingly recalcitrant finding, Freud (pp. 151–52) points out that, at the time of the dream, the patient had been rejecting his inference as to the occurrence of certain events in her life that had presumably been pathogenic, but which she could not recall. This resistance had been prompted by her "well-justified wish that the events of which she was then becoming aware for the first time might never have occurred." By the same token, this wish supposedly also engendered her broader intellectual desire that Freud's theory be generally wrong. Finally, we learn, the latter desire "was tranformed into her dream of spending her holidays with her mother-in-law" (p. 152).

Freud (p. 151) pays tribute to her cunning unconscious, which purportedly has a perspicacious appreciation of the dreams' logical consequence: "it was only necessary to follow the dream's logical consequence in order to arrive at its interpretation. The dream showed that I was wrong. *Thus it was her wish that I might be wrong, and her dream showed that wish fulfilled*" (italics in original). In the German original, the second of these two conjuncts says literally that the dream *showed her* the fulfillment of this wish (Freud, 1940–52, 2/3, p. 157).

Though Freud speaks of counter-wish dreams as "very frequent" (p. 157), he would have us believe that they are *confined* to people who either have a wish to prove him wrong or who, qua "mental masochists," have the self-punitive wish to suffer mental torture. The former group includes (a) patients who become aware of his dream theory while "in a state of resistance" to him during psychoanalytic treatment, and (b) others who are exposed to his writings or lectures on his dream theory but are unfavorably disposed toward it (p. 158). By thus confining the counter-wish dreamers, he hopes to sustain his thesis that their dreams conform to the pattern "the non-fulfilment of one wish meant the fulfilment of another." But this confinement is untenable, unless he could demonstrate that people, children, or animals (e.g., monkeys) who give no *independent* evidence of a significant masochistic disposition and who are in no position to harbor a wish to disprove Freud's dream theory have at least incomparably fewer counter-wish dreams than patients who are in a resistance phase of their analysis or are otherwise hostile to Freudian ideas.

It does not even seem to have occurred to Freud that without the provision of a baseline as to the incidence of counter-wish dreams in the former huge class, there is at best no cogent reason for attributing such dreams causally to hostile motives toward his theory.

Indeed, it would seem that there just is no difference in the incidence of counter-wish dreams in the two classes. For example, there are presumably lots of students who are devoid of a masochistic disposition and who have nightmarish examination dreams before they ever hear of the content of Freud's theory. Moreover, even if a desire to prove Freud wrong were to motivate some counter-wish dreams, that wish fails to satisfy his requirement of being a *repressed infantile* wish! Besides, what of the counter-wish dreams of *ardent* believers in psychoanalysis who are not masochists? And, finally, why should Freud assume that *unanalyzed* educated people exposed to his wish-fulfillment theory will often have a motive for resisting it? After all, the notion that *some* dreams are wish fulfilling is a commonplace in folk (commonsense) psychology.

Recall Freud's wish-fulfillment claim: The dream "*content was the fulfilment of a wish*" [first conjunct] *and its motive was a wish* [second conjunct]" (p. 119; italics in original). My criticisms have shown, I believe, that Freud's *causal* attribution of nonmasochistic counter-wish dreams to the desire to prove him wrong is probably false. So much for the considerations that, in my view, suggest the falsity of Freud's dream theory, rather than merely its clinical ill foundedness.

All the same, there is some plausibility, I believe, in the psychoanalytic claim that there exist such defense mechanisms as repression, denial, rationalization (in Ernest Jones's sense), reaction formation, and projection. But, as I have emphasized, psychoanalytic theory goes far beyond asserting that repression operates in these defense mechanisms. Besides claiming the bare operation of a mechanism of repression, the theory assigns a crucial causal role to it in pathogenesis, dream formation, and in the generation of slips. And, I claim, just that causal role is questionable.

4. Experimental Results

So far, I have made no mention of the results obtained in actual attempts to test some parts of psychoanalytic theory experimentally. Such laboratory tests of psychoanalysis are discussed in a book by the *pro*-Freudian English psychologist Paul Kline (1981). In this work, Kline tries to refute the extremely skeptical conclusions offered by Hans Eysenck and Glenn Wilson (1973). Interestingly enough, Freud himself told us in 1933 that "In psycho[analysis] however, we have to do without the assistance afforded to [other] research by experiment" (S.E. 1933, 22:174).

In my view, the debate between Eysenck and Kline yields essentially the same verdict as the one I have drawn from the *clinical* evidence offered by Freudians. So far, the evidence available from the laboratory has provided no significant support for any of the major hypotheses of psychoanalytic theory or therapy. Let me give two brief but important illustrative examples, which will also convey what I mean by the term *major hypothesis* in this context.

Freud hypothesized that strongly *repressed* homosexual desires are the causal sine qua non for the pathogenesis of paranoid delusions. The experimenter Harold Zamansky (1958) attempted to test this etiologic hypothesis (Grünbaum 1986b, 269–70). But this investigator himself makes only the following quite modest claim for his findings: "Though the present experiment has demonstrated a greater degree of [repressed] homosexuality in men with paranoid delusions than in nonparanoid individuals, these results *tell us nothing* about the role which homosexuality plays in the development of these delusions" (quoted from Zamansky's study, as reprinted in Eysenck and Wilson [1973, p. 308]; italics added). In the same vein, I claim more generally: It is not enough to provide evidence for the existence of a *mechanism* of repression, which was already postulated before Freud by Herbart, among others. The point is that there is no good experimental evidence for Freud's much stronger claim that specific sorts of sexual repressions are *causally necessary* for the production of designated kinds of mental disorders. In fact, Kline (1981) admits as much in the following disclaimer: "Many of the Freudian claims concerning the neuroses have simply never been put to the objective test . . . Thus, the hypotheses which have been put to the test are usually those where convenient measures are at hand rather than those most crucial to the theory" (437).

The psychoanalytic theory of dream production by repressed infantile wishes furnishes a second illustration of my thesis that there is a poverty of experimental support for Freud's *cardinal* postulates. His theory of dream interpretation is appraised by the psychoanalytically oriented psychologists Fisher & Greenberg (1977). On the construal they deem most reasonable, its central thesis is that dreams express (vent) not only wishes, but sundry drives or impulses that originate in the unconscious (46–47). But, besides opting for some revisions, they make two sobering points: (1) Even if one can show that a dream does, indeed, express a certain impulse, there are, at present, still no scientifically reliable means of warranting that the impulse originated in the unconscious sector of the psyche (47); and (2) the available "findings are *congruent* with Freud's venting model. But . . . they do not specifically document the model" (53; italics in original). Moreover, "the data . . . are encouraging but not definitively validating with respect to Freud's venting model" (63).

Yet, despite issuing these vital admonitions, Fisher and Greenberg permitted themselves to *begin* their summary by declaring that Freud's model "seems to be supported by the scientific evidence that can be mustered" (63). By thus sliding from compatibility ("congruence") with the theory into *"support"* for it, these friends of psychoanalysis have lent substance to Popper's complaint against the methodological behavior of some of Freud's sympathizers, although Popper's critique of psychoanalytic theory is largely unsound (Grünbaum 1984, ch. 1; 1986b, 266–70; 1989).

5. Conclusion

But my primary concern here has been with the *clinical* rather than with the *experimental* evidence. After all, it is the *clinical* evidence on which most psychoanalysts rest their case (Grünbaum 1988). And I conclude that despite their appeal to such evidence, the operation of hidden motives in Freud's sense has yet to be cogently tested on an adequate scale. And until it is, the widespread belief in psychoanalytic theory in some segments of our culture is ill founded.[5] For just the reasons I recapitulated in the *Introduction* (§1) from Eagle (1983) and Grünbaum (1984, chapter 7), this unfavorable verdict applies alike to Freud's original formulations and to any and all post-Freudian, revisionist versions of psychoanalysis that rely on the founding father's clinical methods of justifying causal inferences.

Notes

1. Sigmund Freud, "The Question of a Weltanschauung," in *Standard Edition of the Complete Psychological Works of Sigmund Freud,* vol. 22, trans. J. Strachey et al. (London: Hogarth Press, 1955), p. 159. This paper first appeared in 1933. Hereafter any references to Freud's writings in English will be to this *Standard Edition* under its acronym "S.E." followed by the year of first appearance, volume number, and page(s). Thus the 1933 paper just cited in full would be cited within the text as: S.E. 1933, 22:159.

2. Compare also the summary of Loftus's work (1980) in my book (1984, 243–44).

3. "I Was Beheaded in the 1700s," *Time*, September 10, 1984, 68.

4. Compare "Breaking the Spell of Hypnosis," *Time,* September 17, 1984, 62.

5. For a further discussion of the issues raised in this essay, the reader is referred to my "Author's Response" (1986b) to nearly forty commentaries on my (1984) book, *The Foundations of Psychoanalysis: A Philosophical Critique.*

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S.E. (See Freud, S., above.)

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