

## Introduction

# Critique of the Hermeneutic Conception of Psychoanalytic Theory and Therapy

The study before you is a philosophical critique of the foundations of Sigmund Freud's psychoanalysis. As such, it must also take cognizance of his claim that psychoanalysis has the credentials of a natural science. But before examining the cardinal arguments put forward by him, I need to expose a widespread exegetical myth.

It is precisely that myth, the contrived reading, which has served as the point of departure for convicting Freud of "scientistic self-misunderstanding." This demonstrably ill-founded charge was leveled by the philosophers Jürgen Habermas and Paul Ricoeur, champions of the so-called "hermeneutic" version of psychoanalytic theory and therapy. Indeed, their rendition has gained widespread acceptance in various quarters as now being at the cutting edge of the field, if not *de rigueur*. But besides resting on a mythic exegesis of Freud's writings, the theses of these hermeneuticians are based on profound misunderstandings of the very content and methods of the natural sciences.

Hence, it will be useful that I address, at the outset, not only the fabrication of the textual legend but also the multiple ontological and epistemic blunders inherent in the currently fashionable hermeneutic construal of psychoanalysis. The more so since Habermas has deemed precisely this reading of the Freudian corpus to be potentially prototypic for the other sciences of man (Habermas 1971, chapter 10). Thus, my critical scrutiny in the present Introduction may well have considerable import for hermeneutic philosophy, well beyond psychoanalysis proper.

Of course, within this study of the foundations of psychoanalysis, I do not intend to pursue these ramifications, let alone try to explain why this philosophy has achieved popularity. Yet, I hope that others who may do so will find my analysis instructive for their purpose as well.

### 1. The Exegetical Legend of "Scientistic Self-Misunderstanding"

Throughout his long career, Freud insisted that the psychoanalytic enterprise has the status of a natural science. As he told us at the very end of his life, the explanatory gains from positing unconscious mental processes "enabled psychology to take its place as a natural science like any other" (S.E. 1940, 23: 158). Then he went on to declare: "Psychoanalysis is a part of the mental science of psychology. . . . Psychology, too, is a natural science. What else can it be?" (S.E. 1940, 23: 282).

Five years earlier, in his 1933 lecture on *Weltanschauung*, Freud had firmly rebuffed the antinaturalism and methodological separatism that was championed by the *Geisteswissenschaften* movement as a framework for psychology and the social sciences. Its votaries deemed causal explanations to be endemic to the natural sciences in view of the general and law-like causal connections featured by these disciplines. And they rejected such nomothetic causal explanations as generically alien to the humanistic sciences. For as they saw it, the aim of the study of man ought to be the "hermeneutic" quest for idiographic understanding by such methods as empathy and intuitive self-evidence (Möller 1976: 38-62; Möller 1978: 162-211). In diametrical opposition to this delineation of the task of psychology, Freud proclaimed:

the intellect and the mind are objects for scientific research in exactly the same way as any non-human things. Psycho-analysis has a special right to speak for the scientific *Weltanschauung*. . . . If . . . the investigation of the intellectual and emotional functions of men (and of animals) is included in science, then it will be seen that . . . no new sources of knowledge or methods of research have come into being. [S.E. 1933, 22: 159]

Having thus concluded that "psychoanalysis has a special right to speak for the scientific *Weltanschauung*," the founder of this new branch of psychology deplored its reception in the scientific community. As Freud put it plaintively in 1925: "I have always felt it a gross injustice that people have refused to treat psycho-analysis like any other [natural] science" (S.E. 1925, 20: 58).

Three decades earlier, in 1895, the psychoanalytic method of clinical investigation by means of free association was only a fledgling mode of inquiry. Likewise, Freud's clinical theory of psychopathology was still nascent. At that very early juncture, he gave a neurophysiological twist to the notion of a scientific psychology and couched his then vision of a

neurological underpinning for psychic processes in the reductionistic physical idiom of material particles. Thus, in the opening sentence of his 1895 manuscript "Project for a Scientific Psychology," he stated: "The intention is to furnish a psychology that shall be a natural science: that is, to represent psychical processes as quantitatively determinate states of specifiable material particles. . . ." (S.E. 1950, 1: 295). As we learn in the next sentence, the "specifiable material particles" he has in mind are "neurones." (For clarifying commentary, see Holt 1965, Pribram and Gill 1976, Fancher 1973, Kanzer 1973, and McCarley and Hobson 1977.)

Freud's clinical work had inspired the notion of psychic repression, an idea whose theoretical elaboration was destined to become the "cornerstone" of "the whole structure of psychoanalysis" (S.E. 1914, 14: 16). Hence, in his search for a neurophysiological model of the psychic apparatus it became imperative to accommodate the cardinal hypothesis of repression. Yet, the difficulties he encountered in that endeavor generated fundamental doubts as to the sheer soundness of his neurological vision (Freud 1954: 349-350; Sulloway 1979: 123-126).

In the face of this disappointment, Freud abandoned his reductionistic program within two years of having enunciated it in the draft of his 1895 "Project" (Freud 1954: 349). Indeed, besides never publishing it, he refrained from mentioning it in his other writings. The result was that its existence was not even suspected until some years after his death. As if to allude to this failure of the "Project," Freud wrote a decade later:

every attempt. . . to discover a [brain] localization of mental processes, every endeavour to think of ideas as stored up in nerve-cells and of excitations as travelling along nerve-fibres, has miscarried completely. [S.E. 1915, 14: 174]

Here a caveat is in order. By 1896, I claim, Freud had despaired of foreseeably *reducing* the clinical theory *globally* to neurobiology. But I do not thereby run afoul of the well-attested *heuristic* role (Sulloway 1979: 121-123) that the neurobiological models—i.e., the purely mechanical and the organismic-evolutionary ones—retained, via analogies, in Freud's subsequent clinical and metapsychological theorizing. For my concern now is with the grounds on which he attributed *scientificity* to his evolving clinical theory. And, as I shall soon document, the heuristic role perennially enjoyed by the neuromechanical model after its collapse did not prevent that demise from ushering in the following development: Freud forsook his initial, ontologically reductive notion of scientific status in favor of a methodological, epistemic one (Grünbaum 1983a).

By the time he wrote the last chapter of *The Interpretation of Dreams* (S.E. 1900, 5: chapter vii), the legacy of the abandoned neurological

model had become a postulated bipartite structure of the mind whose principal agencies were the unconscious and the preconscious. As he tells us, his "topographic" depiction of the relations of these component systems was "expressed in spatial terms, without, however, implying any connection with the actual anatomy of the brain" (S.E. 1925, 20: 32). And while pointedly eschewing the original neurological connotations of the technical vocabulary he had introduced in the "Project," he now often employed it homonymously in an avowedly mentalistic sense. Thus, the erstwhile excitation or "cathexis" of a *neuron* has now become the cathected state of an *idea* or *memory*. Likewise, a train of thought is now held to involve the *flow* of cathexes from one *idea* to another, so that *psychic energy* is invested in the mental representations of objects. And as he was stressing by 1913:

it is easy to describe the unconscious and to follow its developments if it is approached from the direction of its relation to the conscious, with which it has so much in common. On the other hand, there still seems no possibility of approaching it from the direction of physical events. So that it is bound to remain a matter for psychological study. [S.E. 1913, 13: 179]

Freud retained these mentalistic notions to the end of his life:

We assume, as other natural sciences have led us to expect, that in mental life some kind of energy is at work; but we have nothing to go upon which will enable us to come nearer to a knowledge of it by analogies with other forms of energy. We seem to recognize that nervous or psychical energy occurs in two forms, one freely mobile and another, by comparison, bound. (S.E. 1940, 23: 163-164)

The free mobility of psychic energy toward tension-discharge is supposed to be characteristic of processes associated with the unconscious system, whereas this energy is presumed to be bound against discharge for processes in which the preconscious agency is paramount (Laplanche and Pontalis 1973: 172-173). Yet he allows that there are "hypercathexes . . . in the course of which free energy is transformed into bound energy" (p. 164). By 1923, Freud modified his bipartite structural model, on which he had relied since 1900 (S.E. 1923, 19: 12-59; 1933, 22: 57-80). Writing in 1925, he explained:

In my latest speculative works I have set about the task of dissecting our mental apparatus on the basis of the [psycho-] analytic view of pathological facts and have divided it into an *ego*, an *id*, and a *super-ego*. The super-ego is the heir of the Oedipus complex and represents the ethical standards of mankind. [S.E. 1925, 20: 59]

These successive models of the structure and function of the psychic apparatus—propounded in 1895, 1900, and 1923—are often denominated as the “metapsychology” of Freud’s theoretical edifice (Laplanche and Pontalis 1973: 250). Though the separation of the clinical theory from that metapsychology within the edifice is not always sharp, it is vital to appreciate what epistemic and logical status Freud assigned to its metapsychological part, while steadfastly claiming natural science status for his construction overall. Writing in his 1925 *Autobiographical Study*, he relegated his metapsychological notions to a “speculative superstructure of psycho-analysis” when he declared:

Such ideas as these are part of a speculative superstructure of psycho-analysis, any portion of which can be abandoned or changed without loss or regret the moment its inadequacy has been proved. But there is still plenty to be described that lies closer to actual experience. [S.E. 1925, 20: 32-33]

In the same vein, over a decade earlier he had portrayed the metapsychological hypotheses as “the top of the whole [psychoanalytic] structure,” a top which “can be replaced and discarded without damaging it [the structure]” (S.E. 1914, 14: 77). Significantly, the “plenty. . . that lies closer to actual experience” is, of course, none other than his *clinically* based theory of personality, psychopathology, and therapy. The centerpiece of that corpus of hypotheses is the theory of repression, which features his compromise-model of neurotic symptoms, as well as of manifest dream content and of various sorts of slips. Note that these various phenomena are deemed to be “compromises” in the sense of being *substitutive gratifications* or outlets. For they are held to be conatively vicarious surrogates. Moreover, the clinical theory of repression is often couched in *personalist* language.

But in conspicuous contrast to his depiction of the metapsychology as “a speculative superstructure” which can be sloughed off, if need be, “without loss or regret,” he explicitly deemed his clinical theory to be “the most essential part” of what he had wrought. For, as he told us in his own *History of the Psycho-Analytic Movement*, “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). Indeed, when the psychologist Saul Rosenzweig offered him alleged *experimental* evidence for this foundational doctrine in 1934, Freud replied that such evidence is superfluous in view of “the wealth of reliable [clinical] observations” on which that doctrine rests (MacKinnon and Dukes 1964: 703). Earlier, he had emphasized that “the foundation of science upon which everything rests. . . is observation alone” (S.E. 1914, 14: 77). Furthermore, he

maintained that "Psycho-analysis is founded securely upon the observation of the facts of mental life" (S.E. 1923, 18: 266). Moreover, immediately before making this assertion, he explicitly *subordinated* the observational credentials of the metapsychology to those of the clinical theory: "It must not be supposed that these very general ideas [concerning the mental apparatus as a compound instrument, composed of an *id*, *ego*, and *super-ego*] are presuppositions upon which the work of psycho-analysis depends. On the contrary, they are its latest conclusions and are 'open to revision'." But, as for the centerpiece of the clinical theory, Freud avers: "the theory of repression is a product of psychoanalytic work, a theoretical inference legitimately drawn from innumerable observations" (S.E. 1914, 14:17)

It emerges clearly that when Freud unswervingly claimed natural science status for his theoretical constructions throughout his life, he did so first and foremost for his evolving clinical theory of personality and therapy, rather than for the metapsychology. For he had been chastened in his early reductionistic exuberance by the speedy demise of his "Project." And, once he had repudiated his ephemeral neurobiological model of the psyche after 1896, he perennially saw himself entitled to proclaim the *scientificity* of his clinical theory *entirely on the strength of a secure and direct epistemic warrant from the observations he made of his patients and of himself*. In brief, during all but the first few years of his career, Freud's criterion of *scientificity* was *methodological* and *not* ontologically reductive (S.E. 1914, 14: 77; 1915, 14: 117; 1925, 20: 32).

Yet, as we shall see, Freud's subsequent unflinching assertion of natural science status for the clinical theory has been depicted as having been parasitic on the would-be reduction of its hypotheses to a metapsychology that Freud allegedly deemed *primordially* scientific. And this doctrine of the mediated scientificity of the clinical theory is seen as a relic from the heady days of the "Project," or from the teachings of Helmholtz, Meynert, and Brücke. But, as we can already conclude, this exegesis is an arrant, if widespread, mistake. Thus, Gill (1976: 72) saddles the mature Freud of 1915 with the ontologically reductive notion of scientific status. For, as Gill would have it, Freud's positing of seemingly *psychological* hypotheses "in a natural-science framework . . . is a reductive attempt to convert psychological discourse to a universe alien to it—the universe of space, force and energy".

Indeed, even if Freud could be shown to have believed that he had actually effected such a reduction to the metapsychology, this would hardly establish that he had *predicated* the *scientificity* of the clinical theory upon it! For, as I have documented, he saw the natural science

status of the avowed cornerstone of his edifice as anything but trickling down, so to speak, from the acknowledged speculative superstructure. On the contrary, after 1896 it was the *direct* evidential support he claimed to have for his clinical theory from his office couch—not some fancied explanatory subsumption under the abstract metapsychology—that he saw as authenticating the clinical theory. Scientifically, this authentication proceeded from the bottom up, as it were. In the same vein, the analyst Fenichel paid tribute to the Freudian corpus a few years after the demise of its creator when he said: “An understanding of the multiplicity of everyday human mental life, based on natural science, really began only with psychoanalysis” (1945: 4). Yet, at cross purposes with the mature Freud, Gill (1976: 91) insists on an ontological—rather than methodological—construal of natural science, and believes that he can conclude: “the natural-science framework is inappropriate to the data of psychoanalysis.” And, as we shall see toward the end of this chapter (Section 5), the damage from Gill’s ontological construal is not at all obviated by his brief, tantalizing caveat: “sciences other than the natural sciences . . . conform to the general methodological canons of the scientific method” (1976: 95).

Ironically, the leading spokesmen for the “hermeneutic” construal of Freud’s clinical theory—who champion the interpretive virtues of reading texts—have given wide currency to a reading of Freud that runs afoul of just the conclusions I have documented. According to the philosophers Jürgen Habermas and Paul Ricoeur, and the late analyst George Klein, Freud claimed the status of a natural science for his clinical theory by misextrapolation from its envisaged reduction to the primordially scientific metapsychology. Thus, in an unavailing attempt to justify such an exegesis, Habermas (1971: 249) points to the fact that Freud had hypothesized various *correlations* between clinical and metapsychological concepts as typified by the following: the metapsychological concept pair “pleasure-unpleasure”—which designates libidinal energy discharge versus a dammed up accumulation of excitation-tension—is associated in the theoretical system with its clinical affect-homonyms, which designate subjective pleasure and pain. Similarly, Habermas notes reprimandingly (1971: 251) that, in a paper on therapeutic technique, Freud associated the clinical concept of retrieving a repressed memory with Breuer’s metapsychological notion of “‘abreacting’ of the quotas of affect strangled by repression” (S.E. 1914, 12: 156).

Having imported the Hegelian idiom of “self-reflection” (1971: chapter 10), Habermas coins the term “movement of self-reflection” (p. 251) to designate the kind of psychoanalytic process that is traditionally

described by speaking of the patient's "working through his(her) resistances, defenses and transference-repetitions, lifting his(her) repressions," and achieving "the return of the repressed."

But Habermas sees the self-emancipatory process that is hypothesized to occur in the psychoanalytic "movement of self-reflection" as fundamentally alien to the methodological and ontological categories featured by the natural sciences. And since he had saddled Freud with having invested the metapsychology with primordial scientificity vis-à-vis the clinical theory, he blames the correlations between the metapsychological and the clinical concepts for having trapped Freud in a far-reaching "scientistic self-misunderstanding" (pp. 246-252). Purportedly, Freud's misunderstanding was "scientistic" because he idolatrously endowed the clinical theory with natural science status by misextrapolation from the metapsychology via the stated correlations. And furthermore, his view was purportedly a "self-misunderstanding" to the extent that it involved a philosophical misconception of the clinical theory, a body of hypotheses which he *himself* had wrought. Finally, Freud's "scientistic" construal of the clinical theory was far-reaching, if only because it thwarted the recognition of psychoanalysis as a paradigmatically depth-hermeneutic discipline of inquiry, "as the only tangible example of a science incorporating methodical self-reflection," and as potentially prototypic for the other sciences of man (Habermas 1971: chapter 10). Hence, as the hermeneuticians would have it, it was Freud's misguided aim to confer natural science status on his enterprise by extrapolation from his metapsychological program. And this ill-fated endeavor, they tell us, issued in the "scientistic" adulteration of his entire theoretical edifice. Thus, the title of the chapter that Habermas devotes to this indictment begins with the phrase "The Scientistic Self-Misunderstanding of Metapsychology" (p. 246). Indeed, his censure of Freud is indignant and even patronizing:

Because Freud was caught from the very beginning in a scientistic self-understanding, he succumbed to an objectivism that regresses immediately from the level of self-reflection to contemporary positivism in the manner of Mach and that therefore takes on a particularly crude form. [P. 252]

As recently as 1981, Ricoeur again endorsed Habermas's allegation of "scientistic self-misunderstanding" (Ricoeur 1981: 259). And, in an earlier work, Ricoeur hailed the *failure* of Freud's clinical theory to qualify as an empirical or natural science by the received standards as the basis for "a counterattack" against those who deplore this failure (Ricoeur 1970: 358). Remarkably, the psychoanalyst G. S. Klein went Ricoeur one better by holding that the clinical theory and the metapsychology generate



"two incompatible modes of explanation" (Klein 1976: 13), being two quite disparate theories (chapter 2). But, whereas Freud regarded the metapsychology as a "speculative superstructure," Klein rates it as an "expendable . . . obscurantist jargon" issuing in "the dry rot of overconceptualization" (pp. 12-13).

Habermas, Ricoeur, and Klein each give more or less detailed *arguments* for actually denying natural science status to Freud's clinical theory. In so doing, they mean to gainsay Freud's own affirmation of such status after reproaching him for having misextrapolated it from the presumed scientificity of the metapsychology. But, as I have argued, this reproach of misextrapolation is quite ill-founded. Hence, I now turn to the point-by-point scrutiny of the main additional arguments given by these hermeneuticians to sustain their various versions of the charge of "scientistic self-misunderstanding." The bill of particulars that Habermas lodged against the creator of the psychoanalytic clinical theory includes ontological considerations relating to causality as well as epistemic ones pertaining to the validation of particular and general psychoanalytic clinical hypotheses. Since his methodological claims seem to be inspired, to some extent, by his ontological theses, I shall examine the latter first.

It is to be clearly understood at the outset that, far from questioning the etiologic and therapeutic hypotheses of Freud's clinical theory, Habermas claims to take them for granted without ado. What he purportedly tried to controvert by argument are rather the tenets of the philosophical gloss that, as he sees it, Freud misguidedly grafted onto those hypotheses. Hence, in appraising Habermas's arguments and, indeed, *throughout this introductory chapter, I shall refrain from challenging the credentials of Freud's clinical hypotheses*. This challenge is deferred until chapter 2 and subsequent chapters.

## 2. Critique of Habermas's Philosophy of Psychoanalysis

### A. Does the Dynamics of Psychoanalytic Therapy Exhibit the "Causality of Fate"?

It is a cardinal thesis of Habermas's challenge to the founding father that the *lawlike* causal nexus presumably present in the causality of nature does *not* inhere in the therapeutic dynamics of the psychoanalytic process of "self-reflection." In order to justify this denial, he takes as paradigmatic a patient's conquest of the sort of neurosis whose salient manifestation is rigidly *repetitive* behavior. Examples of this manifestation are repetitious obsessional rituals, the recurrent compulsive reenactment or recall of painful experiences, and other repetitive conduct grouped under the rubric of "the repetition compulsion." Habermas's pivotal contention is as follows: Whenever a neurotic overcomes the

repetition compulsion by the lifting of his pathogenic repressions, this psychoanalytic self-reflection has actually “dissolved” and “overcome” the very *causal connection* that had previously linked the pathogen to the compulsively repetitive behavior (Habermas 1971: 271). Yet there is no counterpart to this alleged overcoming of a causal connection “as such” in the domain of the laws of nature. Hence, Habermas uses the Hegelian phrase “causality of fate” to refer to the sort of causal linkage that, he tells us, is “dissolvable,” because it can purportedly be “subdued” by the therapeutic “power of reflection” (Habermas 1971: 256–257, 271; 1970: 302, 304).

But, as I shall now try to show in detail, the reasoning he uses to justify and articulate this “causality of fate” is quite mistaken. Indeed, I shall argue further that a conclusion he draws from his account *flatly repudiates* the psychoanalytic *explanation* for the patient’s therapeutic transition from unconsciously driven behavior to more consciously governed conduct. Yet, paradoxically, Habermas also appears to endorse that psychoanalytic explanation.

To be specific, he uses the phrase “invariance of life history” (1971: 271) to refer to the persistent reenactment or repetition of a certain kind of conduct *C*, a neurotic symptom familiarly designated by the term “repetition compulsion.” As Freudian theory tells us from the start, if the patient actually succeeds in lifting his own repressions, he thereby removes the pathogen *required* to sustain his affliction, and thus rids himself of his compulsion to repeat the behavior *C* (S.E. 1893, 2: 6). In *this* sense one can say, using Habermas’s parlance, that the patient has overcome or dissolved the “invariance” of the conduct *C* that had previously been characteristic of his life history. But if that parlance is used, one must be alert to its potential to mislead by generating fallacies. To see how Habermas is victimized by his idiolectical use of this vocabulary, we need to be more precise as to the pertinent part of Freud’s repression-etiologies of the psychoneuroses.

In their 1893 “Preliminary Communication,” Breuer and Freud drew the epoch-making conclusion that became the pillar of the clinical theory of repression. They enunciated the following etiologic hypothesis: In the pathogenesis of a psychoneurosis, repression plays the generic causal role of a *sine qua non* (S.E. 1893, 2: 6-7; 3: 29-30). The impetus for this assumption avowedly came from their belief that the therapeutic gains from their method of treatment were causally attributable to the cathartic retrieval of traumatic memories, which their patients had repressed. Once they had decided that such lifting of repressions is therapeutic, they wished to *explain* its remedial efficacy. And, as they soon realized,

the desired explanation could be given deductively by the etiologic postulate that repression is causally necessary not only for the initial development of a neurotic disorder but also for its maintenance. Clearly, if a repression of type *R* is indeed the causal *sine qua non* for the presence of a neurosis of kind *N*, then it follows that the removal of *R* will actually issue in the obliteration of *N*. Hence, any patient who rids himself of *R* and thereupon becomes emancipated from *N* plainly *instantiates* that *R* is the causal *sine qua non* for the presence of *N*. Amazingly enough, Habermas claims that this very causal linkage itself is dissolved by the patient's therapeutic achievement. But surely the instantiation of a causal connection cannot possibly also qualify as the dissolution of this linkage! Hence Habermas's notion that a therapeutic achievement can "overcome" an etiologic linkage by dissolving it is incoherent.

Habermas affirms the stated etiologic hypothesis in less precise form. As he puts it, "the assumed causal connection exists between a past conflict situation [the pathogenic repression] and compulsively repeated reactions in the present (symptoms)" (1971: 272). Furthermore, he seems to have appreciated the deductive explanatory relation between the causally necessary condition asserted in the etiologic hypothesis and its therapeutic corollary. For he points out that, "the concept of a causality of the unconscious also renders comprehensible the therapeutic effect of 'analysis.'" And, in any case, he likewise affirms the therapeutic corollary that the pathological *effect C* is removed ("overcome") once the patient's "self-reflection" has terminated the further operation of its unconscious cause.

Unfortunately, however, his parlance then prompts an altogether fallacious slide from the stated therapeutic claim to the following additional conclusion: When the patient prevented the further recurrence of a pathological effect by means of terminating the ongoing operation of its psychic cause in himself, this therapeutic termination *also* dissolved (overcame) the causal connection *itself* that links the pathogenic cause etiologically to its effect (1971: 271). Yet Freud's etiologic hypothesis has told us that this causal linkage is one in which the pathogenic cause is the *sine qua non* of the neurosis. Hence, far from being "dissolved" by the therapeutic conquest of the neurosis, this very causal linkage even *entails* the therapeutic conquest! For just this sort of causal connection assures that the removal of the pathogenic cause of the neurosis will issue in the latter's elimination.

Nonetheless, Habermas slides from the therapeutic conquest of *effects* by the removal of their cause into the dissolution of the causal *linkage* between the pathogen and the neurosis. Overcoming an effect by

undercutting its cause is hardly tantamount to dissolving the causal connection that links them. This illicit slide seems to have occurred in the following passage:

language and behavior are pathologically deformed by the causality of... repressed motives. Following Hegel we can call this the causality of fate, in contrast to the causality of nature. For the causal connection between the original scene, defense, and symptom is not anchored in the invariance of nature according to natural laws but only in the spontaneously generated invariance of life history, represented by the repetition compulsion, which can nevertheless be dissolved by the power of reflection. [1971: 271]

But this statement is incoherent on the face of it. For if—prior to its alleged therapeutic dissolution—there was indeed a causal connection between the repressed “original scene” and the repetition symptom, how can that repetition compulsion also have been “spontaneously generated”? And if it is spontaneously generated, how are we to understand that the stated causal connection is nonetheless “anchored” in it? Thus, one is driven to wonder at times whether Habermas himself knows just what he wishes to claim. In any case, on the same page he concludes explicitly that the patient’s therapeutic achievement was nothing less than the dissolution of the causal connection codified by Freud’s etiologic hypothesis. He contrasts this purported state of affairs with somatic medicine as follows:

In technical control over nature we get nature to work for us through our knowledge of causal connections. Analytic insight, however, affects the causality of the unconscious as such. Psychoanalytic therapy is not based, like somatic medicine, which is “causal” in the narrower sense, on making use of known causal connections. Rather, it owes its efficacy to overcoming causal connections themselves. [1971: 271]

But this declaration is vitiated by Habermas’s failure to heed two logical facts. First, as we have previously noted, it is a *deductive consequence* of Freud’s etiologic hypothesis that the removal of the repression will cause the beneficial disappearance of the neurosis. And furthermore, the patient’s therapeutic achievement *instantiates* this entailed causal connection, since the patient conquers his neurosis by fulfilling its causal antecedent. Hence, the patient achieves his therapeutic gain precisely by *making use* of a causal connection rather than, as Habermas would have it, by “overcoming” such a connection!

Besides, Habermas’s purported dissolution of causality in psychoanalysis boomerangs: To the detriment of his purported dichotomy between the causality of nature and the alleged psychoanalytic causality of fate,

his specious reasoning can be applied to *somatic* medicine as well. For, by complete parity with it, one could deduce the inanity that a person suffering from recurrent gall colics "overcomes" the *causal linkage* between the movement of gall stones and such colics upon taking medication that dissolves the gall stones, and *thereby* terminates the colics!

He courts a further difficulty by neglecting that the therapeuticity of lifting repressions is actually an *explained* consequence of the etiologic causal connection. For by *denying* that the explanatory premise can hold at the time when its therapeutic conclusion is being instantiated, Habermas is repudiating the explanation furnished by the etiologic premise, unencumbered by his previously cited seeming acceptance of the explanation.

Despite having declared the psychoanalytic etiologies therapeutically "dissolvable," he does deem them applicable prior to the effective onset of the therapy. But, in his attempt to vindicate this *circumscribed* validity, he commits a patent causal fallacy by reasoning that repressed motives "have the status of [pathogenic] causes because they assert themselves behind the subject's back" (1970: 297; my translation). Breuer and Freud enunciated that, as a matter of empirical, logically contingent fact, repression is *causally necessary* for the pathogenesis of a psychoneurosis. And, as we recall, they pointed to the therapeuticity of lifting repressions as their evidence for this etiologic hypothesis. Freud then stressed that, as a matter of further empirical fact, repression is *not* causally sufficient for neurosis. Indeed, he hypothesized that hereditary vulnerability, no less than exposure to the repression of experiences, is a causally relevant antecedent for psychopathology (S.E. 1896, 3: 143-146, 209). Plainly, therefore, when Freudian theory postulated the pathogenic causal relevance of repression, it hardly inferred this etiologic role from the *mere* fact that the subject has no conscious awareness of what he repressed. Such a causal inference would be even more primitive than *post hoc ergo propter hoc*. But, as we just saw, Habermas does infer the causal status of unconscious motives as pathogens from the mere fact that they operate "behind the subject's back." Now, warrantedly assertible causes are sometimes said to operate from behind or *a tergo* in the *temporal* sense. Perhaps the intrusion of the latter idea prompted Habermas's causal inference. In any case, this inference makes a mockery of the epoch-making *empirical* argument given by Breuer and Freud as to why repressions should even be deemed causally relevant *at all* to the genesis of psychopathology (S.E. 1893, 2: 6-7). Indeed, as we shall see in chapter 3, despite the brilliance of *their* argument, even their avowedly inductive inference leaves much to be desired. And to this day, it is one of the great

open questions of psychopathology whether its conclusion, which Habermas reaches with such abandon, is empirically true.

Furthermore, Habermas's inference of the "causality of fate" in the patient's therapeutic achievement also boomerangs. If it were legitimate, it could likewise serve to establish the following absurdity: The elementary law of thermal elongation in physics does not exhibit the nomic invariance of the causality of nature after all, because it too could be shown to be "dissolvable" by complete parity with Habermas's flawed reasoning. For consider a metal bar that is isolated against all but thermal influences. It is subject to the law  $\Delta L = \alpha \Delta T \cdot L_0$ , where  $L_0$  is its length at the fixed standard temperature,  $\Delta T$  the temperature increment above or below the standard temperature,  $\Delta L$  the length increase or decrease due to this temperature change, and  $\alpha$  the coefficient of linear thermal expansion characteristic of the particular material composing the metal bar. Now suppose that the bar, initially at the standard temperature, is subjected to a "pathogenic" temperature increase  $\Delta T$ , which produces the elongation  $\Delta L$  as its "pathological" effect. In addition to supplying this "etiology," the law of linear thermal elongation also provides a basis for a corresponding "therapy": It tells us that if the bar's temperature is reduced to its "healthy" standard value, the "pathological" effect  $\Delta L$  will be wiped out. Thus, we can correlate the "therapeutic intervention" of temperature reduction with the patient's remedial lifting of his own repressions. Similarly, we correlate the bar's "neurotic symptom"  $\Delta L$  with the patient's repetition compulsion.

By parity with Habermas's reasoning, we could then draw the following ludicrous conclusion: When the temperature reduction "therapeutically" wiped out the endurance of the "pathological" effect  $\Delta L$  generated by the "pathogenic" temperature increase, this thermal termination also "dissolved" the stated law of thermal elongation. *Habermas has not given a scintilla of evidence for his causality of fate that could not also be adduced, equally speciously, in the thermal reductio ad absurdum argument.* In neither case can there be any question at all of "dissolving" or "overcoming" a causal connection between an initial condition  $I$  and an effect  $E$  on the strength of terminating  $E$  by a suitable alteration of  $I$ . On the contrary, it is  $E$  itself that is "overcome," not—as Habermas would have it—its causal linkage to  $I$ . In *any* therapy—somatic or psychiatric—overcoming an effect is hardly tantamount to dissolving the causal connection linking it to its cause. And, in *either* case, the "conquest" of  $E$  makes use of a causal connection by instantiating it instead of "dissolving" it. Thus, if previously repressed conflicts are consciously worked through by a patient and hence no longer issue in his repetition compulsion, this therapeutic process makes use, as we saw, of a causal connec-

tion entailed by Freud's repression-etiology. There is no therapeutic dissolution of an etiologic connection in Freud's therapy, any more than in the therapies of somatic medicine. Habermas's claim that there is causality of fate in psychoanalytic therapy but causality of nature in the somatic interventions is totally unsubstantiated.

It is quite remarkable that his causality doctrine has been rehearsed without dissent not only by his disciples but also by others who diverge from him on other topics. Among the former, Thomas McCarthy has endorsed it (1979: xiii), after giving a sympathetic account of it (1978: 201). And, in his account of psychoanalytic clinical generalizations, Schöpf (1982: 114) depicts the causal role of unconscious impulses by explicit recourse to the causality of fate.

On the other hand, the psychoanalysts Thomä and Kächele take issue with Habermas on the clinical validation of psychoanalytic interpretations. Yet, they fail to demur when paraphrasing him as follows: "The dissolution of a 'causal connection' by means of the work of interpretation [in the treatment setting] illustrates the efficacy of psychoanalytic therapy" (1973: 320). A demurrer would at least have called for scare quotes around the word "dissolution." Finally, in an article espousing a Popperian critique of hermeneutic methodological separatism, Blight first speaks of Habermas's causality of fate as "the heart of the rationale for claiming that psychoanalysis, as the prototypic form of hermeneutics, is nothing like a natural science" (1981: 172). Yet after quoting, as a single passage, the aforementioned two crucial passages from Habermas (Habermas 1971: 271), Blight writes without hint of any stricture: "In this passage, Habermas, it seems to me, clearly presents his Hegelian case for psychoanalysis as hermeneutics and psychoanalysis as non-science" (1981: 172). Incidentally, in contrast to Blight's judgment that the presentation in the given excerpt is "clear," we have had occasion to deplore—for example à propos of the "spontaneously generated... repetition compulsion"—the exposition as anything but clear.

**B. Are Nomological Explanations in the Natural Sciences Generically Non-Historical, While Causal Accounts in Psychoanalysis are Historically-Contextual?**

The causality of fate is not the only causal doctrine enlisted by Habermas to contrast psychoanalysis ontologically with the natural sciences. He considers the application of psychoanalytic generalizations ("general interpretations," as in 1971: 259) to the life history of a particular analysand. This utilization generates particular interpretations that combine into a narrative. When offered to the individual patient by the analyst, particular interpretations are presumed to be stated in the "intentional" clinical language of desires, affects, fantasies, sensations,

memories, and the like (1971: 272). Since these interpretations are couched in such motivational language, Habermas speaks of them as deriving from the "hermeneutic application" of general interpretations to the life of a particular analysand.

Now, he is concerned to contrast the logic of hermeneutic utilization with the corresponding application of lawlike principles in the nomothetic empirical sciences to particular cases. And his paradigm of the latter sort of instantiation of the antecedent of a physical law is given by: "'this stone' is considered, for example, as 'mass'" (1971: 265). Claiming that "this subsumption is unproblematic" (p. 265), he proposes to contrast it logically with an instantiation of a general interpretation in psychoanalytic practice:

We can at first view a construction offered to the patient by the physician as an explanatory hypothesis derived from a general interpretation and supplementary conditions. . . .

In its logical form, however, explanatory understanding differs in one decisive way from explanation rigorously formulated in terms of the empirical sciences. Both of them have recourse to causal statements that can be derived from universal propositions by means of supplementary conditions: that is, from derivative interpretations (conditional variants) or lawlike hypotheses. Now the content of theoretical propositions remains unaffected by operational application to reality. In this case we can base explanations on context-free laws. In the case of hermeneutic application, however, theoretical propositions are translated into the narrative presentation of an individual history in such a way that a causal statement does not come into being without this context. General interpretations can abstractly assert their claim to universal validity because their derivatives are additionally determined by context. Narrative explanations differ from strictly deductive ones in that the events or states of which they assert a causal relation is [*sic*] further defined by their application. Therefore general interpretations do not make possible context-free explanations. [Pp. 272-273]

Alas, Habermas did not see fit to give a single example from psychoanalysis to lend specificity to this concluding statement of his chapter "The Scientific Self-Misunderstanding of Metapsychology: On the Logic of General Interpretation" (1971, chapter 11). But even if he had, it would have been unavailing to his thesis that—in contrast to psychoanalysis—explanations in physics are generically based on context-free, ahistorical laws. For, as I shall now show, there are telling counterexamples to it from venerable principles of physics. And these counterexamples likewise will be seen to discredit the following equally grandiose assertions by the hermeneutician H. G. Gadamer, who wrote: "It is the aim of science to so objectify experience that it no longer contains any historical element. The scientific experiment does this by its methodical procedure" (1975: 311). But how, one asks, does the scientific experiment



have this ahistorical import? Gadamer reasons that the experimental method of science predicates confirmation on *repeatability to such a degree* that he feels entitled to conclude: "Hence no place can be left for the historicity of experience in science" (p. 311).

The physical theory of classical electrodynamics will now enable me to show that Habermas and Gadamer have drawn a *pseudocontrast* between the nomothetic and human sciences. For that major physical theory features laws that embody a far more fundamental dependence on the history and/or context of the object of knowledge than was ever contemplated in even the most exhaustive of psychoanalytic explanatory narratives or in any recapitulation of human history. Incidentally, Habermas's example of the stone does not even instantiate a context-free physical *law*, but only the theoretical property of mass. Hence, at best, it instantiates a *part* (the antecedent) of a physical law.

Consider an electrically charged particle having an arbitrary velocity and acceleration. We are concerned with the laws governing the electric and magnetic fields produced by this point charge throughout space at any one fixed time  $t$ . In this theory, the influence of the charge on any other test charge in space is postulated to be propagated with the finite velocity of light rather than instantaneously, as in Newton's action-at-a-distance theory of gravitation. But this *noninstantaneous* feature of the propagation of the electrodynamic influence contributes to an important consequence, as follows: At any space point  $P$ , the electric and magnetic fields at a given time  $t$  depend on the position, velocity, and acceleration that the charge had at an earlier time  $t_0$ . That earlier time has the value  $t - r/c$ , where  $r$  is the distance traversed by the influence arriving at  $P$  at time  $t$  after having traveled from the charge to  $P$  with the velocity  $c$  of light.

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

It follows that at ANY ONE INSTANT  $t$ , the electric and magnetic fields produced throughout infinite space by a charge moving with arbitrary acceleration depend on its own PARTICULAR ENTIRE INFINITE PAST KINEMATIC HISTORY! The specifics of this result are evident from the so-called "retarded" expressions for the electric and magnetic fields at a point  $P$  at time  $t$ . These equations specify the fields as functions of the aforesaid kinematic attributes possessed by the charge at the appropriate earlier time (Page and Adams 1940: 144, equations

[48-7] and [48-8]). The authors of one classic treatise state the relevant upshot of these electrodynamic laws as follows: "expressions for the complete field of an element of charge [throughout space at any one time] involve a knowledge of its entire [infinite] past history" (Page and Adams 1940: 161).

Though the individual histories of each of two or more charged particles can be very different indeed, the electrodynamic laws accommodate these differences while remaining general. The generality derives from the *form* of the lawlike functional dependencies of the electric and magnetic field intensities on the earlier accelerations, velocities, and positions of the field-producing charge. But the latter's individual history consists of the infinite temporal series of the particular values of these kinematic attributes (variables).

As against Habermas, I submit that these electrodynamic laws exhibit context-dependence with a vengeance by making the field produced by a charge for any one time dependent on the particular infinite past history of the charge. And, to the detriment of Gadamer, these laws are based on replicable experiments but resoundingly belie his thesis that "no place can be left for the historicity of experience in science."

Indeed, there is a simple special case of the above very general laws whose *incomplete* statement can misleadingly suggest the context-independence of its validity: Coulomb's inverse square law for the electric field of a point charge. Being an inverse square law, it has the same dependence of the field on the distance from the field-producing entity as Newton's law of universal gravitation for point masses. Yet Newton's law is context-free in the sense of holding regardless of the state of motion of the gravitational masses. Besides, that law is an instantaneous action-at-a-distance law. By contrast, Coulomb's law is highly context-dependent by holding only if the field-producing charge has been *permanently* at rest for all past time. And our earlier considerations enable us to trace this important historical condition of its validity to its being a special case of a delayed action law.

There are other instructive cases of context-dependence of laws of physics that reveal further the poverty of Habermas's supposed paradigmatic example of "'this stone' is considered, for example, as 'mass'" (1971: 265). This class of cases exhibits "hysteresis" in the sense that a property of a physical system, induced by a given present influence upon it, depends not only on that present influence but also on the *past history* of variation of that influence. Thus, "hysteresis" has been defined quite generally as "that property of an element evidenced by the dependence of the value of the output, for a given excursion of the input, upon the history of prior excursions and the direction of the current traverse"

(Considine 1976: 1335). One such case is the hysteresis behavior of highly magnetizable metals (e.g., iron, cobalt, nickel, etc.), which are known as "ferromagnetic."

To be specific, let a previously unmagnetized sample of such a metal be subjected to an external magnetic field  $H$  generated by an alternating current. Then the magnetizing force  $H$  will produce an internal magnetization induction  $B$ . The value of  $B$  will increase from zero as  $H$  increases from that initial value, but  $B$  will approach and attain a limiting or saturation value with the further increase of  $H$ . Yet, the subsequent reduction of  $H$  to zero will *not* issue in the return of the internal magnetization  $B$  to zero. Thus, we can already see that the present response of the ferromagnetic material to one and the same external influence  $H$  will depend on the prior magnetization *history* of the given sample:  $B$  lags behind  $H$ , since it does not decrease to zero when  $H$  returns to zero after being nonzero.

Physicists speak of the residual internal magnetization  $B$  as "the remanence," because it betokens the influence of the sample's prior history of magnetization on its present response to the same external field  $H$ . Indeed, as shown by the pertinent law, if the last trace of this remanence is to be obliterated, the value of  $H$  will actually have to be made negative by reversing its direction. One cycle of magnetization and demagnetization can now be completed by a further decrease and then increase of  $H$  until the saturation state of  $B$  is reached again.

But, very significantly, after one such complete cycle, the dependence of the behavior of the sample on its magnetization *history* further makes itself felt as follows: After the first cycle is depicted graphically by plotting  $B$  against  $H$ , the closed curve ("hysteresis loop") representing that initial cycle will *never* be retraced by subsequent cycles of demagnetization and remagnetization (Efron 1967: 694). Another example in physics in which the response of materials to current influences is sensitive to the history of their prior exposure to like influences is furnished by solids that exhibit *elastic* hysteresis. In this case, the stress and strain (deformation) are the variables corresponding to  $H$  and  $B$ . Even rubber bands display a like behavior. Other examples include the electric hysteresis exhibited by dielectric substances in electric fields and the hysteresis of a radiation counter tube.

Some hermeneuticians may retort that these physical cases do not capture the relevant sense of "history." As if to say: "What is all-important here is *how* past states count in the determination of present behavior, not just *that* they count." Patently, it is anything but a liability to my argument that I rely on the following banal fact: The Freudian narratives adduced by Habermas are psychological, whereas my exam-

ples of context-dependence are avowedly physical. But this assumed ontological difference is itself unavailing to Habermas's thesis that there is an *asymmetry* of context-dependence, whenever general propositions are applied explanatorily to particular instances. For he rules out *simplificiter* just the sort of ingreience of history in physical laws that I have multiply documented: As he would have it (1971: 272-273), in the natural sciences, the laws are context-free *and* remain so, when applied to explain particular cases, whereas concrete psychoanalytic explanations are generically context-dependent. Thus, the stated attempt to parry my critique fails, if only because it modifies, rather than rescues, Habermas's contention. Besides, the modification is unavailing to the alleged asymmetry, since the adduced platitude—that Freudian narratives are *psychological*—is patently insufficient to sustain the asymmetry.

The context-dependent physical laws that I have adduced seem tailor-made as insuperable difficulties for the second of Habermas's alleged causal dichotomies. And I trust that the context-dependence of the physical cases I have developed adequately matches, if not surpasses, the degree of such dependence, if any, encountered when general clinical hypotheses are applied to particular patient histories.

In another connection, the analyst Wallerstein (1976: 222) illustrates such applications by reference to the psychoanalytic explanations of the idiosyncratic deployment of defense mechanisms. Such explanations are furnished, he notes, "*in terms* of the operation of particular combinations and permutations of the generally available armamentarium of possible defense mechanisms in human character." By pointing to such Freudian accounts of "the idiosyncratic patterning and deployment of defensive behaviors," Wallerstein exemplifies his conception of psychoanalysis as "truly a science which is both a general psychology, a study of the general and lawful functioning of the human mind in health and disease, and also [the study of] an idiosyncratic genetic unfolding of . . . the individual" (pp. 222-223).

Habermas's paradigm of the stone that has mass epitomizes the mythic universal notion of the laws of nature on which he relied to gain adherents for his dichotomy of context-dependence. And it is a commentary on our intellectual culture that by trading on such stone age physics, as it were, he and Gadamer managed to parlay the limitations of their own personal scientific horizons into a vaunted pseudocontrast between the humanistic disciplines and the natural sciences.

So much for the two theses on causality enlisted by Habermas to vindicate his reproach of "scientistic self-misunderstanding" against Freud's own construal of the clinical theory. Hence, we now turn to the appraisal of the *epistemic* allegations he puts forward in his further endeavor to sustain this indictment.