PHILOSOPHY, SCIENCE, AND PSYCHOANALYSIS

A CRITICAL MEETING

EDITED BY

SIMON BOAG, LINDA A.W. BRAKEL, AND VESA TALVITIE

KARNAC

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KARNAC

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INTRODUCTION

Vesa Talvitie, Linda A. W. Brakel, and Simon Boag

The perennial interest in psychoanalysis shows no signs of abating, and the longevity of psychoanalytic theory is seen in the varied extensions and elaborations of Freudian thinking in the fields of neuroscience and cognitive theory. The enduring interest in psychoanalysis is, in many respects, understandable: psychoanalytic theory addresses such issues as unconscious mental processes, self-deception, and wish-fulfilment, and makes bold claims in terms of using these concepts to explain both everyday behaviour and clinical phenomena. Nevertheless, while developments in mainstream psychology have repackaged many of Freud's ideas (demonstrating the vitality of Freud's thinking), there remains doubt about the veracity of psychoanalytic claims, and questions concerning the place of psychoanalysis vis-à-vis science. Furthermore, developments in the fields of philosophy, psychology, and the cognitive—and neurosciences—since Freud's time also require consideration with respect to appreciating their implications for contemporary psychoanalysis. Such considerations may have important practical implications, since psychoanalysis is not simply concerned with theory for theory's sake and instead considers the implications of theory for therapeutic practice. However, assessing psychoanalysis in light of modern-day research is not an easy project, not least because

the complexity of psychoanalytic theory raises complex scientific and philosophical questions concerning the nature of mind and the nature of the scientific enterprise itself. Accordingly, a fresh evaluation of psychoanalysis in the new millennium entails a perspective that is at once scientific and philosophical and represents the junction where philosophy, science and psychoanalysis meet.

This book aims to provide a forum within which discussions of psychoanalysis and psychoanalytic research go beyond partitioning philosophy and science, and sees, instead, a rigorous science as being inherently philosophical in nature. But the first obstacle in any discussion here is that the topic of philosophy, science and psychoanalysis branches in many directions. It would be desirable that a book on this topic interests both psychoanalytic audiences and "outsiders" to the field. This is, however, a rather difficult criterion to meet for many reasons. The title *Philosophy, Science, and Psychoanalysis* covers a wide range of topics, and no one person will be fully acquainted with or interested in them all. Some topics are, for want of a better term, "solipsistic"—of primary interest only to a psychoanalytic audience. Counter to that, other topics deal with nuances of philosophical issues, and a clinically oriented reader, for instance, may find both following these issues and appreciating their significance difficult. Additionally, readers will all arrive at the material presented in this volume from their own explicit or implicit philosophies, the assumptions of which may hinder communication and appreciation for the positions presented here.

Nevertheless, it is important not to avoid this kind of challenge. Due to the nature of Sigmund Freud's work, a wide range of topics including both abstract scientific and philosophical issues are embedded within psychoanalytic thinking. Thus, as long as psychoanalysis contains relevance for people, we must keep on diving into those deep waters of science and philosophy. The situation is somewhat circular: if psychoanalysis withdraws from studying topics such as those treated in this volume, it loses its relevance to the academic community.

The importance of the history of psychoanalysis

Due to the enduring respect for the founder's writings, one cannot make sense of psychoanalysis' (prevailing) relations to science and philosophy without knowing the zeitgeist of the late nineteenth-century middle Europe where German romanticism still had a notable impact

on people's thinking. Considering the topic of philosophy, Freud, as a young man, and his contemporaries read such notable figures as Kant and Hegel. Despite appearances to the contrary in his later life, Freud enjoyed reading philosophical writings in his youth, and only later developed a dismissive stance toward the discipline. In fact, Alfred Tauber (2011) describes Freud as a "reluctant philosopher" (see however, Boag, 2011a). When analytic philosophy emerged, Freud was in his fifties and subsequently some would say that Freud and his contemporaries were not terribly interested in such questions as "how should the term 'mental' be defined?"—even if he did offer a subtle and effective philosophical argument against his detractors, who claimed that psychological processes and contents must be conscious, *by definition* (Freud, 1915e). It was not until more than a decade after Freud's death that philosophers began to talk about (Wittgensteinian) language games. Thus the question concerning the relation between language and the world, and more generally the entire philosophy of language, were not particularly vivid for Freud, even if he believed that language was necessary for consciousness (e.g., Freud, 1900a, 1915e, 1940a[1938]). Additionally, when reading Freud and talking about his ideas we easily bypass the above matters, as we tend to downplay the significant temporal distance between us and Freud. For most readers, Kantian thinking and positivism appear as opposite and incompatible philosophical orientations. However, it seems that Freud had no difficulty in possessing sympathies toward both of them (Tauber, 2011), a trait also reflected in Freud's acceptance at various points in his writings of contradictory positions generally (for instance, see Petocz's (2006) discussion of Freud's discussion of the mind-body relation). Had Freud paid greater attention to philosophical issues then possibly there would be fewer disputes and disagreements concerning Freudian theory today.

On the other hand, with respect to scientific issues, the temporal distance is also salient in the case of Freud's appreciation of evolutionary theory. When Darwin's *Origin of Species* was published in 1859, Freud was three-years old. It is easy in retrospect to attribute a contemporary (Darwinian) understanding of evolution to Freud's thinking, but, as generally known, Freud also retained an adherence to Lamarckian lines of thought until the end of his life (see Jones, 1957, pp. 310–311; let us mention that recently certain Lamarckian-like ideas have been revived in the domain of epigenetics). Moreover, the development of psychoanalysis occurred prior to the introduction of psychopharmaceuticals.

For example, in the domain of psychiatry, first generation antipsychotic drugs were developed in the 1950s and evidence-based medicine emerged at the end of the twentieth century—about half a century after Freud's death. Such factors provide some context of the world in which psychoanalysis began. On top of this, our notions of philosophy and science, and the ideals concerning them, are developing and changing all of the time, as do conceptions of psychoanalysis. For instance, in France an idiosyncratic mixture of continental philosophy and psychoanalytic ideas emerged, while in Britain, Wilfred Bion created an original conception of psychoanalysis, and in the United States, psychoanalysis took place in the context of empirical psychology and was influenced by the idea of information-processing. Psychoanalytic clinical theory and its method of cure have also experienced a number of branchings. Currently there are numerous psychoanalytical schools, whose interrelations are not always especially warm and accepting of one another.

When looking at the (big) picture painted above, it is clear that one cannot compose a definitive handbook on the relations between psychoanalytic, scientific and philosophical ideas. Nonetheless, there have been some notable attempts beginning at least from the year 1959, when *Psychoanalysis, Scientific Method, and Philosophy*, edited by Sidney Hook, was published. The book is based on the presentations held in the Second Annual Meeting of the New York University Institute of Philosophy. In its preface the editor states that the seminar was probably the first in the United States where "a distinguished group of psychoanalysts has met with a distinguished group of philosophers of science in a free, critical interchange of views on the scientific status of psychoanalysis" (Hook, 1959, p. xiii). Following that, several edited books have been published, including *Philosophical Essays on Freud* (Wollheim & Hopkins, 1982), *The Cambridge Companion to Freud* (Neu, 1991), *Mind, Psychoanalysis and Science* (Clark & Wright, 1998), *Freud 2000* (Elliot, 1998), *The Analytic Freud: Philosophy and Psychoanalysis* (Levine, 2000), *The Freud Encyclopedia: Theory, Therapy, and Culture* (Erwin, 2002) and *Psychoanalysis at the Limit: Epistemology, Mind and the Question of Science* (Mills, 2004). Thus there is a relatively long-standing recognition of the importance of both philosophical and scientific issues within psychoanalysis.

The topics covered within these volumes may be classified in many ways, the simplest being according to the issues included. Also, alongside of each specific topic, the basic approach of an article may be categorised as either *intrapsychoanalytic* or *interdisciplinary*. With the former

approach, psychoanalysis is treated as a more or less independent discipline possessing its own methodology and object of study. A writer aims to reveal something about a phenomenon by applying psychoanalytic theories or viewpoint. On that basis, a writer may create contributions of an aesthetic, historical, ethical, or educational nature, or of a cultural or philosophical nature without necessarily referring to studies put forward outside psychoanalysis. Alternatively, studies that may be called *interdisciplinary* are grounded on the assumption that psychoanalysis and certain other disciplines possess considerable shared interests and objectives. On one hand this implies that psychoanalytic viewpoints may enrich, for example, the biologists' or philosophers' thinking, and on the other that psychoanalytic hypotheses may be tested and evaluated with the help of the outcomes of (for example) neuroscience or cognitive psychology. Articles falling into this category may be critical toward psychoanalysis, or the author(s) may argue that non-psychoanalytic studies support psychoanalyts' insights.

The other additional core topic of this book is Adolf Grünbaum's critique of psychoanalysis. Although far newer than conflicts over the status of unconscious mentation, Professor Grünbaum's views have already been debated for more than three decades. Perhaps no single contributor's criticism in the academic world has been as influential and long-standing as that of Grünbaum's. While many advocates of psychoanalysis are perhaps irritated about Grünbaum's persistent argumentation, psychoanalysis should be (also) honestly grateful to him: during past decades the discussions around Grünbaum's criticisms have been among the most notable link between psychoanalysis and the academic world. Thanks to Professor Grünbaum, psychoanalysis is less isolated from the academic world than it would be otherwise. In this volume we find Grünbaum's latest formulation of his critique; within the same spirit is Edward Erwin's criticism of psychoanalysis, and there are several reflections on the plausibility and significance of Grünbaum's and Erwin's arguments—particularly Linda A. W. Brakel's reply to Grünbaum and Agnes Petocz's discussion of the scientific status of psychoanalysis.

Psychoanalysis will of course face challenges from science and philosophy beyond those treated in this volume. Developments in neuroscience will continue to warrant re-examining the nature of psychoanalytic theories. Also, increasingly, the market place, insurance companies and public administration will all clamour for

evidence-based evaluation of all psychological and psychiatric treatments. The talking-cure method of psychoanalysis cannot and should not bypass this issue. While we do not address these problems directly, we hope that this volume will have laid the groundwork for facing these future challenges.

CHAPTER ONE

Critique of Psychoanalysis*

Adolf Grünbaum

I. Introduction

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

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The cathartic *lifting* of the repression yielded relief from the particular hysterical symptom. Breuer and Freud believed that they could therefore hypothesise that the *repression*, coupled with affective suppression, was the crucial cause for the development of the patient's psychoneurosis (1895d, pp. 6–7, 29–30).

Having reasoned in this way, they concluded in Freud's words:

> Thus one and the same procedure served simultaneously the purposes of [causally] investigating and of getting rid of the ailment; and this unusual conjunction was later retained in psycho-analysis. (1924f, p. 194)

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

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

Yet Freud was careful to highlight the contribution he made himself after the termination of his collaboration with Breuer. Referring to himself in the third person, he tells us:

> Freud devoted himself to the further perfection of the instrument left over to him by his elder collaborator. The technical novelties which he introduced and the discoveries he made changed the cathartic method into psycho-analysis. (1924f, p. 195)

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

By now, the psychoanalytic enterprise has completed its first century. Thus, the time has come to take thorough *critical* stock of its past performance qua theory of human nature and therapy, as well as to have a look at its prospects. Here I can do so only in broad strokes.

It is important to distinguish between the validity of Freud's work qua *psychoanalytic* theoretician, and the merits of his earlier work, which would have done someone else proud as the achievement of a life-time. Currently, Mark Solms, working at the Unit of Neuro-surgery of the Royal London Hospital (Whitechapel) in England, is preparing a five-volume edition of *Freud's Collected Neuroscientific Writings*

for publication in all of the major European languages. One focus of these writings is the neurological representation of mental functioning; another is Freud's discovery of the essential morphological and physiological unity of the nerve cell and fibre. They also contain contributions to basic neuroscience such as the histology of the nerve cell, neuronal function and neurophysiology. As a clinical neurologist, Freud wrote a major monograph on aphasia (Solms & Saling, 1990). As Solms points out in his preview *An introduction to the neuro-scientific works of Sigmund Freud* (unpublished), Freud wrote major papers on cerebral palsy that earned him the status of a world-authority. More generally, he was a distinguished pediatric neurologist in the field of the movement disorders of childhood. Furthermore, Freud was one of the founders of neuro-psychopharmacology. For instance, he did scientific work on the properties of cocaine that benefited perhaps from his own use of that drug. Alas, that intake may well also account for some of the abandon featured by the more bizarre and grandiose of his psychoanalytic forays.

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

In 1880, he published a (free) translation of some of J. S. Mill's philosophical writings (Stephan, 1989, pp. 85–86). Yet he was often disdainful of philosophy (Assoun, 1995), despite clearly being indebted to the Viennese philosopher Franz Brentano, from whom he had taken several courses: The marks of Brentano's (1874) quondam representationalist and intentionalist account of the mental are clearly discernible in Freud's conception of ideation. And the arguments for the existence of God championed by the quandam Roman Catholic priest Brentano further solidified the thoroughgoing atheism of Freud, the "godless Jew" (Gay, 1987, pp. 3–4).

II. History and logical relations of the "dynamic" and "cognitive" species of the unconscious

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

At the turn of the 18th century, Leibniz gave psychological arguments for the occurrence of *sub-threshold* sensory perceptions, and for the existence of unconscious mental contents or motives that manifest themselves in our behaviour (Ellenberger, 1970, p. 312). Moreover, Leibniz pointed out (1981, p. 107) that when the contents of some forgotten experiences subsequently emerge in our consciousness, we may *misidentify* them as *new* experiences, rather than recognise them as having been unconsciously stored in our memory. As he put it (1981, p. 107):

> It once happened that a man thought that he had written original verses, and was then found to have read them word for word, long before, in some ancient poet. … I think that dreams often revive former thoughts for us in this way.

As Rosemarie Sand has pointed out (private communication), Leibniz's notion anticipates, to some extent, Freud's dictum that "*The interpretation of dreams is the royal road to a knowledge of the unconscious activities of the mind*" (1900a, p. 608).

Before Freud was born, Hermann von Helmholtz discovered the phenomenon of "unconscious inference" as being present in sensory perception (Ellenberger, 1970, p. 313). For example, we often unconsciously infer the *constancy* of the *physical* size of nearby objects that move away from us, when we have *other* distance cues, although their *visual* images decrease in size. Similarly, there can be unconsciously

inferred constancy of brightness and colour under changing conditions of illumination, when the light source remains visible. Such unconscious *inferential compensation* for visual discrepancies also occurs, when we transform our *non*-Euclidean (hyperbolic) binocular *visual* space into the "seen" Euclidean physical space (Grünbaum, 1973, pp. 154–157).

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

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

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

> Freud did not seem to take seriously the possibility that cognition and thought could be inherently programmed to reflect reality and could have their own structure and development—an assumption basic to cognitive psychology. After World War II, the psychoanalyst Heinz Hartmann was driven, by facts of biological maturation discovered *non*-psychoanalytically, to acknowledge in his so-called "ego psychology" that such functions as cognition,

> memory and thinking can develop autonomously by innate genetic programming, and independently of instinctual drive gratification. (Eagle, 1993, pp. 374–376)

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

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

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

More importantly, some Freudian apologists have overlooked that even after the two different species of the genus "unconscious" are thus made logically *compatible*, the dynamic unconscious as such cannot derive any *credibility* from the presumed existence of the cognitive unconscious. Nonetheless, faced with mounting attacks on their theory and therapy, some psychoanalysts have made just that fallacious claim.

Thus, the Chicago analyst Michael Franz Basch (1994, p. 1) reasoned in vain that since neurophysiological evidence supports the hypothesis of a *generic* unconscious, "psychoanalytic theory has passed the [epistemological] test with flying colours." On the contrary, we must bear in mind that evidence for the cognitive unconscious does not, as such, also furnish support for the dynamic unconscious as such.

III. Has psychoanalytic theory become a staple of western culture?

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

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

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

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

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

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

As for the soundness of the design of Schüttauf *et al*., Hans Eysenck (private communication to Rosemarie Sand, March 1, 1996; cited by permission to her) has raised several objections: (i) "as the author [Schüttauf] himself acknowledges, this is not an experiment, as

ordinarily understood; it is a simple correlational study … correlation cannot be interpreted as causation, which he unfortunately attempts to do," (ii) The members of the experimental group were severely neurotic, while the control group were normals. But "the proper control group would have been severely [disturbed] neurotics suffering from a different form of neurosis than that of obsessive compulsive behaviour," (iii) "Freudian theory posits a causal relationship between the anal stage of development and obsessive compulsive neurosis; the author does not even try to document this hypothetical relationship", (iv) "obsessive-compulsive neurotics suffer from fear of dirt and contamination, so that on those grounds alone they would be likely to react differentially to stimuli suggesting such contamination. … It is truly commonsensical to say that people whose neurosis consists of feelings of dirt will react differentially to verbal presentations of words related to dirt."

Naturally, I sympathise with Schüttauf and his co-workers in their avowed effort (Section 4) to escape my criticism (Grünbaum, 1984, pp. 202–205) of an earlier purported experimental confirmation of Freud's theory of slips by M. T. Motley (1980). I had complained that the independent variable Motley manipulated in his speech-error experiments did *not* involve *unconscious* antecedents—but only conscious ones. As Schüttauf *et al*. tell us, precisely in order to escape my criticism of Motley, they relied on Freud's aetiology of obsessive-compulsive neurosis to infer that subjects who exhibit the symptoms of that neurosis fulfil the requirement of harbouring repressions of anal-sadistic wishes. Thus, *only* on that etiologic assumption does their use of compulsive subjects *and* their manipulation of words pertaining to anal-sadistic themata warrant their expectation of a higher incidence of verbal slips in this group than among normals.

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

Thus, as long as good empirical support for the Freudian scenario is unavailable, we actually don't know whether any *bona fide* Freudian slips exist at all. Just this lack of evidence serves to undermine Nagel's thesis that cultural influence is a criterion of validity. After all, if we have no cogent evidence for the existence of genuinely Freudian slips,

then Freud's theory of bungled actions ("*parapraxes*") might well be false. And if so, it would not contribute one iota to its validity, even if our entire culture unanimously believed in it, and made extensive explanatory use of it: When an ill-supported theory is used to provide explanations, they run the grave risk of being bogus, and its purported insights may well be *pseudo*-insights.

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

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

For centuries, even as far back as in New Testament narratives, both physical disease and insanity have been attributed to demonic possession in Christendom, no less than among primitive peoples. That demon theory has been used, for example, to explain deafness, blindness, and fever, as well as such psychopathological conditions as epilepsy, somnambulism, and hysteria. Our contemporary medical term

"epilepsy" comes from the Greek word "epilepsis" *for seizure*, and reflects etymologically the notion of being seized by a demon. Since exorcism is designed to drive out the devil, it is the supposed *therapy* for demonic possession. In the Roman Catholic exorcist ritual, which has been endorsed by the present Pope and by Cardinal O'Connor of New York, the existence of death is blamed on Satan. And that ritual also survives in baptism as well as in blessing persons or consecrating houses.

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

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

IV. Critique of Freudian and post-Freudian psychoanalysis

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

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

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

It will be expositorily expeditious to deal with Freud's earlier etiologic argument within Section B below, and to appraise the subsequent one, which goes back to his "Dora" case history of 1905, in Section C (Freud, 1905e). But, also for expository reasons, it behooves us to devote a prior Section A to his account of the actuation of the hypothesised mechanism of repression by "motives of unpleasure."

A. Negative affect and forgetting

As Freud told us, "The theory of repression is the cornerstone on which the whole structure of psycho-analysis rests. It is the most essential part of it" (1914d, p. 16). The *process* of repression, which consists in the banishment of ideas from consciousness or in denying them entry into it, is itself presumed to be unconscious (1915d, p. 147). In Freud's view, our neurotic symptoms, the manifest contents of our dreams, and the slips we commit are each constructed as "compromises between the demands of a repressed impulse and the resistances of a censoring force

in the ego" (1925d, p. 45; 1917, p. 301). By being only such compromises, rather than fulfilments of the instinctual impulses, these products of the unconscious afford only *substitutive* gratifications or outlets. For brevity, one can say, therefore, that Freud has offered a unifying "compromise-model" of neuroses, dreams and parapraxes.

But what, in the first place, is the *motive* or cause that initiates and sustains the operation of the unconscious mechanism of repression *before* it produces its own later effects? Apparently, Freud assumes *axiomatically* that distressing mental states, such as forbidden wishes, trauma, disgust, anxiety, anger, shame, hate, guilt, and sadness—all of which are *unpleasurable*—almost always actuate, and then fuel, *forgetting* to the point of repression. Thus, repression regulates pleasure and unpleasure by defending our consciousness against various sorts of *negative affect*. Indeed, Freud claimed perennially that repression is the paragon among our *defence* mechanisms (Thomä & Kächele, 1987, vol. 1, pp. 107–111). As Freud put it dogmatically: "The tendency to forget what is disagreeable seems to me to be a quite universal one" (1901b, p. 144), and "the recollection of distressing impressions and the occurrence of distressing thoughts are opposed by a resistance" (1901b, p. 146).

Freud tries to disarm an important objection to his thesis that "distressing memories succumb especially easily to motivated forgetting" (1901b, p. 147). He says:

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

He acknowledges as "also a true fact" that "distressing things are particularly hard to forget" (1916, pp. 76–77).

For instance, we know from Charles Darwin's Autobiography that his father had developed a remarkably retentive memory for painful experiences (cited in Grünbaum, 1994), and that a half century after Giuseppe Verdi was humiliatingly denied admission to the Milan Music Conservatory, he recalled it indignantly (Walker, 1962, pp. 8–9). Freud himself told us as an adult (1900a, p. 216) that he "can remember

very clearly," from age seven or eight, how his father rebuked him for having relieved himself in the presence of his parents in their bedroom. In a frightful blow to Freud's ego, his father said: "The boy will come to nothing".

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

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

Astonishingly, Freud thinks he can parry this basic statistical and explanatory challenge by an evasive dictum as follows: "… mental life is the arena and battle-ground for mutually opposing purposes [of forgetting and remembering] (1916a, p. 76) …; there is room for both. It

is only a question … of what effects are produced by the one and the other" (p. 77). Indeed, just that question cries out for an answer from Freud, if he is to make his case. Instead, he cavalierly left it dangle epistemologically in limbo.

B. The epistemological liabilities of the psychoanalytic method of free association

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

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

One of Freud's justifications for the use of free association as a *causally probative* method of dream investigation leading to the identification of the repressed dream thoughts, he tells us (1900a, p. 528), is that it "is identical with the procedure [of free association] by which we

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

To articulate the argument adequately, however, we must still clarify Freud's original basis for claiming that (unsuccessful) repression is indeed the pathogen of neurosis. Only then will he have made his case for claiming that free association is etiologically probative, because it is uniquely capable of uncovering repressions. The pertinent argument is offered in Breuer and Freud's "Preliminary Communication" (1895d, pp. 6–7). There they wrote (p. 6, italics in original):

> For we found, to our great surprise at first, that *each individual hysterical symptom immediately and permanently disappeared when we had succeeded in bringing clearly to light the memory of the event by which it was provoked and in arousing its accompanying affect, and when the patient had described that event in the greatest possible detail and had put the affect into words*. Recollection without affect almost invariably produces no result. The psychical process which originally took place must be repeated as vividly as possible; it must be brought back to its *status nascendi* and then given verbal utterance.

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

> It is plausible to suppose that it is a question here of unconscious suggestion: the patient expects to be relieved of his sufferings by this

> procedure, and it is this expectation, and not the verbal utterance, which is the operative factor. This, however, is not so. (p. 7)

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

In sum, Breuer and Freud inferred that the therapeutic removal of neurotic symptoms was produced by the cathartic lifting of the patient's previously ongoing repression of the pertinent traumatic memory, not by the therapist's suggestion or some other placebo factor (See Grünbaum, 1993a, ch. 3 for a very detailed analysis of the placebo-concept). We can codify this claim as follows:

> T. Therapeutic Hypothesis: Lifting repressions of traumatic memories cathartically is *causally relevant* to the disappearance of neuroses.

As we saw, Breuer and Freud (p. 6) reported the immediate and permanent disappearance of each hysterical symptom after they cathartically lifted the repression of the memory of the trauma that occasioned the given symptom. They adduce this "evidence" to draw an epoch-making inductive *etiologic* inference (p. 6), which postulates "a causal relation between the determining [repression of the memory of the] psychical trauma and the hysterical phenomenon". Citing the old scholastic dictum "*Cessante causa cessat effectus*" ("when the cause ceases, its effect ceases"), they invoke its contrapositive (p. 7), which states that as long as the effect (symptom) persists, so does its cause, (the repressed memory of the psychical trauma). And they declare just that to be the pattern of the pathogenic action of the repressed psychical trauma. This

trauma, we learn, is *not* a mere *precipitating* cause. Such a mere "*agent provocateur*" just releases the symptom, "which thereafter leads an independent existence". Instead, "the [repressed] memory of the trauma … acts like a foreign body which long after its entry must continue to be regarded as an agent that is still at work" (p. 6).

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

> E. Etiologic Hypothesis: An ongoing repression accompanied by affective suppression is causally necessary for the initial pathogenesis *and* persistence of a neurosis.

(This formulation of the foundational aetiology of psychoanalysis supersedes the one I gave at the hands of a suggestion by Carl Hempel and Morris Eagle [in Grünbaum, 1984, p. 181, last paragraph]. The revised formulation here is faithful to Breuer and Freud's reference to "accompanying affect" [p. 6] à propos of the traumatic events whose repression occasioned the symptoms.)

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

But I maintain that this inductive argument is vitiated by what I like to call the "*fallacy of crude hypothetico-deductive ('H-D') pseudo-confirmation*". Thus note that the remedial action of aspirin-consumption for tension-headaches does not lend H-D support to the outlandish etiologic hypothesis that a hematolytic aspirin-*deficiency* is a causal *sine qua non* for having tension headaches, although such remedial action is validly deducible from that bizarre hypothesis. Twenty-five years ago, Wesley Salmon called attention to the fallacy of inductive causal inference from mere valid H-D deducibility by giving an example in which a deductively valid pseudo-explanation of a man's avoiding pregnancy can readily give rise to an H-D pseudo-confirmation of the addle-brained attribution of his non-pregnancy to his consumption of birth-control pills. Salmon (1971, p. 34) states the fatuous pseudo-explanation:

> John Jones avoided becoming pregnant during the past year, for he had taken his wife's birth control pills regularly, and every man who regularly takes birth control pills avoids pregnancy.

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

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

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

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

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

First, Freud inferred that the therapeutic disappearance of the neurotic symptoms is *causally attributable* to the cathartic lifting of repressions *by means of the method free associations*. Relying on this key therapeutic hypothesis, he then drew two further major theoretical inferences: (i) The seeming removal of the neurosis by means of cathartically *lifting* repressions is good inductive evidence for postulating that repressions accompanied by affective suppression are themselves *causally necessary* for the very existence of a neurosis (1895d, pp. 6–7), and (ii) Granted that such repressions are thus the essential causes of neurosis, *and* that the method of free association is uniquely capable of uncovering these repressions, this method is uniquely competent *to identify the causes* or pathogens of the neuroses. (Having convinced himself of the causal probativeness of the method of free associations on therapeutic grounds in the case of those neuroses he believed to be successfully treatable, Freud also felt justified in deeming the method reliable as a means of unearthing the etiologies of those *other* neuroses—the so-called "narcissistic ones", such as paranoia—which he considered psychoanalytically *untreatable*.)

But the argument fails for the following several reasons: In the first place, the durable therapeutic success on which it was predicated did not materialise (Borch-Jacobsen, 1996), as Freud was driven to admit both early and very late in his career (1925d, p. 27; 1937c, pp. 23, 216–253). But even in so far as there was transitory therapeutic gain, we saw that Freud *failed* to rule out a rival hypothesis that undermines his attribution of such gain to the lifting of repressions by free association: The ominous hypothesis of placebo effect, which asserts that treatment ingredients *other than* insight into the patient's repressions—such as the mobilisation of the patient's hope by the therapist—are responsible for any resulting improvement (Grünbaum, 1993a, ch. 3). Nor have other analysts ruled out the placebo hypothesis during the past century. A case in point is a 45-page study "On the Efficacy of Psychoanalysis" (Bachrach *et al.*, 1991), published in the official *Journal of the American*

Psychoanalytic Association. Another is the account of analytic treatment process by (Vaughan & Roose, 1995).

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

C. Freud's etiologic transference argument

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

According to Freud's theory of transference, the patient *transfers* onto his psychoanalyst feelings and thoughts that originally pertained to important figures in his or her earlier life. In this important sense, the fantasies woven around the psychoanalyst by the analysand, and quite generally the latter's conduct toward his or her doctor, are hypothesised to be *thematically recapitulatory* of childhood episodes. And by thus being recapitulatory, the patient's behaviour during treatment can be said to exhibit a thematic kinship to such very early episodes. Therefore, when the analyst interprets these supposed re-enactments, the ensuing interpretations are called "transference interpretations."

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

Thus, in the context of Freud's transference interpretations, the thematic re-enactment is claimed to show that the early scenario had originally been *pathogenic*. According to this etiologic conclusion, the patient's thematic re-enactment in the treatment setting is also asserted

to be *pathogenically* recapitulatory by being pathogenic in the adult patient's here-and-now, rather than only thematically recapitulatory. Freud (1914d, p. 12) extols this dubious etiologic transference argument in his "History of the Psycho-Analytic Movement," claiming that it furnishes the most unshakable proof for his sexual aetiology of all the neuroses:

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

On the contrary, the patient's thematically recapitulatory behaviour toward his doctor *does not show* that it is also *pathogenically* recapitulatory. How, for example, does the re-enactment, during treatment, of a patient's early conflict show at all that the original conflict had been pathogenic in the first place? Quite generally, how do transference phenomena focusing on the analyst show that a presumed current replica of a past event is *pathogenic* in the here-and-now?

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

Marshall Edelson (1984, p. 150) has offered a rebuttal to my denial of the cogency of the etiologic transference argument:

> … in fact, in psychoanalysis the pathogen is not merely a remote event, or a series of such events, the effect of which lives on. The pathogen reappears in all its virulence, with increasing frankness and explicitness, in the transference—in a new edition, a new

> version, a re-emergence, a repetition of the past pathogenic events or factors.

And Edelson elaborates (p. 151):

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

But how does the psychoanalyst demonstrate, within the confines of his clinical setting, that the supposed *current* replica of the remote, early event is *presently* the virulent *cause* of the patient's neurosis, let alone that the original pathogen is replicated at all in the transference? Having fallaciously identified a conflict as a pathogen because it reappears in the transference, many Freudians conclude that pathogens must reappear in the transference. And, in this way, they beg the key question I have just asked. How, for example, did Freud show that the Rat Man's marriage conflict depicted in that patient's transference fantasy was the *current* cause of his *ongoing death obsessions*? Neither Edelson's book, nor his (1986) paper, offer a better answer. Thus, in the latter paper, he declares: "The psychoanalyst claims that current mental representations of particular past events or fantasies are constitutive (i.e., *current* operative) causes of current behaviour, and then goes on to claim that therefore past actual events or fantasies are etiological causes of the analysand's symptoms." And Edelson concludes: "Transference phenomena are … nonquestion-begging evidence for … inferences about causally efficacious psychological entities existing or occurring in the here and now" (p. 110).

In sum, despite Edelson's best efforts, the etiologic transference argument on which both Freud and he rely is ill-founded: (i) They employ epistemically circular reasoning, when inferring the occurrence of infantile episodes from the adult patient's reports, and then claiming that these early episodes are thematically recapitulated in the adult analysand's conduct toward the analyst; (ii) they beg the *etiologic* question by inferring that, qua being thematically recapitulated, the infantile

episodes had been pathogenic at the outset; (iii) they reason that the adult patient's thematic re-enactment is *pathogenically* recapitulatory such that the current replica of the infantile episodes is pathogenic in the here-and-now.

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

D. Free association as a method of dream interpretation

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

But what do free associations tell us about our dreams? Whatever the manifest content of dreams, they are *purportedly wish-fulfilling* in at least two logically distinct specific ways as follows: for every dream D, there exists at least one normally unconscious infantile wish W such that (i) W is the motivational cause of D, and (ii) the manifest content of D graphically displays, more or less disguisedly, the state of affairs desired by W. As Freud opined (1925d, p. 44): "When the latent dream-thoughts that are revealed by the analysis [via free association] of a dream are examined, one of them is found to stand out from among the rest … the isolated thought is found to be a wishful impulse …" . But Freud manipulated the free associations to yield a distinguished wish-motive (Glymour, 1983).

Quite independently of Freud's abortive therapeutic argument for the causal probativeness of free association, he offered his analysis of his 1895 "Specimen Irma Dream" as a *non*-therapeutic argument

for the method of free association as a cogent means of identifying hypothesised hidden, forbidden wishes as the motives of our dreams. But in my detailed critique of that unjustly celebrated analysis (Grünbaum, 1984, ch. 5), I have argued that Freud's account is, alas, no more than a piece of false advertising: (i) It does not deliver at all the promised vindication of the probativeness of free association, (ii) it does nothing toward warranting his foolhardy dogma that *all* dreams are wishfulfilling in his stated sense, (iii) it does not even pretend that his alleged "Specimen Dream" is evidence for his compromise-model of manifest-dream content, and (iv) the inveterate and continuing celebration of Freud's analysis of his Irma Dream in the psychoanalytic literature as the paragon of dream-interpretation is completely unwarranted, because it is mere salesmanship.

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

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

Freud's initial 1900 statement of his dual wish-fulfilment in dreams had been: "*Thus its content was the fulfilment of a wish and its motive was a wish*" (1900a, p. 119). But the sense in which dreams are wish-fulfilling

overall is purportedly *threefold* rather than only two-fold: One motivating cause is the universal *preconscious* wish-to-sleep, which purportedly provides a generic causal explanation of dreaming *as such* and, in turn, makes dreaming the guardian of sleep (1900a, pp. 234, 680); another is the individualised *repressed* infantile wish, which is activated by the day's residue and explains the *particular* manifest *content* of a given dream; furthermore, as already noted, that manifest content of the dream graphically displays, more or less disguisedly, the state of affairs desired by the unconscious wish. The disguise is supposedly effected by the defensive operation of the "dream-*distortion*" of the content of forbidden unconscious wishes.

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

But in a 1993 book (Grünbaum, 1993a, ch. 10), I have argued that this dream theory of universal wish-fulfilment should be presumed to be false at its core rather than just ill-founded.

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

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

that free associations reliably identify repressions to be the causes of symptoms, dreams, and slips.

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

As a corollary of the latter epistemological defects of the method of free association, it appears that such associations *cannot* reliably vouch for the *contents* of presumed past repressions that are lifted by them. Thus, the products of such associations cannot serve to justify the following repeated claim of the later (post-1923) Freud: The mere painfulness or unpleasurableness of an experience is *not itself* the prime motive for its repression; instead, its negativity must involve the conscious emergence of an instinctual desire that is recognised by the super-ego as illicit or dangerous (1940a[1938], pp. 184–187; 1933a, pp. 57, 89, 91, 94; 1937c, p. 227).

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

E. The Explanatory pseudo-unification generated by Freud's *compromise-model of neuroses, dreams, and slips*

My indictment of the compromise-model, if correct, spells an important lesson, I claim, for both philosophical ontology and the theory of scientific explanation. Advocates of psychoanalysis have proclaimed it to be an explanatory virtue of their theory that its compromise-model gives a *unifying* account of such *prima facie* disparate domains of phenomena as neuroses, dreams, and slips, and indeed that the theory

of repression also illuminates infantile sexuality and the four stages hypothesised in Freud's theory of psychosexual development. In fact, some philosophers of science, such as Michael Friedman, have hailed explanatory unification as one of the great achievements and desiderata of the scientific enterprise. Thus, one need only think of the beautiful way in which Newton's theory of mechanics and gravitation served all at once to explain the motions of a pendulum on earth and of binary stars above by putting both terrestrial and celestial mechanics under a single theoretical umbrella.

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

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

F. The "hermeneutic" reconstruction of psychoanalysis

The French philosopher Paul Ricoeur (1970, p. 358), faced with quite different criticisms of psychoanalysis from philosophers of science during the 1950s and 1960s (von Eckardt, 1985, pp. 356–364), hailed the *failure* of Freud's theory to qualify as an empirical science by the received standards as the basis for "a counter-attack" against those who deplore this failure. In concert with the other so-called "hermeneutic" German philosophers Karl Jaspers and Jürgen Habermas, Ricoeur believed that

victory can be snatched from the jaws of the *scientific failings* of Freud's theory by abjuring his scientific aspirations as misguided. Claiming that Freud himself had "scientistically" misunderstood his own theoretical achievement, some hermeneuts misconstrue it as a *semantic* accomplishment by trading on the multiply ambiguous word "meaning" (Grünbaum, 1984, Introduction, Sections 3 and 4; 1990, 1993a, ch. 4). In Freud's theory, an overt symptom manifests one or more underlying unconscious causes and gives evidence for its cause(s), so that the "sense" or "meaning" of the symptom is constituted by its latent motivational cause(s). But this notion of "meaning" is different from the one appropriate to the context of *communication*, in which *linguistic* symbols *acquire semantic* meaning by being used deliberately to designate their referents. Clearly, the relation of being a manifestation, which the symptom bears to its cause, differs from the semantic relation of designation, which a linguistic symbol bears to its object.

The well-known academic psychoanalyst Marshall Edelson (1988, ch. 11, "Meaning", pp. 246–249) is in full agreement with this account and elaborates it lucidly:

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

That the mental phenomenon substitutes for these states is a manifestation of a causal sequence (pp. 247–8). And drawing on Freud's compromise-model of symptoms in which symptoms are held to provide *substitutive* outlets or gratifications, Edelson continues:

> Suppose the question is: "Why does the analysand fear the snake so?" Suppose the answer to that questions is: "A snake stands for

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

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

Speaking of Freud's writings, Edelson (1988, p. 247) says illuminatingly:

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

The "hermeneutic" reconstruction of psychoanalysis slides illicitly from one of two familiar senses of "meaning" encountered in ordinary discourse to another. When a paediatrician says that a child's spots on the skin "*mean* measles," the "meaning" of the symptom is constituted by

one of its *causes*, much as in the Freudian case. Yet, the analyst Anthony Storr (1986, p. 260), when speaking of Freud's "making sense" of a patient's symptoms, conflates the fathoming of the *etiologic* "sense" or "meaning" of a symptom with the activity of making *semantic* sense of a text (Grünbaum, 1986b, p. 280), declaring astonishingly: "Freud was a man of genius whose expertise lay in semantics." And Ricoeur erroneously credits Freud's theory of repression with having provided, *malgré lui*, a veritable "semantics of desire."

In a book that appeared before (Grünbaum, 1990; 1993a, ch. 4), Achim Stephan (1989, Section 6.7, "Adolf Grünbaum," pp. 144–149) takes issue with some of my views. (Quotations from Stephan below are my English translations of his German text.) He does not endorse Ricoeur's "semantics of desire" (p. 123). But he objects (p. 146, item (3)) to my claim that "In Freud's theory, an overt symptom manifests one or more underlying unconscious causes and gives evidence for its cause(s), so that the 'sense' or 'meaning' of the symptom is constituted by its latent motivational cause(s)."

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

Stephan does countenance (p. 148) my emphasis on the distinction between the relation of manifestation, which the symptom bears to its cause, and the semantic relation of designation, which a linguistic symbol bears to its object. Yet, his principal objection to my view of the psychoanalytic "sense" of symptoms as being causal manifestations of unconscious ideation is that I assign "exclusively non-semantic significance" to them by *denying* that they also have "semiotic" significance like linguistic symbols (pp. 148–149). He grants that Freud did not construe the sense or meaning of symptoms as one of semantic reference to their causes. Yet according to Stephan's own reconstruction of Freud's conception, "he did assume that the manifest phenomena [symptoms]

semantically stand for the same thing as the (repressed) ideas for which they substitute", i.e., "they stand semantically for what the repressed (verbal) ideas stand (or rather would stand, if they were expressed verbally)" (p. 149).

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

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

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

Yet some version of a hermeneutic reconstruction of the psychoanalytic enterprise has been embraced with alacrity by a considerable number of analysts no less than by professors in humanities departments of universities. Its psychoanalytic adherents see it as buying absolution for their theory and therapy from the criteria of validation mandatory for causal hypotheses in the empirical sciences, although psychoanalysis is replete with just such hypotheses. This form of escape from accountability also augurs ill for the future of psychoanalysis, because the methods of the hermeneuts have not spawned a single new important hypothesis. Instead, their reconstruction is a negativistic ideological battle cry whose disavowal of Freud's scientific aspirations presages the death of his legacy from sheer sterility, at least among those who demand the validation of theories by cogent evidence.

G. Post-Freudian psychoanalysis

But what have been the contemporary *post*-Freudian developments insofar as they still qualify as psychoanalytic in content rather than only in name? And have they advanced the debate by being on firmer

epistemological ground than Freud's original major hypotheses (Grünbaum, 1984, ch. 7)? Most recently, the noted clinical psychologist and philosopher of psychology Morris Eagle (1993) has given a comprehensive and insightful answer to this question on which we can draw.

Eagle (1993, p. 374) begins with a caveat: "It is not at all clear that there is a uniform body of thought analogous to the main corpus of Freudian theory that can be called contemporary psychoanalytic theory. In the last forty or fifty years there have been three major theoretical developments in psychoanalysis: ego psychology, object relations theory, and self-psychology. If contemporary psychoanalytic theory is anything, it is one of these three or some combination, integrative or otherwise, of the three." Eagle makes no mention of Lacan's version of psychoanalysis, presumably because he does not take it seriously, since Lacanians have avowedly forsaken the need to validate their doctrines by familiar canons of evidence, not to mention Lacan's wilful, irresponsible obscurity and notorious cruelty to patients (Green, 2007).

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

Yet these two post-Freudian schools not only diverge from Freud but also disagree with one another. Thus, the orthodox psychoanalysts Arlow and Brenner speak ruefully of "the differences among all these theories, so apparent to every observer" (p. 9), hoping wistfully that refined honing of the psychoanalytic method of free association will yield a common body of data, which "would in the end resolve the

conflict among competing theories" (p. 11). But their hope is utopian, if only because of the severe probative limitations of the method of free association. How, for example, could a method of putting adults on the couch possibly have the epistemological resources to resolve the 3-way clash between the Freudian and two post-Freudian schools in regard to the *infantile* etiologies of psychopathology? Otto Kernberg's (1993) account of the "Convergences and Divergences in Contemporary Psychoanalytic Technique" does not solve that problem. And, as other psychoanalysts themselves have documented, there are several clear signs that the future of the sundry clinical and theoretical enterprises that label themselves "psychoanalytic" is now increasingly in jeopardy. For example, the pool of patients seeking (full-term) psychoanalytic treatment in the United States has been steadily shrinking, and academic psychoanalysts are becoming an endangered species in American medical schools (Reiser, 1989). No wonder that the subtitle of the 1988 book *Psychoanalysis* by the well-known analyst Marshall Edelson is "*A Theory in Crisis*" (Edelson, 1988).

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

H. What are the future prospects of psychoanalysis?

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

> Of some things about the future of psychoanalysis we can be certain. Fortunately, they are the most important issues as well. Psychoanalysis will continue to furnish the most comprehensive

> and illuminating insight into the human psyche. It will continue to stimulate research and understanding in many areas of human endeavour. In addition to being the best kind of treatment for many cases, it will remain, as it has been, the fundamental base for almost all methods that try to alleviate human mental suffering by psychological means.

By contrast, a dismal verdict is offered by the distinguished American psychologist and psychoanalyst Paul E. Meehl (1995, p. 1021). Since one of my main arguments figures in it, let me mention that, à propos of my critiques of Freud's theories of transference and of obsessional neurosis (Rat Man), I had demonstrated the *fallaciousness* of inferring a *causal* connection between mental states from a mere "meaning" or thematic connection between them. Meehl refers to the latter kind of shared thematic content as "the existence of a theme":

> His [Grünbaum's] core objection, the epistemological difficulty of inferring a causal influence from the existence of a theme (assuming the latter can be statistically demonstrated), is the biggest single methodological problem that we [psychoanalysts] face. If that problem cannot be solved, we will have another century in which psychoanalysis can be accepted or rejected, mostly as a matter of personal taste. Should that happen, I predict it will be slowly but surely abandoned, both as a mode of helping and as a theory of the mind [reference omitted].

Returning to Arlow and Brenner, I hope I have shown that, in regard to the last 100 years, their rosy partisan account is very largely ill-founded, if only because the lauded comprehensiveness of the core theory of repression is only a *pseudo*-unification, as I have argued. Among Arlow and Brenner's glowingly optimistic statements about the future, just one is plausible: The expectation of a continuing heuristic role for psychoanalysis. Such a function does *not* require the correctness of its current theories at all. As an example of the heuristic role, one need only think of the issues I raised à propos of Freud's dubious account of the relation of affect to forgetting and remembering. These issues range well beyond the concerns of psychoanalysis. As the Harvard psychoanalyst and schizophrenia researcher Philip Holzman sees it (Holzman, 1994, p. 190): "This view of the heuristic role of psychoanalysis, even in

the face of its poor science, is beginning to be appreciated only now." Holzman (private communication) mentions three areas of inquiry as illustrations: (i) The plasticity and reconstructive role of memory as against photographic reproducibility of the past, (ii) the general role of affect in cognition, and (iii) the relevance of temperament (e.g., shyness) in character development, as currently investigated by Jerome Kagan at Harvard.

CHAPTER TWO

Psychoanalysis and philosophy of science: basic evidence

Edward Erwin

Albert Einstein repeatedly congratulated Sigmund Freud for the latter's "brilliant achievement" without ever endorsing the truth of any of Freud's theories. After receiving one more such letter from Einstein, sent to honour Freud's eightieth birthday, Freud exclaimed: "But I have often asked myself what indeed there is to admire about them (his theories) if they are not true—i.e., if they do not contain a high degree of truth" (Grubrich-Simitis, 1995, p. 121).

There was a time not long ago when many philosophers of science would have disagreed with Freud's comment. Many thought of scientific theories as shorthand for observation statements or as predictive devices useful for generating research but having no capacity to explain anything. Views of this sort have been refuted and are rejected by most contemporary philosophers of science—even anti-realists, such as Bas van Fraassen (1980). Theories, at least in the social sciences, consist of one proposition or more. If true, they may explain quite a lot. If Freud is right, his theories explain to some degree the content of dreams, the etiology of neuroses, and how psychoanalysis works. But they explain only if true (see below the section on inference to the best explanation). So, before we admire their explanatory value, we need to ascertain if

they are true, or, as Freud says, whether they contain a high degree of truth.

When non-philosophers such as post-modernist thinkers hear of a quest for truth they immediately sense problems about its nature. Is truth not just high probability or agreement among scientists or something useful to believe? Such ancient questions can now be resolved quite simply: the condition for a proposition being true is just what is stipulated by Tarski's schema, named after the logician Alfred Tarski. The proposition expressed by the sentence "Snow is white", for example, is true if and only if snow is white. The proposition expressed by "All dreams are wish fulfilments" is true if and only if all dreams are wish fulfilments. The condition on the right-hand side of the "if and only if" does not require a high probability, or agreement, or utility.

Tarski's schema tells us what it is for something to be true, but it is obviously of no help in determining which propositions are true and which are false. To determine truth, we clearly need evidence. In Freud's case, the status of the evidence for or against his theories and therapeutic claims has long been disputed. Some of these disputes are mainly empirical and need not raise any significant philosophical issue. Other disagreements raise fundamental questions. Can evidence obtained by observing patients in psychoanalysis support Freudian theory or indeed any type of psychoanalytic theory? Freud thought so, but others have challenged him on this issue (Grünbaum, 1984). Another question is even more fundamental: what counts as evidence for any psychological theory or indeed for any sort of theory?

To answer this question, it seems natural to appeal to a definition of "evidence" or perhaps a theory of evidence, such as Bayesianism (Howson & Urbach, 1989) or one of the newer theories (Achinstein, 2001; Mayo, 2005). Unfortunately, none of the philosophic definitions or theories of evidence are of much help in resolving disputes about the psychoanalytic evidence; the reasons are spelled out in Erwin (2013). Even if I were wrong about this, there is an alternative: figure out what counts as evidence per se and what counts only as derived evidence.

Evidence per se

Evidence per se is evidence that all by itself supports some hypothesis. Any evidence that is not evidence per se is derived evidence.

If a therapist were to say that a certain score on the Blacky Pictures Test counts as evidence for castration anxiety in a patient, presumably the claim would not be that it would be evidence independent of all empirical assumptions. Such scores are typically counted as confirming evidence only if there is empirical evidence that the test measures what it purports to measure, which means that a separate assumption about validity is presupposed. Such scores, in so far as they constitute evidence, are derived evidence, derived in part from other empirical assumptions.

In contrast, if a psychotherapist looks right at the first number on a printed page, the fact that it appears to be seventy-one is normally taken to be evidence all by itself that the number on the page is seventy-one. No further argument or evidence would be needed except perhaps in very special circumstances, such as where the eyesight of the examiner is known to be poor. Such evidence is considered to be what I am calling "evidence per se"; other philosophers call it "basic evidence".

Just because something is evidence per se on a given occasion does not mean that it always is. Certain conditions have to be met. If a client enters my office, my looking right at her and seeming to see that she has arrived for her appointment is evidence all by itself that she has shown up, but if the client appears in the street fifteen blocks from me, looking directly in her direction might fail to give me any good reason to believe that it is her unless I have evidence for other assumptions, such as evidence about her walking along the same street every day at the same time.

If evidence per se depends on the circumstances, it might appear that the evidence per se/derived evidence distinction collapses. My looking right at a number on a page is evidence that it is there only if certain assumptions are true, such as that my eyesight is not poor; the score on the Blacky Pictures Test is evidence for a diagnosis only if an empirical assumption about the validity of the test is true. What is the difference?

The difference is that in the first case, no positive evidence is needed that my eyesight is working properly; all that is needed is that it be working properly, whether we have evidence for this or not, and there be no contrary evidence. This is not true in the second case. In order for the Blacky Pictures test score to have probative value, positive evidence is needed for the validity of the test.

In any event, I will assume that some evidence is evidence all by itself and that other evidence, indeed most evidence, is only derived evidence. It is not evidence independent of all empirical assumptions. The best candidate for being evidence per se is of course observational evidence. No one challenges this, except someone who denies that any type of evidence is evidence per se. I will not discuss this position here, but I believe it leads to a total scepticism about evidence, a position not attractive to anyone trying to provide evidential support for a psychoanalytic theory.

Assuming that empirical evidence sometimes counts as basic evidence, what else counts? What follows is not meant to be a complete list of proposals, but it does cover those that have played a key role in debates about psychoanalysis.

Explanatory power, simplicity, and parsimony

Many scientists and philosophers hold that if one theory has more explanatory power than a rival, or is simpler or more parsimonious in that it postulates fewer types of entities, then it should be preferred. Some state this view in a needlessly bold form. Paul Kline, in reply to an argument of mine, says: "Where there are competing explanations, the simplest is to be preferred: Occam's razor or the law of parsimony" (Kline, 1988, p. 226).

This is too bold because a less simple theory may have powerful empirical support that overrides simplicity. A more defensible view is that when and only when two competing theories are empirically equivalent, simplicity, explanatory power or parsimony can break the tie. So amended, I agree with the view, but it is neutral between two conflicting philosophical views about evidence. The first says that these pragmatic factors count as evidence in and of themselves; they qualify as evidence per se. The second view says that when they count as evidence, it is always because of empirical backing: they are at best derived evidence.

Psillos (1996) appears to be taking the first position in his discussion of Fresnel and Arago's discovery about the propagation of light. He is using this case to support his view that when our background information does not suggest exactly one theoretical explanation, explanatory considerations are called forth to select the best among the hypotheses that entail the evidence. If such considerations counted

only derivatively—that is, only when empirical evidence made it likely that the hypothesis with the most explanatory power is the true one, then the argument in its favour would be entirely empirical. We would be relying on empirical evidence to select the best—that is, the one most likely to be true.

According to Psillos, the hypothesis that the propagation of light is a uniquely and exclusively transversal process was singled out by Fresnel as the best explanation of polarisation after ruling out the sole rival theory. If this were all there was to Fresnel's argument, then he would not have had any basis for his conclusion. Merely by ruling out the only competitor that was considered, he would have left open the possibility that a theory he had not contemplated explained the phenomena.

As Psillos points out, however, there was more to the argument. According to Fresnel, given the background wave theory of light, the phenomena he was trying to explain could be explained on the assumption that light waves are purely transversal. But this too would not be enough. So far, the claim is merely that the purely transversal theory could explain the phenomena if it were true. It is potentially explanatory. But Fresnel would still need evidence that in fact it does explain and this would require showing that the theory is true, not merely potentially explanatory.

Fresnel did have such an argument. First, he argues that the empirical evidence for the background theory of light made it likely that at least one of the two theories being considered gave the correct explanation. He then discounted the rival theory. This is where the issue of explanatory considerations comes in. Fresnel claimed that if the rival theory were true, it would have to explain something it could not explain: the disappearance of the longitudinal wave after the light wave had gone past the polariser. But why think that the rival theory would need to explain this unless there was empirical evidence supporting this claim? Without supporting empirical evidence, neither Fresnel nor Arago could have ruled out the rival to their own theory.

Assuming they had such evidence, their argument is through and through empirical. They rule out all possible explanations of the phenomena except two by appealing to empirical evidence for the background theory of light, and then they rule out one of the remaining two theories by appeal to further empirical evidence, evidence that the rival theory would be false if it did not explain the disappearance of the longitudinal wave. In this case, explanatory considerations count as

evidence, but only in virtue of background evidence. It does not count as evidence in and of itself.

There have been many other attempts to show that explanatory power, simplicity or parsimony in and of themselves have evidential value, but there is a basic problem argued by Elliot Sober (1990), which, I believe, has not been overcome. One of Sober's examples concerns the units of selection controversy in evolutionary biology. Some biologists have argued that group selection hypotheses are less simple than hypotheses claiming that the unit of section is the individual or the gene. If they were right to infer that the former hypotheses have a lower initial probability than rival hypotheses, this might appear to be an instance where simplicity makes an epistemic difference—all by itself.

As Sober demonstrates, however, it is not simplicity, but empirical details about natural selection that justify the initial assignment of a lower probability for group selection hypotheses. The general difficulty is this: in cases where simpler hypotheses should be preferred, how do we show that it is simplicity itself and not empirical details that supply the justification? (Erwin, 1996; Sober, 1990).

We have, then, two rival epistemic theories about cases where simpler theories or theories with more explanatory power are justifiably preferred. One says that simplicity or explanatory power by themselves are evidence for a theory; the other says that background evidence alone provides the warrant. If we cannot point to something that favours the first theory, there is at least one good reason for not according simplicity or explanatory power separate epistemic value. The reason is this. If we do not, then we do not have to answer the difficult question: why would simplicity or explanatory power in and of themselves provide a reason for believing that a theory is true?

For precision, fecundity, and other pragmatic factors, it is even harder to make the case that their possession by itself adds to the likelihood of a theory being true. A precise theory may be easier to test than a less precise theory, and this might be reason to employ it for certain practical purposes, but why believe that if both theories are tested, the more precise theory is more likely to be true?

Fruitful theories, if true, explain more than their less fruitful competitors and they may have other advantages, but in the absence of empirical data grounding the claim that if they are more fruitful they are more likely to be true, why believe that they will beat their competitor with respect to truth?

Conclusion: The verdict about simplicity, explanatory power and precision should be given for all of the "pragmatic virtues". They should count as derived evidence when background evidence supports this, but until it is shown otherwise, they should not be classified as evidence per se.

Inference to the best explanation

The rule of inference to the best explanation (IBE) may seem an odd choice for the role of basic evidence. IBE, after all, is a rule of inference, not a form of evidence. Still, if the rule is valid, then the fact that some hypothesis H provides the best available explanation of event E is evidence all by itself that H is true. This makes the evidence basic.

Someone who disagrees with my terminology can treat the proposal advanced by supporters of IBE as about a basic rule of causal inference, not about evidence. Nothing of substance turns on characterising IBE only as a rule or treating the evidence it yields as a form of as evidence per se. IBE will get the most attention because it is so widely accepted not just in philosophy of science, but in other branches of philosophy. It is also widely endorsed or implicitly relied on in many non-philosophic disciplines.

Although there are variations in the wording, here is a standard version of the IBE rule:

Assume (1), (2), and (3):

1. Event E (or events E1, E2 … En) occurred
2. Hypothesis H explains E
3. H provides the best of the available explanations of E.

From the above propositions, infer that H is probably true. For a very similar version, see Lycan (1988).

Some clarifications are needed, especially about the criteria for deciding which explanation is best, but I will address this issue later. The rule of IBE has been widely used in the philosophy of science especially to defend scientific realism's claim that hypotheses referring to unobservables such as unconscious wishes or black holes can be empirically confirmed. It also has been widely used in metaphysics, epistemology, and ethics. When psychoanalysts rely on IBE, they do not necessarily announce this fact, but they often do rely on it implicitly. An

analyst in the course of a three-year treatment may propose tentative explanations of the origins of the client's problem, but at some point may defend a final interpretation by arguing that compared to its rivals, this last one provides the best explanation of all of the relevant clinical facts. Similar sorts of arguments are given in the Freudian experimental literature.

IBE is also brought in explicitly by some philosophers who defend psychoanalytic theory. M. Michael, in his reply to Rosemarie Sand's (2012) criticisms of Freud's dream theory and what she calls the "free association fallacy", first tries to show that non-Freudian alternatives proposed to explain Freud's data are inferior to Freud's explanation. He then lays out the theoretical foundation for claiming that there is evidence in support of the dream theory:

> Here is a rough outline of how inference to the best explanation works. One observes a phenomenon—say, that free association enables the interpretation of dreams. One then posits an explanation of this—say, that, in general, thoughts free associated with a dream are ones that are causally active at the time the dream was formed. Supposing that this is a good enough explanation and that no better one exists, one then infers it is the correct explanation. (Michael, 2012, p. 106)

Sebastian Gardner (1993) makes a similar use of IBE to support psychoanalytic theory: "I will argue that psychoanalytic theory provides the most penetrating and satisfying explanation of irrationality. Given that irrationality is real, and requires explanation, this amounts to an argument for the truth of psychoanalytic theory (p. 1)." Michael Lacewing also claims that use of inference to the best explanation provides support for parts of psychoanalytic theory (Lacewing, 2012).

The bad lot objection

Despite the plausibility of IBE, philosophers of science have raised a deep difficulty known as the "bad lot" objection (Achinstein, 1992; Van Fraassen, 1980; see as well, Erwin, 1996). The objection is that if we compare only hypotheses we have thought of, as we do when using IBE, and argue that one of these is the best of the set of available explanations, we have no ground for thinking that the set is not just a bad lot,

that the correct explanation of the phenomena does not lie outside the set. It is one we have not yet formulated.

Not everyone is convinced by the bad lot objection. In the second edition to his *Inference to the Best Explanation* (2004 [1991]), Peter Lipton (p. 152) reconstructs it as an argument with two premises. The first, the "ranking" premise, says that the testing of theories yields only a *comparative* warrant; scientists can only rank the competing theories they are contemplating with respect to likelihood of truth. The second premise, the "no privilege" premise, says that scientists have no reason to suppose that the process by which they generate theories for testing makes it likely that the set of available explanations contains the true one.

Lipton has no trouble creating problems for each premise. This is hardly surprising. It is easy to show that there are cases in which there is independent evidence that the available set of explanatory hypotheses contains the true one. Where we have this independent empirical evidence, we have more than a comparative ranking. We have evidence that the most likely hypothesis in the set is true. So, Lipton is right about the first premise he criticises.

He is also right about the second premise. In some cases, the independent evidence concerns the way the favoured hypothesis was generated. This fact refutes the "no privilege" premise. If scientists have independent empirical evidence that, given the way the set of available hypotheses were generated, it contains the correct explanation, then the set is "privileged" by this evidence.

The standard objection to IBE, however, does not rest on either of the two premises criticised by Lipton. As to the first, the standard objection implies that in cases where *IBE has application* there is only comparative warrant. Where we have independent evidence that the set of available explanations contains the true one, IBE has no application. To see this, suppose we obtain empirical evidence that either A, B, or C is true, and then discover additional evidence that B is more likely to be true than the other two. We then have an empirical argument for B, but one that makes no use of IBE. Explanation is not even mentioned in the argument.

The same point applies to Lipton's second premise. Scientists might have grounds in a particular case for thinking that the process by which they generated the theories being considered makes it likely that a true theory will be among those we know of. But then the scientists would

have independent evidence that the set of considered hypotheses contains the true one. The next required step would be to get evidence that one of these is more likely than its competitors. In short, they would need further evidence that of A, B, C, one of these, say, B, is more likely than its competitors. As before, this straightforward empirical argument makes no use of IBE.

The philosopher of science Stathis Psillos's (1996) also challenges the bad lot objection. He argues that the objection works only on the assumption that the only relevant information is that the hypotheses we are considering entail the evidence; background information is excluded. This is wrong, he says, because we often have background evidence that is relevant. This fact suggests two aspects of privilege. The first is: sometimes the background information drastically narrows down the list of potential explanations. The second aspect is that when the background information does not suggest one theoretical explanation, then explanatory considerations are called forth to select the best among the hypotheses that entail the evidence.

Psillos's first point is the same as Lipton's: there are cases in which there is background evidence that the set of known hypotheses contains the true one. Where we have this independent empirical evidence, we have more than a comparative ranking. We have evidence that the most likely hypothesis in the set is true. For example, in the early 1980s there were a half dozen hypotheses proposed to explain the etiology of HIV, including one about the sheer quantity of sexual experiences, another that postulated the Epstein-Barr virus as the cause, and several others that postulated one of the retroviruses, perhaps type I, or type II, or type III, as the cause. New evidence was then discovered that the cause was a retrovirus. This discovery narrowed down the set of potential hypotheses to just three: the three retrovirus hypotheses. Two of these were then discounted on empirical grounds, leaving the remaining one the winner.

The reply here is the same as the one given earlier to Lipton. Scientists had background evidence that made it likely that one of the retrovirus hypotheses was the true one. They then obtained additional empirical evidence that ruled out two, thus providing evidence for the remaining one. The argument from beginning to end was empirical; inference to the best explanation played no role.

Psillos's second point, however, requires a different answer. When the background information does not suggest exactly one theoretical

explanation, explanatory considerations, he claims, are called forth to select the best among the hypotheses that entail the evidence.

This gets us to the issue I alluded to earlier. How do we decide which of several competing hypotheses is the best? Some who defend IBE take the position that the best explanation is the most likely; and only observational evidence determines what is most likely. In short, they do not appeal to pragmatic factors. A more common view is that non-observational factors—such as simplicity, parsimony, or explanatory power—can also help identify the hypothesis most likely to be true. I argued earlier, however, that at best these factors count as derived evidence: they provide grounds for belief only in virtue of empirical backing. If this is right, then the additional evidence that makes B more likely than A or C will always be empirical.

There is a deeper problem, however, about explanatory power that has so far gone unremarked. The philosopher Norwood Russell Hanson and others once argued that because explanatory power has epistemic force, general relativity theory was well supported before being subjected to any experimental test. The fact that Einstein's theory explained so much not explained by any other theory was itself good grounds, Hanson claimed, for thinking it was at least approximately true. Hanson, however, was wrong. Once his mistake is exposed, it should become clearer why IBE is a hopeless rule. (The mistake, by the way, is not one made by most current defenders of IBE.)

In what follows, I will restrict H to a simple causal hypothesis of the form "Event C caused event E". It would not matter if H were to speak instead of C-type events generally causing E-type events, or whether it said that C was the most important cause of E, or was causally relevant to the occurrence of E, or is generally a contributing causal factor, etc. The point can also be extended to any version of a causal hypothesis, as well as to non-causal explanations.

In the IBE rule, what is meant by "Hypothesis H *explains*" an event? If taken literally, H does not explain event E if H is false. Only true hypotheses can explain why events occur. This seems obvious, but not everyone agrees. So, it is better to state the argument.

If the hypothesis H says that C caused E, there are only three conditions that would make H false. Either C did not occur (there is no explainer), or the alleged effect E did not occur (there is nothing to explain), or there was no causal connection between them (the relationship was not causal). If any one of these falsifying conditions is met,

then the hypothesis does not explain the occurrence of the event C. But if they are all met, the hypothesis is true. Hence, H explains the occurrence of E if and only if H is true. (I omit considerations of approximate truth.)

Because hypotheses cannot explain unless true, we cannot even apply IBE without first getting evidence that H is true, and so too for its competitors. But, then, what would be the point of using IBE in the first place if the sole goal is to get evidence that H is true? We would have obtained evidence that H is true *before* employing IBE—that is, before finding grounds for thinking that it beats its competitors.

There is an obvious way to avoid this problem and most defenders of IBE take this way out. Interpret "H explains E" to mean "H would explain if it were true". In other words, in using IBE, we do not compare competing explanations; we compare *potential* explanations. This is what Lipton says. He agrees that explanations must be true, but he replies that in speaking of inference to the best explanation, we are not speaking of explanations; rather we are referring to hypotheses *that would explain if true*. We are referring to potential explanations. Lycan (1988) says the same thing and so do almost all defenders of IBE. The Lipton-Lycan reply is correct, but their answer gives rise to the deeper objection to IBE.

If, once again, H says that C caused E, then to say that H explains E is just to say that H correctly identifies the cause of E, and to say that H is true, is, once more, to say that H correctly identifies the cause of E. But, then, what are we saying when we say "If H were true, it would explain E"? We are saying no more than this: If H were to correctly identify the cause of E, it would correctly identify the cause of E. But this empty tautology does not logically imply that there is any reason to believe that H actually does specify the cause of E. The tautology gives us no reason to believe that we have identified the actual cause of E.

Of course, the rule of IBE relies on another premise: that in a set of potential explanations, H is more likely to be true than the others in the set. Yet this premise plus the tautological one do not imply that there is any evidence at all that H is true. All we can infer is that the other hypotheses are even less likely to be true than H.

This is easy to see where only one hypothesis, H, has so far been proposed to explain an event. Since H is the only proposed explanation, there is no competitor that is of equal or greater likelihood. Yet meeting this sufficient condition for applying IBE and adding the tautology that

if the hypothesis were to correctly identify the cause of E, then it would correctly identify the cause of E, provides no reason at all for thinking that H is true. Add some competitors to H that are less likely than H and still no evidence has been provided for H. Saying that the competitors are less likely does not imply that the winner is likely; we have only comparative warrant when we say that, compared to its inferior competitors, H is more likely than them.

The upshot of all of this is that if "explains" is taken literally, then IBE is of no use. We would need evidence that H is true before we apply the rule; and if "explains" is interpreted to mean "would explain if only it were true", then the fact that H "explains E" in this potential sense better than its known competitors is never evidence that H is true.

Hermeneutical evidence

There was a time when many hermeneutically oriented supporters of psychoanalysis endorsed a causal replacement thesis (Ricoeur, 1970; Schafer, 1976). Causal explanations of such phenomena as dreams, human action and psychological symptoms can be replaced, it was argued, by meaning explanations; this replacement thesis has been criticised by Davidson (1963), Grünbaum (1984), and (Erwin, 1996)—few defend it today. A more widely supported view among hermeneutically minded philosophers and psychoanalysts (Hopkins, 1991; Lacewing, 2012a; Levy, 1988) might be called the "indicator theory". It does not deny that psychoanalytic hypotheses are often causal, but it holds that meaning connections, sometimes called "thematic affinities", are often indicators of causal connections and so provide evidence for causal hypotheses.

Those holding this indicator view are clearly defending a novel basic form of evidence. Where others argue that psychotherapy causal claims generally require experimental evidence for their confirmation, defenders of the indicator view argue that meaning connections all by themselves can often warrant psychological causal hypotheses. No experimental evidence or empirical evidence of any kind is needed in such cases. James Hopkins (1991) explains the indicator view as follows. Suppose that a therapist says that the client's desire has a certain meaning. We easily understand what is meant by saying that individual words or whole sentences have a meaning, but how can a desire have a meaning? Hopkins explains this in terms of the sentence that

articulates the desire. Suppose that the client desires that his daughter will stop using drugs. The content of his desire is given by the sentence "His daughter will stop using drugs."

Assume that a man believes that Freud worked in Vienna. The content of the belief is captured in the sentence "Freud worked in Vienna". If the man hopes that Freud worked in Vienna, the content of the hope will be expressed by the same sentence, namely, "Freud worked in Vienna". Hopkins's next step is to appeal to Tarski's schema to provide truth conditions, or what he sometimes calls "satisfaction conditions", for sentences describing psychological states. According to Tarski, we can provide truth conditions for the sentence "snow is white is true" merely by writing: snow is white. The advantage of doing this is that we can provide truth conditions for any sentence without using unexplained notions such as "fact".

In applying Tarski's idea to psychological sentences, Hopkins says that the truth (or satisfaction) condition for "John believes that snow is white" is "snow is white"; for "John hopes that snow is white", it is again "snow is white", and so on for wishes and desires. This shows, Hopkins claims, that there is a logical or conceptual connection between a motive and its truth (satisfaction) condition. It is a norm or rule of language that having a drink of water satisfies a desire to drink water. Once we grasp this conceptual connection we know a central causal feature of the desire or other motive—that is, what it is *supposed to do*. The desire is *supposed* to cause the agent to get a drink of water. We can know this a priori by reflecting on the meaning of desires, hope, beliefs, etc. No empirical evidence is needed.

This argument goes wrong from the start. "Snow is white" is a truth condition for "it is true that snow is white", but it is not a truth condition for "John believes that snow is white" or "John desires that snow be white". John can believe that snow is not white and also desire that it not be white, but neither logically guarantees that snow is not white. Snow can also be white even if John does not believe it is.

There is also no logical or conceptual connection between having a desire to drink water and drinking water. Even if we could figure out just by reflection what a desire to drink water is "supposed to do"—I am not sure what Hopkins means by this—we would still need empirical evidence to show that if John drank a glass of water he was caused to do so by a desire to drink water rather than, for example, a desire to please his physician, or a desire to test the water, and so on. In general,

noticing a thematic connection between a desire and an action is not evidence by itself that the desire caused the action or made any difference at all to its occurrence.

Hopkins, like others, also tries to make his case for the per se evidential value of meaning connections by appealing to examples. But the examples he and others describe always fall into one of two classes: cases where there is evidence of causation but the evidence is supplied by empirical observation, or cases where there is no empirical backing but also no evidence at all (Erwin, 1996, pp. 35–38).

Michael Lacewing (2012a) has recently tried to defend Hopkins' claim that there is a logical or conceptual connection between a motive and behaviour of a certain sort. He claims that we cannot answer the question "what is a desire to drink" without referring to the behaviour to which it is causally relevant. If this were true, we could know a priori just by reflecting on the meaning of "a desire to drink" the behaviour to which this desire is causally relevant.

What Lacewing assumes and must assume for his argument to work is not true (Erwin, 2012). To have a desire to do something is to want to do it; to have a desire to drink is to want to drink. No behaviour need be mentioned. Even though it is highly unlikely, it is logically and conceptually possible that having a desire to drink is never causally relevant to drinking or any other behaviour. It is not a contradiction to say that people often have a desire to drink but the desire never affects their behaviour. When they drink, the drinking is always caused by conditioning without the causal intervention of any psychological state. Wrong perhaps, but not self-contradictory.

Lacewing replies (2012b) that my explanation of what is a desire to drink does not constitute a philosophical theory of desire. This is right, but why assume that a philosophical theory of desire is needed to answer the question: what is a desire to drink? He gives no reason to accept this assumption. Lacewing further replies "I can think of no (widely accepted) philosophical theory about the nature and content of desire that would not reject this possibility"—that is, the logical possibility that having a desire to drink never affects behaviour. If, contrary to fact, all widely accepted philosophical theories about the nature of desires did deny this possibility, they would all be false. To repeat the earlier point: there is no conceptual or logical contradiction in saying that drinking is always caused by external events such as the agent's history of reinforcements and punishments, and current environmental

stimuli, in brief, saying that desires never affect behaviour. Despite what Lacewing claims, however, not all philosophical analyses of the concept of desire rule out the possibility of desires having no effects on behaviour.

One of the theories he mentions—that a desire is a disposition to act—allows for this possibility. There is no logical contradiction in saying that humans typically have a disposition to do something, including a disposition to drink, but they never act on that disposition. Lacewing is wrong, then, to conclude that if a dispositional account were true, it would license a priori inferences concerning the causal role of desires. On a dispositional account, we need empirical evidence to know whether operant conditioning theories of drinking behaviour are wrong and empirical evidence to know that humans sometimes act on their dispositions to drink water, liquor, or anything else.

One might try to defend the evidential role of meaning connections without treating them as basic evidence. Donald Levy originally held that to speak of an affinity of any kind, including a thematic affinity, generally signifies *by itself* a causal connection between them (1988, p. 212). In his 2001 paper, however, he advocates the weaker position that meaning connections can be evidence of a causal connection *if* combined with other evidence, which, of course, is true, but is quite trivial.

Stipulate that we have evidence for (1) there is a thematic affinity between Anna O observing a dog drink water and her hydrophobia. Now, suppose we also had evidence for the conditional proposition "If (1) is true, then Anna's observing the dog drink caused her hydrophobia". In fact, however, there is no evidence for the conditional or for any other proposition that would get us from (1) to the causal conclusion. Virtually any X, including the free associating of a psychotic or the guessing of a patient about his own condition, can be evidence of a causal connection, *assuming* that we have evidence for the conditional that if X is observed then there is a causal connection of a certain type. This fact is of no theoretical or practical interest. We rarely, if ever, have evidence for the conditional.

The attempt to defend the indicator theory of meaning connections, like the earlier attempt to defend the replacement thesis, has come to nothing. None of its defenders has been able to show that thematic affinities are ever evidence of causal connections (Erwin, 1996, Chapter One; 2010; Grünbaum, 1998, 1990).

Clinical expertise and testimony

After a sustained debate among psychotherapists about what counts as evidence, the American Psychological Association appointed a presidential task force to examine the issues. The task force then issued a report concluding that in addition to certain recognised types of empirical evidence, there are two other types of evidence relevant to outcome research: clinical opinion and consensus among recognised experts (American Psychological Association, 2006, p. 1054):

> Types of research evidence with regard to intervention research in ascending order as to their contribution to conclusions about efficacy include 'clinical opinion, observation, and consensus among recognized experts representing the range of use in the field' (Criterion 2.1); 'systematized clinical observation' (Criterion 2.2); and 'sophisticated empirical methodologies, including quasi experiments and randomized controlled experiments or their logical equivalents' (Criterion 2.3).

Was the committee right? There is a simple way of dealing with the issue of whether the testimony of clinical experts constitutes evidence: where there is empirical backing for the belief that the testimony is likely to be right, treat it as evidence; otherwise don't. If the issue were this simple, however, there would probably not be so much disagreement about it. Two complicating factors that fuel the controversy are: the task force definition of "clinical expertise" and recent philosophical work on testimony.

A single clinical expert or a group of experts, quite obviously, could offer an opinion about, say, the efficacy of cognitive therapy in treating clinical depression and yet have no basis for the opinion. We might deal with this problem, however, by building into the definition of "clinical expertise" features that logically guarantee the likelihood of truth of expert clinical testimony. The APA report does not quite do this but it goes far in this direction. Those with clinical expertise, we are told, (a) recognise meaningful patterns and disregard irrelevant information; (b) organise their extensive knowledge in ways that reflect a deep understanding of their domains; (c) possess scientific expertise; (d) have competencies that promote positive therapeutic outcomes, including competency in assessment, diagnostic judgement,

treatment implementation, and (e) have a cogent rationale for clinical strategies; and (f) possess many other favourable psychotherapeutic characteristics.

Even with this broad definition there is a logical gap between the clinical testimony of an expert and evidence that the testimony is correct. Still, if we were to add a few more features to our definition, we could close that gap. Even as the definition is now we would have reason to believe that if X has clinical expertise, then X has a cogent rationale for clinical strategies, possesses competencies that promote positive therapeutic outcomes, and recognises meaningful patterns.

In giving this expansive definition, however, we are moving further away from the idea of expert clinical opinion as a distinct kind of evidence. The most we would have at our disposal is a conditional proposition: *if* X has clinical expertise, then X has characteristics a, b, c, d, e, and so forth. But to complete the argument that X's clinical opinions constitute evidence, we would need empirical evidence that X has these features. In addition, we would need empirical evidence correlating the possession of the characteristics with reliable clinical opinion.

In brief, if we stipulate that what we mean by "clinical expert" is someone who has characteristics that make it probable that his or her clinical judgements have evidential support, we would need empirical evidence for any therapist believed to be an expert that he or she has the required characteristics, and we would also need empirical evidence that possessing the features signals the likelihood of truth. In relying on such a definition we are thereby transforming evidence based on the opinions of clinical experts into at best derived evidence.

Sometimes, of course, we do have evidence that if an expert asserts something about a particular subject, then the expert is likely to be right, but this is not a new kind of evidence. It is just plain old empirical evidence, and it is not what the authors of the APA report (2006) seem to have had in mind. When they speak of clinical opinion and consensus among recognised experts, they do not say: the expression of the consensus is evidence provided that these opinions are backed by empirical evidence (which would make their claim trivial). They depict these expert opinions as a distinct type of research evidence.

Recent philosophical work on testimony, however, might seem to support the idea that testimony is a distinct kind of basic evidence. In a seminal paper on the subject, Tyler Burge (1993, p. 472; 1997) argues for his acceptance principle: "We are *prima facie* entitled to accept something as true if we seem to understand it and it is presented as

true by another person". I assert something; you seem to understand what I say; you are thereby *prima facie* entitled to believe me. The presumption of truth may be very weak, depending on the case, and in any event, it is only *prima facie*. It can be overridden by contrary empirical evidence.

On Burge's theory, the justification for this "acceptance principle" is not statistical evidence that most people when they speak sincerely and intelligibly speak the truth. Instead, Burge claims, we have an a priori justification for accepting the word of others under the specified conditions. Testimony in and of itself, then, gives us entitlement to believe. The entitlement need not be backed by any empirical evidence.

Before going too far down this entitlement road, however, it should be noticed that on Burge's view, it is not just the testimony of clinical experts that we are entitled to believe when discussing psychotherapy issues. The testimony of the untutored may have the same epistemic status as that of the expert.

There is no point, however, in travelling very far down this road if we are searching for a different form of basic evidence. We will not find any here. As Burge uses the concept of entitlement, if you are entitled to believe something, you have an "epistemic right" to believe it (1993, p. 458). But one can have an epistemic right to believe something without having any evidence that it is true, just as on some philosophic theories of justification, if one does one's "epistemic duty"—making an honest search for evidence, being willing to change one's mind when the facts dictate it, etc.—one is justified whether or not one's belief is supported by evidence. Entitlement does not equate with evidence.

Suppose that we depart from Burge's usage and stipulate that being "entitled to believe" logically entails that one has evidence for one's belief. In that case, the acceptance principle is wrong. The mere fact that someone tells me something is not by itself evidence of truth, not even prima facie evidence.

Burge does offer an a priori argument for his acceptance principle and this has to be considered: "… Intelligible propositional expressions presuppose rational abilities and entitlements; so intelligible presentations-as-true come prima facie backed by a rational source or resource for reason; and both the content of intelligible propositional presentations-as-true and the prima facie rationality of their source indicate a prima facie source of truth" (p. 472).

If we interpret Burge's conclusion in terms of evidence, however, then his conclusion does not follow from his premises. Concede to Burge that if

someone says something intelligible this presupposes a "rational source" with "rational abilities". The source of the pronouncement is rational at least to the degree that he or she has the capacity to say something understandable. Yet it clearly does not follow that this "rationality of the source" is evidence for the truth of what the speaker says. The speaker can be rational enough to say something intelligible without our presupposing that the pronouncement is backed by evidence. So, if "prima facie source of truth" is taken to mean prima facie *evidence* of truth, the argument is invalid. If, however, "entitlement to believe a proposition" entails nothing about evidence, then Burge's acceptance principle, even if true, tells us nothing about a distinct kind of evidence.

Although Burge relies on an a priori argument, others rely on an empirical argument that runs like this. Many of our evidence-based beliefs are based on testimony. For example, I ask someone where the 39 bus stops. She directs me to the northwest corner and I believe her. I am asked why I am carrying an umbrella and I reply that the weatherman just reported that it will rain this afternoon. I am asked why I believe such and such and I reply: "Jones told me". According to C. A. Coady, this response can be just as appropriate as saying "I saw it", or "I remember it" (Coady, 1992, p. 6). Testimony, like perception, is a form of evidence per se.

We could say that we are justified in relying on testimony when and only when we have specific evidence for the testimony's reliability. The problem with this theory, which Coady call the "reductionist" view, is that too many of our beliefs that appear well founded would be without foundation if the theory were true; for only in unusual cases do we have the required specific evidence about an individual speaker's reliability. So, the argument concludes, the evidence that testimony provides is not derived evidence; it is rather evidence per se, although, like direct perception, it is evidence only under certain conditions, such that there be no evidence against what is reported and no evidence for the speaker's unreliability (Coady, 1992, pp. 144–145).

What is plausible in the above argument is the assumption that many of our testimony-based beliefs are well supported even without specific evidence of the speaker's reliability. Yet the choice is not between: testimony provides evidence per se or the listener generally has specific evidence of the speaker's reliability. A third option is that we have background evidence that very often takes the place of specific evidence of reliability.

Jonathan Adler does an excellent job of providing the details (Adler, 2002) about such evidence. As Adler points out, if I hear the weatherman say that it will be sunny today, even if I have no specific evidence about this particular weatherman's reliability, I can soon check out the accuracy of the prediction. Once I do this, I can henceforth use not only the information from this one case but also the information gathered by many people about the accuracy of weather predictions. If a scientist reports in a medical journal article the results of his or her experiment, in the absence of direct evidence about the scientist's trustworthiness, the choice is not between remaining agnostic about whether the report is accurate or treating the report as evidence per se. The third and more plausible choice is to rely on many bits of empirical evidence about the general reliability of such reports, including evidence about the reward and punishment systems in science, the motivations of scientists who publish their research findings, and the reliability of past reports where the reports have been authenticated. Even in the case of a complete stranger who tells me where the bus stop is, most of us have an extensive source of evidence about what has happened not only in our own experience but in the experience of many others who have asked for directions to the bus stop.

Coady dismisses reliance on the experience of others as question-begging, since we are now relying on their testimony about their experience. There is nothing question-begging, however, about doing this. We are not trying to prove that testimony is reliable by assuming that it is generally reliable. Rather, we are trying to show that in particular cases, if testimony counts as evidence there are various sorts of background evidence to use as backup. I find this weatherman to be reliable on several occasions, but on other occasions, so does my wife, as do my friends. Do I have evidence that my wife and friends are reliable, not necessarily in general, but on what they tend to say about a weatherman? I may well have such evidence. There are, of course, also many sorts of cases where people rely on testimony—of preachers, of governments, of individual politicians—where they lack the background evidence to trust the source, but in these sorts of case, testimony is not evidence.

Even if the case could be made for treating testimony as evidence per se in some cases, something I deny, there would be another obstacle to overcome if we were to use this result to support the APA position on outcome evidence. In simple cases where a stranger gives directions or the parking attendant tells us that our car is parked on the second level,

the speaker is in a position to know the truth of what is asserted. It would be a leap to conclude that because testimony is evidence per se in these sorts of cases, it is evidence per se when a speaker makes a causal judgement without being in a position to know that the judgement is true or even supported by any evidence at all.

A psychoanalyst may be an expert on the type of therapy she uses, but she is in no position to conclude that her treatment is effective if all she has to go on is her observation that in most of her cases a patient improves soon after completing treatment. She is in no position to infer this conclusion if she cannot rule out both a spontaneous remission and placebo explanation of the treatment, which she generally cannot do without using experimental controls.

What is true of the individual expert is true of groups of clinical experts: unless they are in a position to know or at least to have some evidential support for their causal claims, their testimony that a treatment causes remission of symptoms would not be evidence per se for their claims even if, contrary to fact, it were true that testimony *sometimes* counted as evidence per se.

Clinical opinion and consensus among recognised experts, then, may sometimes give us reason to believe a certain proposition about effectiveness—but only if the opinion of the individual or the group of recognised experts is backed by proper empirical evidence. Expert clinical testimony is not evidence per se, nor is it a distinct kind of evidence, contrary to the APA task force report, and contrary to those who treat expert testimony as a separate kind of research evidence.

Conclusion: I have not attempted to show that empirical evidence is the only kind of basic evidence. Intuition, for example, when used in logic or mathematics may also qualify. What I have argued is that certain types of proposed forms of evidence per se that have played an important role in discussions of psychoanalytic evidence, especially in attempts to meet the demand for experimental evidence (Erwin, 1996; 2006; Grünbaum, 1984), are not basic. The evidence one gets when using IBE is not evidence per se; or, alternatively, the rule is simply invalid. Pragmatic factors, such as simplicity, can sometimes be tiebreakers when the empirical evidence is neutral between two theories, but this is only because background empirical evidence makes it likely that in a certain domain the simpler is more likely to be true. Meaning connections are never evidence per se, but, of course, they can count as derived evidence if they have empirical backing. The same is true of expert testimony.

CHAPTER THREE

Critique of Grünbaum's "Critique of psychoanalysis"

Linda A. W. Brakel

Before reviewing some of Professor Adolf Grünbaum's claims in detail, there are two overarching problems with his account I would like to set forth:

1. Like almost everyone else, Grünbaum takes psychoanalytic clinical theory to be the core of psychoanalysis. There are many advantages in holding a different view—namely that the psychoanalytic general theory of mind constitutes the core of psychoanalytic theory. With this alternate view in place, many of the problems Grünbaum raises are largely obviated. Because the arguments for the general theory and its five presuppositions are given in great detail in Chapter Six, Part One (and see also Brakel, 2009), I will rehearse them here in the briefest possible manner and only when they bear directly on my critique of Grünbaum's critique. There will, however, be several places in which a contrast between these two views of psychoanalysis' constituent theoretical core can prove quite revealing.
2. Grünbaum takes particular statements of Freud's about psychoanalytic clinical theory as though these characterisations actually fixedly define and determine psychoanalysis. To the extent psychoanalysis is and can remain a viable clinical theory, both analytic

> clinicians and theoreticians must be able to allow for modifications in the initial conceptualisation Freud provided—expanding some aspects, retracting others, all while retaining that which is essential. Grünbaum allows no such flexibility. As such, many of his arguments have a straw man opponent, vitiating the overall importance of his critique.

Now for specifics:

Assuming that all reading this critical analysis of Grünbaum's "Critique of psychoanalysis" will have read Grünbaum's chapter in this volume, I will proceed to discuss the details of the sections on which I will comment according to his organisational plan. (Note that all Grünbaum references are to the work within this volume.) Let me begin, then, with:

Grünbaum's section II: logical relations of the "dynamic" and "cognitive" unconscious

Although it is the case that many clinical psychoanalysts as well as many cognitive psychologists *want* a clear separation between "the dynamic unconscious" and the "cognitive or adaptive unconscious," wanting does not make it so. Psychoanalysts object to seemingly relevant subliminal research findings on the basis that the so-called "real" dynamic, repressed conflictual unconscious is not being tapped (Boesky, personal communication, 1997). Academic psychologists either struggle to differentiate their views on the unconscious from the now academically toxic "Freudian unconscious" (e.g., Wilson's 2002 "adaptive unconscious"), or ignore the clear Freudian analogue to their accounts. (See for instance, Wegner, 2002; and especially Kahneman, 2011, in which Freud is not referenced at all!)

But where is the evidence that these are separate domains? In Grünbaum's claim that psychoanalysts should find no confirmation (but perhaps disconfirmation) for their views in cognitive science findings regarding non-conscious mentation, he has apparently accepted the rather implausible notion that there are actually radically separate instantiations of the unconscious. Can he mean that ontologically—that is, biologically in the brain there are separate domains? He cannot be unaware that a great amount of recent research on brain plasticity argues against that view. (A summary of some of

this work can be found in Brakel, 2013.) This leaves the only other possibility: that Grünbaum predicates separate domains conceptually; not based on biological findings but instead psychological considerations. But it turns out that this is not Grünbaum's view either. Actually, he does not allow for any type of *dynamic* unconscious at all. Rather, as it becomes clear in this section, he dismisses the very concept of a dynamic unconscious, begging an aspect of the very questions he's attempting to address—all of this without acknowledged argument.

A different issue emerges toward the end of this section. Grünbaum (p. 6, this volume) states that there are associative problem-solving processes in the cognitive unconscious. This seems right; and is indeed largely uncontested, and properly so. However, Grünbaum contrasts these associative processes with the wish-driven "highly illogical" Freudian dynamic unconscious. But do these operational differences argue for separate unconscious domains? Not at all. It seems just as plausible, indeed much more plausible, that unconscious/non-conscious mental processes, states, and attitudes—biological processes all—can, like most biological processes, serve multiple functions. With this in mind, I have offered (Brakel, 2009, 2010) a more parsimonious solution to the "cognitive/adaptive *vs*. dynamic unconscious problem," holding that primary process mentation, largely associative and a-rational in character, predominates in unconscious/non-conscious mentation; and that it is *appropriated* either toward adaptive problem-solving—and in this sense seemingly "rational" ends—or toward irrational psychological symptoms.

There are salutatory implications if I am right in this part of my critique of Grünbaum's critique. Namely, contra Grünbaum's position, psychoanalysts can indeed feel heartened by taking "evidence for the cognitive unconscious … as … also furnish[ing] support for the dynamic unconscious" (Grünbaum, p. 7, this volume).

Grünbaum's section III: psychoanalytic theory as a staple of Western culture

This section first narrowly defines Freudian slips and other parapraxes in an idiosyncratic way; then declares that many slips apparently well explained by analytic theory are merely pseudo-Freudian; and finally argues that "real" Freudian slips meeting Grünbaum's criteria may

not exist at all. All of Grünbaum's argumentation here (and then the even more detailed and complicated work to follow) seems to rest on Freud's own words (1901b, p. 239 quoted by Grünbaum, p. 8, this volume), declaring that slips of the tongue and parapraxes are such that "we must not be aware in ourselves of any motive for it." But what is the trouble here? In the very example of a pseudo-slip Grünbaum offers (p. 7, this volume): the bosom-tracking man who wants a "breast of flesh air"—the man was almost certainly not aware of his motivation toward pursuing the breast. He remained unaware *until* he heard his own utterance, at which point the unconscious motivation became immediately conscious and transparent to all. Even if he accepted this point, Grünbaum would still hold that examples like this do not fit the Grünbaum view of Freud's requirement for unconscious causes of slips, because, according to Grünbaum, "true" cases require that the psychological motive be repressed, owing to "motives of unpleasure." But again I must ask, even considering Grünbaum's own example of the breast-seeker, how could this case not fit the Freudian requirement? The breast- seeking man's motive to "get a woman's breast," if occurring in a public place and in the context of a non-intimate relationship, is likely unacceptable to him, as such would be unpleasurable, and therefore on that ground occasion repression.

Moreover, Grünbaum is wrong on another count here: Freudian theory does not require that it be one's *motive* that undergoes repression in a slip of the tongue, or parapraxis. Rather, in order to deal with "motives of unpleasure" any one of a number of components of a psychological act, including but not limited to the motive, can be repressed or transformed. The repressive process, often in concert with other psychological defenses, contributes to the formation of symptoms, parapraxes, etc., all in order to avoid psychological unpleasure. To demonstrate, let's imagine a different version of our breast-admiring man: this man is not daunted by, and in fact is well aware of, his desire to "get her breast". But this man fears rejection, especially by desirable women, and he is consciously aware of this too. Trying to win her sympathies, but simultaneously to appear gallant (two conscious strategies employed to prevent rejection), he says he is feeling faint from the heat, then suggests that both he and she would be better off outside. Leading the way, he crashes into the glass sliding door, falling to the floor. She bends over him, concerned and attentive, her breasts very near his face. But this is certainly not the way he consciously intended to "get her breast".

What were the psychological elements repressed and unconscious in this parapraxis? Certainly not the content of his motives; but perhaps what was repressed was the *intensity* of his desire. In addition, the visual perception of the door was undoubtedly outside of his conscious awareness.

Turning now briefly to the last part of this section, I must strongly disagree with Grünbaum's argument against Thomas Nagel's view that psychoanalysis is an extension[1] of ordinary common sense. With the general psychoanalytic theory of mind and its five presuppositions in place (as proposed in Brakel, 2009 and in Chapter Six, Part One), not only will psychoanalysis fit as a regular science, but this portion of Nagel's notion of psychoanalysis can be rescued and restored. The general psychoanalytic presuppositions—instead of the much less fundamental, more derivative, and thereby contestable (sometimes dubious) clinical theories—allow *behaviour* that appears "bizarre" and leaves one "incredulous"[2] to be understood as in fact regular, lawful, and in this sense, ordinary. (See Brakel, this volume, Chapter Six, Part One, for much more detail on this matter.)

Grünbaum's section IV: critique of Freudian and post-Freudian psychoanalysis

In the introduction to this section, Grünbaum (p. 12, this volume) identifies three major theses considered to be at the "… 'cornerstone' of Freud's (1915d) theoretical edifice …" These are that: 1) repression of unpleasurable psychic states takes place; 2) repressed material is causally active in the production of the neurosis; and 3) free association can not only identify, but also lift the pathogenic repressed content, and in this way prove therapeutic. Grünbaum goes on to argue against each of these three.

First (in IV A) Grünbaum attempts to undermine the concept of repression. He presents a long section on forgetting, which concludes with the obvious point that many distressing experiences are remembered, not forgotten; a point that Grünbaum believes damaging to Freud's notion of repression. Instead this is an illustration of Grünbaum's rather constricted (and straw man) view of psychoanalytic clinical theory. Unacceptable and painful content indeed can be repressed and forgotten. But repression of content is just one of the psychological mechanisms of defence Freud proposed. For example, a distressing experience can

be hyper-remembered or emphasised in a so-called "screen memory" in order to repress and disguise similar but even more unacceptable impulses or events. Or, the affect involved with various unpleasurable contents can be that which is repressed. In these cases the content is remembered in great detail but the affect blunted, absent, altered, or even reversed. Other defences involve displacement, externalisation, and projection, in which the agency and ownership of that which is painful and/or unacceptable is repressed and changed. What is common in all of these defensive manoeuvres is that something critical is rendered unconscious. And this leads to the next stage in Grünbaum's attempts to dismantle psychoanalytic theory—Grünbaum's critique of the causative role of that which is repressed.

The idea that repressed contents are causally active is one of Freud's basic tenets. On this Grünbaum and I agree. In my account (in Chapter Six, Part One) the inference of an extant causally active, meaningful dynamic unconscious is perhaps the most basic of the presuppositions of the psychoanalytic general theory. This of course Grünbaum cannot countenance. To understand his view, we must first appreciate that the Grünbaum attack on the notion of a causally active dynamic unconscious is inextricably linked to his third thesis above regarding the therapeutic success of free association. (See Grünbaum, p. 12, this volume, in the introduction to section IV.) And, there is much to say about this matter. But before I do, I must take up a prior pressing issue: there is empirical work providing evidence for unconscious conflicts and their causal efficacy; work of which Grünbaum has been apprised. I will first briefly present the relevant experiments, and then report on Grünbaum's recent (2012) endorsement of these studies as indeed providing the evidence claimed.

In a complex investigation by Shevrin et al. (1992), then expanded and reported in greater detail (1996), four psychoanalysts interviewed eleven participants with social phobia. The analysts picked out each participant's central unconscious conflict. Next, for each subject, the analysts a) selected a number of words they could agree upon as representing each unique core unconscious conflict, and b) chose an equal number of words to reflect each subject's conscious symptom. These two categories of individualised, highly personal words, along with two categories of general non-individualised words—Osgood (1975) pleasant and unpleasant words—were presented both subliminally (at 1 millisecond) and supraliminally (30–40 milliseconds) to the eleven

clinical subjects while measures of brainwave activity, evoked response potentials (ERPs), were collected.

The salient findings of this study for our purposes here are: 1) time-frequency information analysis of the ERP brain responses demonstrated that the words in the unconscious conflict category were grouped together *only* during subliminal presentation, whereas the conscious symptom words formed a better group when presented supraliminally; and 2) the subjects, when asked after the brainwave collection and in full awake consciousness "to organise the words", placed the conscious symptom words in one category, whereas the unconscious symptom words were placed in several categories. For none of the subjects did the unconscious conflict words cohere as a single category.

Grünbaum, with whom Shevrin communicated shortly after the full publication of this study, according to Shevrin (personal communication) and reported in *Science Daily* (June 16, 2012, www.sciencedaily.com) "… agreed that [in this experiment] Shevrin [et al.] had obtained objective brain-based evidence for the existence of unconscious conflict." However, as Shevrin continued in his report, Grünbaum was not satisfied that a causal role for these unconscious conflicts in producing psychiatric symptoms had been established.

Hence, in order to provide evidence for the causal role of unconscious conflicts, we constructed an experiment quite similar to the one described just above, but with a few important differences. We again employed a within-subjects design (in which every subject experiences every condition), but this time we used different categories of primes and targets designed specifically in order to test the possibility of a causal relationship between unconscious conflicts and conscious symptoms. Here is a condensed description of the study: Shevrin and colleagues (2013) gathered ERP brainwaves of eleven new phobic participants as *their* unique unconscious conflict words were presented as primes—subliminally in the experimental condition and supraliminally in the control condition—then followed immediately by the presentation of two categories of supraliminal targets—personalised conscious symptom experimental words and the control Osgood unpleasant words. To make this clearer, ERPs were collected on each participant under the following four conditions presented:

1. Subliminal unconscious conflict word primes followed by supraliminal conscious symptom word targets.

2. Subliminal unconscious conflict word primes followed by Osgood unpleasant word targets.
3. Supraliminal unconscious conflict word primes followed by supraliminal conscious symptom word targets.
4. Supraliminal unconscious conflict word primes followed by Osgood unpleasant word targets.

After their collection, the analysis of these ERPs was performed. On the ERPs from each subject, the measure of interest was his or her brainwaves' alpha frequency (8–13 cycles per second)—a frequency associated with cognitive inhibition. This was analysed in all four conditions for every participant. Since we reasoned that a causal relationship could be demonstrated if the unconscious conflict words produced a differential inhibitory effect on the conscious symptom words, we hypothesised that that would be the outcome.

And the following results did emerge: quoting Shevrin again in *Science Daily* (June 16, 2012) "Highly significant correlations, suggesting an inhibitory effect, were obtained as increased amounts of alpha generated by the unconscious conflict stimuli predicted increased amounts of alpha associated with the conscious symptom …" But, Shevrin further noted, this inhibition was observed *only* when the unconscious conflict words were presented as subliminal primes, not when these same words were presented as supraliminal primes. A further control solidified the findings: no results were obtained with either subliminal or supraliminal unconscious conflict word primes when the targets were the Osgood unpleasant words. In other words, the hypothesised result was obtained.

Concerning this experiment and its attempts to gain empirical evidence for causally active unconscious conflicts, Shevrin reports (*Science Daily*, June 16, 2012, and personal communication) that Grünbaum sent him an email stating, "I am satisfied".

And yet if Grünbaum holds to the views expressed in the current volume, he cannot really be satisfied regarding the causal powers of unconscious conflicts, his recent seeming acceptance of empirical evidence for such causation notwithstanding. Partly this may be due to the unfortunate fact that Grünbaum's critique of unconscious causation herein is far from direct. It is instead linked in a complicated fashion to the lifting of repression and the removal of symptoms. Since,

according to Grünbaum's understanding of psychoanalysis, these are both claimed to be effects of free association (this amounting to his third thesis referred to above), I must soon turn to his discussion of free association in section IV B.

But first, before we even get to free association, I think it is important to reveal a subtle but fallacious background argument Grünbaum is attempting. Namely, his account implies that if lifting repression does not affect a cure, then it has been demonstrated that unconscious contents and conflicts do not have causal powers. This is clearly wrong, as can easily be shown with a medical example. Eradicating beta hemolytic strep bacteria—the essential cause of rheumatic valvular heart disease—will do nothing to improve this cardiac disease once it has been established. Thus, the fact that removing the cause of an ailment does not result in a cure in no way establishes that the causative powers were wrongly assigned.

Interestingly, Grünbaum (p. 12, 17, this volume) maintains that it is Breuer and Freud (1895) who advanced this particular type of faulty reasoning in *their* contention that if a symptom persists so must its cause. In fact, Grünbaum implies that this line of reasoning by Freud and Breuer is the very source of his argument (above) against them. But Grünbaum cannot be allowed to use this trick. Holding psychoanalysis to this nineteenth-century claim is another example of Grünbaum's tendency to define psychoanalysis rigidly and restrictively, and then to battle with the straw man he created. A more modern understanding of the economy of symptoms suggests that symptoms may indeed persist, even if its cause has been eradicated, as it is economical for the same symptom to now be fuelled by another and different causal unconscious conflict. An analogue with a pulmonary infection can serve to elucidate this further. Suppose a lung abscess is primarily caused by microbe A. Even if an antibiotic kills many of these initial causative agents, the infection can persist. How? With fewer populations of microbe A around, microbe B numbers can multiply, settle in, and perpetuate the infected abscess.

With this out of the way, let me proceed to discuss Grünbaum on free association. First, I believe that this section (IV B) can usefully be divided into two subsections—one centring on the capacity of the free associative method to reveal unconscious conflicts; and the other concerning the putative therapeutic value in this revelation in itself. With regard to this last, I think Grünbaum is totally right. There appears little

curative power in merely knowing what had been repressed. Practising psychoanalysts marvel at Freud's apparent optimism in holding that psychological problems will be relieved merely by helping patients know what had been un-known—that is, knowing repressed contents and unconscious processes. Rather, cure or even symptomatic improvement is a lengthy operation, one that probably is still not well understood. Knowing what had been unconscious and therefore enlarging the realm of active agential choice is a part of improvement—but just a part—albeit a necessary one, and likely not fully functional until near the end of an analysis. Other aspects are probably equally (or more) important. Recently, for instance, I have hypothesised that much psychopathology is predicated upon a complex of aversively conditioned behavioural and psychological responses associated with unconscious conflicts. As such, and since recent evidence suggests that aversive responses can never be made extinct, but that the best approximation is for many contexts of "safe-here" and "safe-now," I have proposed that psychoanalysis works by indeed providing, through multiple transference experiences, many and diverse instantiations of "safe-here" and "safe-now". (See Brakel, 2013.)

But the implications—even in this matter of our agreement about the highly questionable therapeutic value of uncovering unconscious causally active contents via free associations—are far different for Grünbaum and for me. Unlike Grünbaum, I do not think there are valid either/or questions regarding the therapeutic aspects of psychoanalysis. For me it is not the case that either insight is curative *or* it is the "non-insight ingredients" (Grünbaum p. 20, this volume) that cure. I along with any and every psychoanalyst will assert that both contribute, in variable ways, differently for different patients, and even for any particular patient at various stages in the treatment. Likewise for psychoanalysts, what Grünbaum (p. 20, this volume) terms "[t]he ominous hypothesis of placebo effect" should not be regarded as the least bit ominous. Placebo responses likely contribute some baseline therapeutic effect to every effective treatment,[3] much in the manner of an intact immune system providing the baseline for any antibiotic treatment. (Further, the placebo effect itself can be at least partly explained on the basis of psychoanalytic principles. See Brakel, 2010).

Returning now to look just at the free associative method's capacity to reveal, but not its putative curative effect—Grünbaum does not have much to say. He keeps the link with cure tightly in place and then

demolishes the curative notion, using a complicated, antiquated and rigidly defined version of clinical theory (pp. 17–18, this volume). Now on my account I need not defend either of Grünbaum's two supposedly *Freudian* clinical theory hypotheses—the "T. Therapeutic Hypothesis" or the "E. Etiologic Hypothesis" (pp. 17–18, this volume). Instead, I hold that there is a more direct solution to the seemingly intractable problems for psychoanalysis these hard-to-defend theses set up. It is a solution predicated on considering free association a method that can reveal previously unconscious causally active content, quite separate from any "cure".

In Brakel (2009) and then again in Chapter Six, (Part One), I proposed that there are five presuppositions that comprise the foundation of the psychoanalytic general theory of mind. Further, that the general theory of mind is the core psychoanalytic theory with the clinical theory (or theories) derivative thereof. As such, these derivative theories are not only much more variable, they are much less reliable, and *should* be subjected to the sort of criticism Grünbaum offers. But a critique of repressed sexuality with an original traumatic transference (Grünbaum p. 12, 17 this volume) is not a critique of a basic presupposition at the core of psychoanalytic theory. It is a critique of an amalgam of some older ideas that, while they were useful in establishing an original version of psychoanalytic clinical theory, are no longer operative. I promised not to rehearse the arguments I make in Chapter Six, (Part One) here. But let me very briefly restate my view that free association is unique among the presuppositions in that it reveals a break in the psychological continuity and psychological determination one presumes of one's fellow humans.[4] Also when free association is continued it can reveal the inferred unconscious content that is the cause of the gap. Thus the free associative process can restore continuity; but alas it cannot restore mental health.

Now I'm sure Grünbaum would not be happy with this account. Let me try to anticipate what he would contest. Judging from Grünbaum's (p. 19, this volume) less than favourable views on "inference to the best explanation" (Harman, 1965, and see Brakel, 2010, Chapter Six for a review), he might contend that there is no evidential warrant to count specific content revealed by free association as content constitutive of the causative unconscious conflict, no matter how consistent these data look. Grünbaum states (p. 19, this volume): "… a hypothesis [e.g., that some content 'X' is the unconscious causative

conflict] that is pseudo-confirmed by some data [and inference to the best explanation] cannot be warranted qua being 'the only [explanatory] [Grünbaum's brackets] game in town.'" But, because there is no claim that any particular content revealed is the only possible unconscious conflictual cause, this standard is one that the free associative method does not need to meet. In fact, just as any particular data set can often admit of more than one explanatory theory, the free associations and the content uncovered can lead to more than one possible unconscious conflict candidate.

Take, for example, a patient in analysis, who while freely associating distorts the words to a song in the following fashion. Whereas the lyrics are "My world is empty without you babe" the patient sings "My world is M.D. without you babe". His associations reveal a break in psychological continuity; he has sung the wrong words. Next he recognises the slip of the tongue and begins to associate to this parapraxis. The initials "M.D." bring to mind that his analyst is a doctor and the idea of an empty world reminds him that the analyst will soon be leaving on vacation. He continues that his father was a doctor and often paid little attention to him. And now he realises he has been quite angry at doctors, past and present, often without knowing either his anger or its dangerous intensity. These contents, uncovered by the free associations, lead to positing the following as figuring centrally in a causative unconscious conflict: he is angry at doctors. He cares about them and still they leave him. When they leave him he feels he's in an empty world. Yet he fears he is too angry.

But, now let me alter the history and current context of this patient for the purpose of illustration. Suppose his wife is eight months pregnant and that he's talked vaguely in the past of fears of obstetrical accidents. The same verbal slip, and the same associations, revealing the same contents could pertain to a different central unconscious conflict: namely, a fear that were something to go wrong with his wife's pregnancy, he'd be left alone—not only to deal with his anger at the doctors, but more importantly to cope with the emptiness of losing his "babe"—that is, his wife, his child, or both.

I will now take up sections IV C, D, and E together, compressing my critique of Grünbaum's critique. Let me start with the concept of the transference neurosis as presented in IV C (pp. 21–24, this volume). This can be salvaged from Grünbaum's criticism if one can first

unhook the simpler concept of transference from its earliest narrow definitional requirements of repeating an original pathological sexual episode. Note that Grünbaum holds the concept of transference hostage to this outdated description. Grünbaum could instead regard the phenomenon of transference more broadly as an attitude one feels in the present, profoundly influenced but "wrongly" based upon some similar past experience.[5] With this more modern (and more accurate) understanding, the transference neurosis concept can be shown to be a rather useful part of clinical theory; a gathering and centring of the various problematic transferences toward the person of the analyst for easier access to the "safe-here" "safe-now" multiple contexts of a psychoanalytic treatment.

This discussion of the transference neurosis does lead to a deeper problem made clear in sections IV D and IV E. Grünbaum's criticism focuses on the compromise-model of neurotic symptoms, now extended to manifest dreams and parapraxes. With full disclosure of my own clinical theory preferences, I own up to endorsing the compromise-model as set forth by Freud (1926d) and elaborated by Arlow and Brenner (1964) and Brenner (1982). This is a model that features ubiquitous compromise formations constituting mental events of all sorts. Thus, the theory allows compromises between conscious and unconscious processes; primary and secondary process mentation; various different transferences; and among ego and superego functions with id drives and impulses. Further, it is true that under the compromise formation model one can unify all sorts of mental goings-on, including dreams, phantasies, neurotic symptoms, parapraxes, and even job and mate choices. But this strength of the model also demonstrates its weakness as a theory. Once one posits that compromise formations are ubiquitous, you *can* find them in every mental product, including dreams, phantasies, neurotic symptoms, parapraxes, even job and mate choices.

So, why then do I find Grünbaum's critique problematic? It is because however useful the compromise formation model is as a concept of psychoanalytic clinical theory, it is a concept at the level of psychoanalytic clinical theory. In fact, it is but one version of clinical psychoanalysis. As such it is a category mistake to take it as a central tenet of psychoanalysis. Further, although the compromise-model does allow the sort of unification Grünbaum describes, psychoanalysis as a general theory of mind with its five presuppositions does so too, just as well, and maybe better for being more inclusive. Thus (and here again I

am condensing Chapter Six, this volume, Part One) if one posits that all psychological events, productions and contents are determined in the sense of being caused, and continuous in the sense of being potentially explained, then any break or gap in the seeming continuity allows one to posit unconscious meaningful (conflictual) mentation as the cause of the gap, the content of which, if known, would fill in the gap. Free association is the method that reveals the gappy areas and fills them in, with primary process mentation often playing a role both in constituting the gap and restoring continuity. There is no cure in restoring continuity. I can agree with Grünbaum that to promise cure is overreaching. But I must strongly take issue with Grünbaum's (p. 28, this volume) claim that Freud "… generated a pseudo-unification of neurotic behaviour with dreaming and the bungling of actions." Grünbaum continues (p. 28, this volume):

> This dubious unification was effected by conceiving of the *normal* activities of dreaming and occasionally bungling actions as *mini*-neurotic symptoms, of-a-piece with *abnormal* mentation in neurosis … To emphasize this monistic psychopathologizing of normalcy, Freud pointedly entitled his magnum opus on slips *The Psychopathology of Everyday Life*. (1901b)

Indeed I think Grünbaum has this exactly wrong. Freud's book on slips, and his book on dreams, and his many books on neurosis—all based on the foundations of the general psychoanalytic theory of mind—far from pathologising the normal, allow—actually insist on—normalising the psychopathological. The psychoanalytic general theory of mind, Freud's greatest contribution, provides a way to understand all human behaviour; the bizarre, troubled, and distorted, no less than the brave, joyous, and work-a-day.

CHAPTER FOUR

From scientific explanations to micropsychology: what should psychoanalytic theories be like?*

Vesa Talvitie

Currently nobody is able to single out the curative factors of psychotherapy, and the theories of all branches of psychotherapy are probably more or less misled. The state of the art is far from gratifying. We must nevertheless continue to study the "talking cure" in the context of science and humanities—individuals, governments and insurance companies still need criteria for choosing between different branches of therapies. It is reasonable to favour the branches whose effectiveness and background theories are studied in the scope of the academic community. It is of minor importance to dispute, for example, whether in psychoanalytic and cognitive therapies one is more scientific than the another—the crucial thing is to maintain the distinction between schools of therapy that take seriously the viewpoints of science and humanities and those that do not.

I study, below, psychoanalytic explanations in this spirit. True, nobody can pick out the curative factors of psychotherapy, and it is also

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true that the background theories of psychotherapy schools are very, very far from complete. To this extent we should not take those theories per se too seriously. However, it is appropriate and important to study whether it is at least *in principle* possible that disorders and curative factors of psychotherapy could be explained in the manner each school currently describes.

I will focus on three topics. First, the relation between psychological and neurobiological explanations; second, the nature of causes in general, and the causes of psychic disorders in particular (which turns out to be closely related to the Freud wars seen in the critique presented by Adolf Grünbaum); and third, the consequences of the fact that psychoanalysis is interested in idiosyncratic phenomena. At the end of the chapter I will study the notable implications these considerations bear on what we should think about the nature of psychoanalytic theories. We will begin, however, with the study of whether it is possible to reveal laws in the domains of psychoanalysis and other psychological disciplines.

Are there laws in the domain of psychology?

Could we claim that when considering a group of people possessing, say, unresolved oedipal conflict, all the members have had certain *particular* problems with early development (or, from different psychoanalytic perspectives, the members all share a certain restriction in the ability for mentalisation, or a certain particular repressed intrapsychic conflict)? Is the consequence of unresolved oedipal conflict *always* a certain psychic disorder, pattern of behaviour, or characteristic of personality? Even if just one of the above claims were justifiable, psychology could—similar to the natural sciences—present laws.

When claiming that A causes B in science, there are two basic routes to justify the claim. For the first, a scientist may argue (in the spirit originating from C. G. Hempel's covering law model) that the causal connection is due to a law of nature. For the second, he or she may try to show through statistical methods (in the "Bayesian spirit") that B follows A (see, for example, Rosenberg, 2000). Since Robert Cummins's *The Nature of Psychological Explanation* (Cummins, 1983), it has been thought that psychology cannot present (universal) causal laws (see, for example, Kim, 2010, pp. 282–319). Thus, Stanovich (2000) states: "Virtually all the facts and relationships that have been uncovered by

the science of psychology are stated in terms of probabilities" (p. 155). In order to illustrate the difficulty of pinpointing psychological laws, let us think of probably the most general and widely accepted statement in the domain of psychology—"Miller's law" concerning the "magical number seven". The "law" refers to George A. Miller's (1956) suggestion that the capacity of the working memory is 7+/–2 chunks. Note that although we deal here with foundational cognitive characteristics shared by the whole of humankind, researchers end up with an imprecise description ("+/–2"). Miller actually used the expression "magical number seven" in an ironic sense. All in all, from the viewpoint of natural science, talk about *law* is here a remarkable overstatement.

Let us consider how the most ordinary psychological phenomena appear from the perspective of laws ("When A, always B"). When something sad happens, does a human always become sad? No. When a human faces an insult, does he or she always feel hurt? No. When a neural tissue in, say, a finger, becomes damaged in ordinary conditions (when one is not, for example, under anaesthetic), does a human always feel pain? No. There seems to be no reason to doubt Cummins's claim—psychology is not a discipline of laws. Actually, according to the commonly accepted view (for example, Fodor, 1989; Kim, 2010, pp. 282–319) we cannot find laws from the domains of other special sciences either. (In philosophers' jargon the term "special science" refers to sciences that cannot present general laws or explanations. According to the strictest line of thought there is just one general science, physics.)

Jaegwon Kim holds that there are even no *biological* laws since "nonbiological physical events (exposure to high levels of radiation, the unavailability of necessary nutrients, ecological changes, natural disasters, and so on) can always intervene to break up biological causal processes" (Kim, 2010, p. 292). We might say that foundational physical processes are "stronger" than those studied by special sciences. According to Kim the multi-realisation of mental properties is another reason why there are no laws in the domain of psychology. To put it shortly, he holds that since all people are unique (similarly as "say, all samples of the 2006 Honda Accord LX Sedan" [Kim 2010, p. 301]), we cannot find laws without exceptions. Thus, should psychoanalysis also avoid sketching universal laws, and restrict itself to probabilities?—No universal Oedipus complex, no universal claims about developmental matters (in terms of, for example, stages of psychosexual development

and Kleinian positions), no general principles of mental functioning, no wish fulfilment behind every dream? In any case it is extremely unlikely that psychoanalysis could reveal an exceptionless law (and if it were to actually do so then that would create a sensation in the philosophy of science). Although many psychoanalytic thinkers would miss those strict Freudian principles, giving up the universal claims concerning the essence of humans is reasonable, since that makes the burden of the proof of theories considerably lighter.

On psychological and neurobiological explanations

When considering the essence of psychology and psychological explanations, two big names from the history of psychology have to be mentioned. First, Freud's teacher, the philosopher Franz Brentano, who published *Psychologie vom Empirischen Standpunkt* (*Psychology from an Empirical Standpoint*) in 1874, second, the cognitive scientist Robert Cummins, mentioned above, who wrote the classic *The Nature of Psychological Explanation* (1983). In some circles the rapid development of neuroscience has given rise to the idea that neurobiological explanations will make psychological explanations redundant. Thus, the rise of neuroscience and biological psychiatry has made the question concerning the essence of psychology topical, and in the domains of both the philosophy of psychiatry and the philosophy of psychology there is a vast literature on it (for example, Bermúdez, 2005; Kendler & Parnas, 2008; Schouten & Looren de Jong, 2007). Let us note here that neuropsychoanalytic writings lack, completely, references to this literature, which reflects the shallowness and non-reflective enthusiasm of that particular discipline.

One might think that psychology and neurobiology possess territories of their own, and as a consequence psychological phenomena were given psychological explanations, and neurobiological phenomena neurobiological explanations. The situation is not that simple, however. In some cases a neurobiological explanation is clearly the correct one for a psychological phenomenon—when one is given a certain kind of drug, he or she becomes hyperactive or calm, for example. Similarly, a psychological explanation is sometimes the one that we feel is the most plausible for a physiological phenomenon: perceiving or thinking of a frightening matter may accelerate one's heart rate, and psychological stress may affect levels of hormones.

When approaching the relation between psychological and neurobiological explanations it is important to be sensitive to how the phenomenon under scrutiny is described. If the phenomenon aimed at being explained is "why was Jane's hand raised?", there is no room for psychological explanation, and the explanation is to be given in terms of neuroscience—that happened because of certain neural activity in Jane's body and brain. If we describe the same phenomenon by indicating that Jane raised her hand in order to vote for Hank as president of the club, the situation is radically different: that description opens up a plethora of psychological issues. When studying Jane's act on the foundational level of cognitions (studied by cognitive science), we make the assumption that Jane supposedly knows what an election is, what a president is, and that the current social situation is an election, and so on.

Psychoanalytically interesting issues emerge if we ask why Jane voted for Hank instead of Waylon. Such a conceptualisation leads us to study why Jane is a member of the organisation in which the election takes place; what desires and fantasies she has about it; what desires and hopes does she possess towards Hank (and Waylon), and so on. From the psychological point of view it would be interesting if Jane had planned to vote for Waylon, and raised her hand by accident—what would be the explanation for that slip? The above considerations lead us to a view commonly accepted in the domain of the philosophy of science (for example, Godfrey-Smith, 2003; van Fraassen, 2008; Wimsatt, 2007): the appropriateness of an explanation depends on one's viewpoint and interests.

Inside the realm of psychology there are a considerable number of differing viewpoints and interests. Psychotherapists, and thus also the theories behind psychotherapy schools, focus on idiosyncratic phenomena—why does this particular person possess these particular disorders? Contrary to that, the interests of cognitive science, evolutionary psychology and the neurosciences lie (for the most part) on a species-specific level, bracketing individual differences. For example, evolutionary psychology can offer explanations as to why women in general, or probabilistically, prefer certain kind of men. Applying this kind of finding to individual cases is a complicated affair—how and to what extent this kind of evolutionary matter has a bearing on, for example, why Jane voted for Hank. If Hank possesses features that women are attracted to, those (unconscious) factors make it more probable that

Jane and other women shall vote for him. However, other matters may also overwhelm the evolutionary factor. These considerations bring us to the preliminary conclusion that perhaps we should not search for a single (correct) explanation for phenomena. Instead, there are several psychological, social and neurobiological matters that cause, or at least affect, how we feel, react, and behave.

Causes, reasons, and clinicians' realism

Cause is a metaphysical concept. Non-philosophers tend to think that if B always follows A, some kind of power, force or energy has to be responsible for that state of things. However, David Hume challenged that traditional conception and argued that causation is an empirical matter: we talk about causes only because we have noted that B always follows A. According to the Humean lines of thought, popular among present-day philosophers, a cause is a theoretical concept, and causal relation is something we project into the world. Let us note here that even in the domain of natural science it is very difficult to show a strict causal relationship (Chalmers, 1999; Loux, 2006, pp. 187–204; Rosenberg, 2000, pp. 21–47).

It is often claimed that talk about causes belongs to the natural sciences, and the humanities focus on the *reasons* of behaviour. The logic behind the claim is that when a cause for an occurrence in the physical world (for instance, raising a hand) is searched for, a mentalistic explanation ("Jane raised her hand because she wanted to vote for Hank") does not specify the material cause of the occurrence (i.e., the activity of certain neural networks). When restricting the use of the term "cause" to the domain of natural science, one ensures that the materialistic doctrines do not become violated.

However, such a stand is against common sense; it is not *realistic*. Everybody "knows" that my desire to have a beer is the cause for going to the pub, and Jane's desire to vote for Hank was the cause for raising her hand. Thus, it is sometimes argued that mental matters may also cause behaviour. Advocates of that view (for example, Bolton & Hill, 2003; Lennon, 1990) remind us that mental states are coded in the brain, and, thus, materialistic doctrines do not become (seriously) violated by holding mental states as causes of behaviour. This issue is rather complicated (see Kim, 2005), but for our topic it is not necessary to go deeper into it—it is, anyway, clear that clinicians cannot help being "realists"

here, and to think of desires, fears, memories and fantasies as causing different matters.

Philosophers of science stress that behind different kinds of real-life occurrences and events we do not find a single cause. In the literature we often find the following example as illustrating this state of things: the causes of car accidents depend on if we focus on the driver(s), the car(s), the weather, or the road. Driver's error is not necessarily sufficient cause: if the brakes of the car had been better, or if the surface of the road had not been so slippery, the accident would not have happened. Plurality of causes holds also with the current topic. Actually, when thinking of the relations between both neurobiology (or biological psychiatry) and psychology, and different schools of psychotherapy, it is extremely crucial to keep in mind that behind whatever disorder there is unlikely to be a single cause but actually several. Below I will shed light on this issue from several viewpoints. We will begin with the biologist Ernst Mayr's distinction between two kinds of causes.

Distal (ultimate) and proximal causes

Ernst Mayr talked about distal and proximal causes in order to characterise different branches of biology (for example, Mayr, 1997, pp. 107–123). When we think of, for instance, why giraffes have long necks, we find that genes are one cause. Another cause can be sketched through evolutionary theory: giraffes with longer necks survived better such that they could reproduce more than their shorter-necked fellow creatures, and this is the cause for present-day giraffes' long necks. The former (gene expression) is a *proximal* cause, and the latter (selective reproductive advantage) is the *distal* (or ultimate) one. It is evident that these two kinds of causes are not exclusive.

By applying Mayr's scheme we can make a chart showing the different causes of psychic disorders. Let us start with the distinction between psychological and neurobiological causes. The cause of depression, for example, may be stated in terms of intrapsychic conflicts (psychological causes), or by referring to the neurotransmitters of one's brain (neurobiological cause). Both causes may exist at the same time. Second, in the categories of both psychological and neurobiological causes there are distal and proximal causes. Distal psychological causes for depression may be found from early object-relations, and repression of certain desires may be a proximal psychological cause. A distal neurobiological

cause may be, for example, a defect during the pre-natal period, and an imbalance of neurotransmitters a proximal neurobiological cause.

Distal and proximal causes are different sides of the same coin: the distal cause can be seen as setting the proximal cause in an historical context. Consider that a proximate cause of depression is repression of certain ideas. Repression of that idea in particular, or use of repression as a defence in general, has historical origins that constitute a distal cause. Usually the historical cause is not a particular event, a trauma, but rather a web of earlier experiences, or general conditions of early life. Similarly, the distal cause of a pre-natal defect has changed the brain, and a proximal cause of certain problems can be stated in terms of the neurophysiological consequences of the distal cause.

There are also connections between psychological and neurobiological causes. Presumably all mental matters possess neurophysical counterparts, and thus early psychic traumas have to be thought to change the brain. Thus, when the trauma later affects one's life, those psychic troubles possess both neurobiological and psychological distal causes. In such a case, the psychological distal cause appears as the "original" cause, but in general it is not always clear whether psychological factors proceed the neurobiological or vice versa.

Daniel Dennett (1987, pp. 13–35) has created a formal model of different kinds of explanations. Let us present just a brief sketch of it. According to Dennett's model there are three kinds of explanation. In order to stress that the appropriateness of an explanation is tied to a researcher's viewpoint and interests, Dennett talks about *stances*—one can approach phenomena in terms of physical, design and intentional stances. "Physical stance" refers to the explanations of natural sciences, neurobiology among them. "Intentional stance" means that an organism's acts are made sense of in terms of its desires, beliefs, and fears (folk-psychology-type explanations). In the core of the middle level of explanation, "design stance", there are functions. When we say that giraffes have long necks due to the logic of evolution (referring to the evolutionary function of a long neck), and that in a certain person garrulousness is a defence (a defensive function of verbosity), we present a design level explanation. Psychological explanations often mix intentional and design level perspectives.

The moral here is that it does not make sense to think that a disorder has either a neurobiological or psychological cause, or just one cause. It is a difficult empirical issue as to what are the actual causes of a certain

disorder. In principle, however, we must think that disorders may be approached from several perspectives, and that there are several different causes behind them. The relevance of each of the causes depends on many matters. One cause may be something like a general boundary condition, which is interesting for an evolutionary psychologist, but not for a clinician.

A psychoanalytic reader may find the above lines of thought familiar. Namely, Freud had stressed already in his early writings that dreams and disorders are multi-determined, and analytic candidates are sometimes taught to study their clients' problems from the topographical, structural, genetic (historical or developmental), energetic and dynamic viewpoints. Familiarity may also be due to the fact that the psychoanalytic tradition entails many different models and theories.

On the causes of psychic disorders—sketching the battleground around Adolf Grünbaum's critique

The issue of the causes of psychic disorders, and whether or not psychoanalysis has revealed them, has come under discussion through Adolf Grünbaum's works. Grünbaum has presented an influential critique concerning the scientific status of psychoanalysis, and his view has given rise to a very lively and extensive debate that has already lasted for almost three decades. The logic of his (Grünbaum, 1993a) main thesis may be stated as follows:

> Freud presented psychoanalysis as an etiological theory that states that repressed desires, traumatic experiences or sexual ideas are necessary and sufficient causes of disorders. However, it is not possible to verify causal claims based on clinical material, i.e., case studies. Neo-Freudian psychoanalysis has suggested different aetiologies for psychic disorders, but they share the basic problem of Freud's view: psychoanalysis presents causal explanations, but is not able to confirm them.

Psychoanalysts have reacted to Grünbaum's views in many ways, but often the logic of the defence is either that, actually, psychoanalysis *can* present causal explanations, or that psychoanalysis is a hermeneutic discipline that *does* (or *should*) *not try* to do that. The above considerations on the plurality of causes hint that the Freudo-Grünbaumian debate may have gone awry in a foundational manner. Below we will see that

when set in a wider context the dispute around Grünbaum's critique appears as a battle; one that continues, although the war—if such has ever been—is over.

The Grünbaum battle is commonly known even outside of psychoanalytic circles, but it is rarely noticed that it does not necessarily have a lot to do with the psychoanalytic therapy as practised by present-day clinicians. Grünbaum and, for example, Edward Erwin (1996) restrict their studies to Freud's views, and I cannot avoid thinking that they do so partly because Freud's hundred-year-old views are an easy target for criticism. Psychoanalysts and psychodynamic therapists that might advocate Freud's views as presented by Grünbaum and Erwin form a minority of the psychoanalytic community.

Since Freud's views are still highly respected in psychoanalytic communities, analysts and therapists that are, say, "less Freudian", typically avoid addressing explicit criticism towards them. When talking about revisions of psychoanalytic theories, psychoanalytic folks often state in a somewhat obscure way that the revisions do not necessarily replace or contradict Freud's conceptions. Anyway, post-Freudian psychoanalysis has developed original conceptualisations on the causes of disorders and the aims of psychotherapy, and it is worth introducing, briefly, some of these.

Peter Fonagy, the former chairman of the International Psychoanalytic Association, and his collaborators state: "The goal of therapy, then, is the observation of patterns of interaction and the identification and correction of maladaptive models, principally through strengthening an overarching mental capacity to activate alternative models of interaction selectively; in language influenced by cognitive science, this capacity may be labelled 'mentalization' or 'reflective function'" (Fonagy, Gergely, Jurist, & Target, 2002, pp. 470–471). Thomas Ogden (1997, p. 15) believes "that the analytic task most fundamentally involves the effort of the analytic pair to help the analysand become human in a fuller sense than he has been able to achieve to this point. This is no abstract, philosophical quest; it is a requirement of the species as basic as the need for food and air." In his book treating the psychoanalytic treatment of people diagnosed as possessing a borderline personality organisation, Vamik Volkan states: "I focus on the therapeutic regression and subsequent progressive developments that occur over the course of lengthy and intense treatment in which the patient successfully identifies with some of the therapist's functions, particularly his ability to analyse and integrate" (Volkan, 1987, p. 1).

Some hermeneutic researchers like Jürgen Habermas and Paul Ricoeur hold—contrary to Freud—that psychoanalysis does not study causes but meanings. However, it is difficult to maintain both that a psychoanalytic cure often relieves the disorders and that the discussion between the therapist and a client has nothing to do with the causes of the disorders. As Erwin puts it, if the meanings "make a difference, if they do affect something, then they are causes. They may not enter into causal law; they may not be either causally necessary or causally sufficient, but they are causally relevant factors" (Erwin, 2010, p. 72; see also Grünbaum, 2004).

Erwin's view leads us to ask whether there is *a* cause behind Laura's depression or Larry's phobia, or if there are several causes. Grünbaum and Erwin base their critiques on the idea that Freud thought that behind certain kinds of psychic disorders there are always *similar* causes, and also that for each of these there is a *single* cause. Consider someone having seen a violent assault at the age of six, having had violent nightmares thereafter, and falling into psychosis after having seen a violent movie at the age of thirty. There seems to be evident relationships between early trauma, nightmares, and psychosis. Grünbaum calls this kind of relation "thematic kinships", and argues that thematic kinship does not justify talk about causal relations. Erwin (2010, p. 73) agrees with him. When considering a man promoting his attractive female supervisor, and his desire to have sex with her, Erwin points out that it may be the case that the man's desire to have sex played no role (i.e., it is not a cause) in his promoting his supervisor.

Regardless of whether thematic kinship or meaning connection is or is not *proof* for a causal relationship, could there be, in principle, a causal relationship between the trauma on the one hand, and the nightmare and the psychosis on the other? Is it possible that an early trauma was a necessary and sufficient cause of the dreams and psychosis of later life? If early trauma were a necessary and sufficient cause of the dreams and psychosis of later life, everyone having had experienced the trauma would have had nightmares, and suffered psychosis after having seen the violent movie. That is surely not true, and—as suggested above—we should hold that behind disorders there are always several causes: different (traumatic) experiences during the lifespan, genetic matters, and several properties of one's structure of mind or personality. This should not be news for any clinician.

When studying a psychoanalytic way to explain disorders and create theories, one should not treat psychoanalysis as an island, but rather

take a look at the other disciplines that operate on the same field. The fields of psychiatry, psychology and psychotherapy form the general context for the topic of the causes and explanations of psychic disorders. The multi-determination of disorders is widely accepted in psychiatry (see, for example, Bolton & Hill, 2003, pp. 241–278; Cooper, 2007, pp. 83–101; Mitchell, 2008; Murphy, 2008; Paris, 2008, pp. 6–9; Schaffner, 2008; Woodward, 2008). Let us cite John Campbell, who states: "My conclusion is that we should take at face value the flood of empirical work that suggests causation in psychiatry is multifactorial and the causal explanation will characteristically be 'many sorted', using variables of many different types" (Campbell, 2008, p. 214). Considering the above mentioned mainstream view of psychiatry, we might say that it is a form of scientific mutilation to lean on an idea that psychoanalysis could reveal *the* cause behind psychic disorders. A critic demanding that psychoanalysis presents the causes of a disorder appears undue, and a psychoanalyst presenting such causes appears as a megalomaniac.

When the question concerning the scientific status of psychoanalysis is raised, psychoanalysis should be set not just in the context of psychiatry but also in the context of the psychotherapies—could we say that one or several schools of psychotherapy are scientific? In addition to psychoanalysis, the only school of psychotherapy that at least tries to relate its methods and theories to the views of present-day research is cognitive psychotherapy. Similar to psychoanalysis, cognitive psychotherapy, too, has divided into several branches (cognitive-behavioural, cognitive-constructive, cognitive-analytic), and there is no shared view on the causes of disorders. All in all, we should not think as if there was some kind of battle between psychoanalytic folks and some Grünbaumians. Instead, we should ask whether such a practical and idiosyncratic affair as psychotherapy between two persons can be studied by the methods of science and humanities. Below I will approach this issue.

Idiosyncratic study and the networks of causes

Let me introduce still one more framework that enables us to make sense of the existence of several causes, and the logic between statistical and idiographic study. Mackie (1965, pp. 306–308) introduces the concept of "causal field", and when "influenza" in his original example is replaced with "depression", his idea appears as follows. On the most

general level the question "What causes depression?" can be understood as calling for an answer as to why some people "get" depression and some others do not. In that case the causal field is human beings in general. When recalling that certain matters (genetic liability and a certain kind of personal history) increase the probability of depression, we can see another, more restricted causal field: human beings possessing predispositions for depression. In the scope of this field the question is why only a few people possessing those predispositions get depression. The "What causes depression?" question may be raised also in a singular case, and in that context the causal field may be seen in terms of the career of the person in question—why he or she developed depression at a particular age, and not sooner or later. Mackie reminds that "what is the cause in relation to one field may not be the cause in relation to another" (Mackie, 1965, p. 307). The term "causal field" enables us to formalise the erroneousness of the Grünbaumian battlefield. Grünbaum's presupposition is the claim that according to Freud it is enough to think in terms of only one causal field—behind all depressions, hysterias, perversions and so forth there is a certain single cause. Regardless of whether or not Freud actually thought that way, in the current state of things the presupposition is foolish.

The network of the causes of disorders and the curative aspects of psychotherapy—or rather, how we conceptualise these two issues—intertwine in a complex manner. Psychic disorders may have such distal causes as pre-natal trauma, the death of a parent, and problems with early interaction with primary caretakers. An empirical study may show on a statistical level that such matters cause psychic disorders. When practising and studying psychotherapy those kinds of causes are rather insignificant, since they describe only outer conditions, not a person's mind/brain. They are also very rough in their outline: the death of a parent, for example, is and may be traumatic in many different ways.

Outer conditions may be partial causes of disorders by affecting a person's mind/brain, or the functioning of it. In terms of Mayr's conceptualisation, these matters are distal causes, and conceptualising the distal causes of psychic disorders is a rather straightforward issue. The situation is radically different with proximal causes, since conceptualisation is always more theory-laden. We might present countless suggestions as to what kind of matters the early loss of a parent might give rise to. Or, to put it in other words, a distal cause is either basically just an event (death of a parent), a characteristic of one's environment (nature

of interaction with parents), or physical matter (pre-natal trauma), but there are numerous ways to conceptualise what kind of proximal cause it is transformed into.

For example, the following matters may be claimed as a proximal cause of a psychic disorder or other mental trouble: low self-esteem; proclivity to negative thinking; weak ego; fear of losing a close person (again); a strict super-ego; repressed desire; proclivity to feelings of guilt; and a low level of serotonin. As we see, many matters often suggested as proximal cause are not "things in the head" but behavioural dispositions or other kinds of constructs (see Talvitie, 2012, pp. 1–15). It is easy to agree with Erwin when he states: "For a science of psychotherapy to exist, however, there need not be such [natural] laws [as in physics and chemistry]. It is enough that there be clinically significant true generalizations between certain causally relevant factors and certain effects … they may be of the sort 'under certain initial conditions, C, A-type events generally make an important causal difference to the occurrence of B-type events'" (Erwin, 1997, p. 78).

Having approached the causes of disorders on a general level, or in principle, let us study therapists' knowledge concerning the networks of causes in a particular case after a long successful therapy. Consider case studies, presented frequently in clinicians' congresses and journals. In some cases clinical data speaks in a convincing manner in favour of certain causes, and in some other cases disagreements emerge among clinicians. Disagreements tend to relate to the therapists' different theoretical backgrounds.

Let us still assume that a psychotherapist's client had been in comprehensive psychological testing at the ages of, say, seven, fifteen, and thirty, and the psychologists' reports were available for clinicians. I think that the additional data would not change the basic picture: in some cases a consensus will emerge; in other cases clinicians disagree. (Erwin [1996, pp. 87–142] has presented a comprehensive study on the problems of determining the causes of disorders based on clinical case study.) Actually, this is the core problem of induction: when there is a claim X (for example, "all swans are white", or "Ben's disorders are caused by the gene X and his relationship to his mother") it is not possible to determine what evidence should be accepted as sufficient proof to validate the claim.

In the beginning of the chapter I asserted that we should favour those psychotherapy schools that take seriously academic ideas on what kind

of causes and reasons may lie behind psychic disorders. On that basis, psychotherapy schools should create models on how and why psychotherapy works—although the models are in the current state of knowledge doomed to be imperfect. The situation is somewhat frustrating, and the considerations of this section open an additional frustrating aspect: in the context of case studies, it is never possible to *prove* (in the strict sense of the word) that clients' disorders are caused by certain matters.

Rethinking the grounds for being scientific (enough)

In his book *Psychoanalysis at the Margins*, Paul Stepansky (2009) states that theoretical pluralism has become a "resting place"—the existence of several more and less conflicting psychoanalytic theories has been commonly accepted in the psychoanalytic community. Stepansky is rather sceptical that psychoanalysis could get from the state of controversies and (cheap) theoretical pluralism to that of normal science, in which psychoanalytic knowledge would begin to cumulate; according to him, certain ameliorative efforts to find common ground have indeed "stimulated a further round of animated controversy" (Stepansky, 2009, p. 213). I share Stepansky's pessimism, since considering that theories and explanations in general are always tied to certain viewpoints and interests, the plurality of psychoanalysis is by no means surprising.

Despite the diversity of psychoanalytic and other psychotherapeutic schools, psychotherapy in general is a very effective form of cure. In addition to this finding, consulting literature on psychotherapy research (for example, Cooper, 2008; Paris, 2008, pp. 23–30) informs us that in the current state of things it is problematic to argue that one form of psychotherapy is superior to others, particularly as we do not know what it is in psychotherapy that cures. Thus, the talking cure somehow affects the proximal causes of disorders, but, similarly, because there are lots of candidates for the causes of disorders, the curative aspects of psychotherapy may also be seen in many ways—for example, the working through of traumatic memories or relationships; operant conditioning; relationship with the therapist (empathy, corrective experience); becoming conscious of intrapsychic conflicts; cognitive shifting; or becoming able to recognise better one's feelings, desires, and fears.

Since disorders are supposed to possess several necessary but not sufficient causes, the talking cure may be successful for many different

reasons. In practice it is extremely difficult to show which of the causes the cure has affected: "In summary, there is no direct cause-and-effect relationship between either biological or psychological factors and mental disorders … People become ill only when they suffer from temperamental vulnerability *and* are exposed to environmental stressors. This is why no theory exclusively based on biology (or psychology) can explain why people develop mental illnesses" (Paris, 2008, p. 9). It seems that one should not even conceive of presenting either a complete, non-theory-laden explanation of the causes of disorders, or the curative factors of psychotherapy.

The age-old Freud wars directed us to think that the most important criterion for a theory is that it presents the cause(s) of disorders in a truthful manner—otherwise it could not direct a therapist towards correct interventions, since therapeutic techniques are always based on therapists' background theories. Joel Paris rephrases the critique I have presented above towards the Grünbaumian battlefield: "The problem with psychoanalysis is not that it is too complicated, but that it is too simple … As a therapy, its failures were rooted in grandiose claims, unsupported by data" (Paris, 2005, p. 156). I think the considerations presented so far on the Freud wars—the causes (of psychic disorders) and the nature of the talking cure—should lead us to rethink the grounds on which the scientific standing of psychoanalytic models and theories are assessed. Psychoanalysis may have been evaluated by wrong criteria, originating from a very different kind of study. As a matter of fact, such a claim is often presented, referring to the distinction between natural science and hermeneutics (which the participants and spectators of the Freud wars are very well aware). Below I introduce another distinction, which appears (at least for me) more fruitful.

From general psychology to micropsychology

In the scope of the disciplines of economy and history, at least, we find talk about "micro" and "macro" approaches to the subject matter. In economics the idea of a micro-viewpoint began to evolve at the turn of the nineteenth and twentieth centuries. In *Microeconomic Theory* Mas-Colell, Whinston and Green (1995) characterise microeconomics as follows: "A distinctive feature of microeconomic theory is that it aims to model economic activity as an interaction of individual economic

agents pursuing their private interests" (Mas-Colell, Whinston & Green 1995, p. 3).

Below I sketch another frame for psychoanalytic study by applying the distinction between micro- and macrohistory. I lean on Carlo Ginzburg's collection of essays in Finnish (Ginzburg, 1996) and certain chapters of Rahikainen and Fellman, 2012; Peltonen, 2012, and Levi, 2012. Let me admit that the introduction is non-comprehensive and—due to my aim to make sense of the essence of psychoanalytic study—is also in a certain sense biased.

Interest in microhistory emerged in Italy in the 1980s, Carlo Ginzburg and Giovanni Levi being the core figures. The interpretation of clues in historical research is one of the main themes of the field. Not surprisingly, Sigmund Freud and his "Sherlock Holmes method" are repeatedly mentioned. On this basis, microhistorians' views concerning the nature of humanistic research might (or should) have had a significant role in the Freud wars. However, psychoanalytic literature refers just occasionally to microhistory (see psychoanalytic literature search www.pep-web.org/).

History is the discipline that discovers, collects, organises and presents information about past events. We are accustomed to matters such as revolutions, wars, and leaders enjoying key roles in historians' narratives—history that focusses on events that possess general and widespread significance. Microhistory has, however, challenged this kind of thinking. It has been argued that a detailed study of one ("ordinary") person, one village, small scale happenings and individual events may be fruitful toward understanding more general matters. Grand narratives about revolutions, migrations and wars are abstractions and generalisations: they have "eaten"—or rather they emerge from—numerous small-scale events; people's experiences and interpretations; dynamics between groups, ways of thinking, political and geological issues, and so on. On the one hand microhistorians challenge the hegemony of macrohistory and remind us that macrohistory is not the only history. On the other hand they focus on how a great number of micro (historical) matters are converted into but a few macro matters. It is worth asking *how*—based on which kind of principles or rules, generalisations, and abstractions—are these conversions actually made.

Everybody agrees that macrohistory must not distort the actual state of affairs. However, there are lots of viewpoints, minor-scale events,

and people, and there are always tensions between informants, and different kinds of data and interpretations. A historian must choose which one he or she takes into account. It is also clear that the principles of narration (or narratives) affect macrohistory: historical (as well as other) narratives have to present a (relatively simple) logic between consecutive events. There are also restrictions on how much a narrative can contain examples that are exceptions, make incongruences explicit, introduce tensions between different interpretations of data, and make the complicatedness of the picture visible. If a narrative goes too much in these directions, it is not any more a narrative (or at least an attractive narrative).

Microhistory brings up small-scale events and makes critical remarks on generalisations and grand narratives of macrohistory. However, microhistory is not *against* macrohistory. Levi states that microhistory "describes the priorities of a historian who wants to insist on the absolutely unrepeatable nature of an event while at the same time retaining the possibility of generalizing from particular cases … history is the science of focusing on the specificity of cases and of generalizing not their consequences but the questions arising out of them" (Levi, 2012, pp. 128–129). He presents Freud's idea of the Oedipus complex as a "somewhat paradoxical" example of such an activity. Let us take another citation from Levi: "Microhistory therefore does not abstract the fact under observation from its general context, but rather it attempts to establish, rigorously examining the single case, fundamental questions that allow a reconstruction of reality …" (Levi, 2012, p. 129).

A reader has probably already begun to apply microhistorians' ideas to the topic at hand. "Macropsychology", a word that seems not to have been used in this sense, would refer to "general psychology", or cognitive and evolutionary psychologists' efforts to determine species-specific characteristics of man: number and nature of memory systems, capacity of short-term memory, origins of motivation, functioning of sensory systems, structure of personality, and so on.

Psychobiography (see Schultz, 2005), clinical psychology in general and perhaps all domains of applied psychology should be held to fall in the domain of micropsychology: their primary interest is in individual, unrepeatable cases (let us note that the term "micropsychology" has also another meaning, see www.micropsychology.org/). The relation between general (macro) psychology (or behavioural sciences in general) and psychotherapy resembles that of macro- and microhistory.

Thus, I believe that microhistorians' ideas resonate strongly among clinicians pondering the scientific status of psychoanalysis and other psychotherapies. Psychotherapists, too, work with complex, unique cases, and thus they face similar questions as microhistorians: how macro theories might contribute to our (clinical) work, and how microphenomena should be abstracted in order to become incorporated in macrotheories.

Perhaps we can find from the field of philosophy of science an orientation according to which macropsychology falls within the scope of science but micropsychology does not. Even the members of such an eccentric and backward community would nevertheless admit that each branch or school of micropsychology (psychoanalysis, cognitive psychotherapy, psychospiritual healing, etc.) is more or less scientific, and we should presume them to be as scientific as possible.

In terms of the micro-/macropsychology distinction, Freud's metapsychology appears as macropsychological theory: it aims at describing general (psychological) characteristics of man, and suggests that there are laws in human psychic functioning. Elsewhere (Talvitie, 2012, 2015) I argued that the macro part of psychoanalysis is dependent upon metaphysical presuppositions, which makes thoughtful and genuine communication with present-day academic community impossible. As nothing has dissuaded me from this view, I cannot avoid thinking that Freudian macropsychology has turned into a hopeless project.

In this situation, psychoanalysis should take a step back and approach the relation between macro- and micropsychology in a rather modest way. Let us set the starting point of this kind of project by paraphrasing Levi (see above): psychoanalysis should insist on the absolutely unrepeatable nature of clinical phenomena while at the same time retaining the possibility of generalising from particular cases. Below I will sketch how we might see psychoanalytic models and theories on this kind of basis.

What are psychoanalytic theories for, and what should they be like?

There is an important difference between microhistorians' and psychotherapists' efforts: while the former possess theoretical objectives typical for academic researchers, the latter are paid in order to promote change in the state of things of the world (i.e., the client's life or psychic

condition). This matter becomes reflected, also, in the functions of theory in the field of psychotherapy. Let us present a list of the possible functions and requirements for a scientific-enough background theory of psychotherapy:

1. A theory, or all of its parts and presupposition, can never be proven right. A realistic criterion for a theory is that its presuppositions and claims are supported by the prevailing views of science, or at least there are no serious tensions with contiguous academic disciplines in this respect.
2. There is a circular relationship between clinical theories and the techniques of psychotherapy—both shape the other. The foundational function of theories is to set the basic lines of clinical techniques and clinical practice. A therapist may have only rather vague hypotheses on the causes of clients' disorders, and how and why therapy might relieve them. Thus, from the perspective of our topic, the crucial aspect of the therapeutic technique and setting, and the purpose of the theories behind the particular school of psychotherapy, is to direct or help the therapist and the client to talk about significant matters (whatever they appear to be). To put it another way, theories should increase therapists' sensitivity to recognising the topics and kinds of interaction that may be crucial for the therapy. In a word, the nature of therapeutic technique should be *explorative*.
3. Because both the causes of disorders and the phenomenal reality of clients are always idiosyncratic, a therapist should be good at imagining the personal dynamics of each person (see, for example, Reeder's [2004, pp. 37–52] sophisticated study). Reading theories and case studies presented in the psychotherapeutic literature enhances a therapist's abilities in this respect, so that he or she is able to make better therapeutic interventions. On this basis we might expect that it is favourable if a therapist is not restricted in his or her theoretical thinking.
4. Sometimes psychotherapeutic relationships are very confusing and anxiety-provoking (also) for therapists. By sketching the dynamics of interactions on a theoretical level, theories enable therapists to cope with this aspect of his or her work.

When psychotherapists' or psychotherapy schools' background theories are examined in academic forums, theories' multiple functions

are standardly neglected. When taking into account their practical micropsychology essence, we are led to a much more cautious stance toward theories than both the advocates and critics of psychoanalysis most often suggest.

In his excellent book *Psychiatry in the Scientific Image*, Dominic Murphy (2006) tightens the dynamics between academic study and clinical practice by stating: "The scientific project generalizes, whereas the clinical one uses the resources of the science to deal with particulars" (Murphy, 2006, p. 205). As a matter of fact, after having acquainted oneself with economists' and historians' micro-viewpoint, Murphy's ideas sound familiar: "In trying to explain a mental disorder, we prescind from clinical variation across individual clients and treat the explanatory target as a process that unfolds the same way over and over again—a set of phenomena that usually occur together (the signs and symptoms) and that have a natural history (or course)—as a characteristic process that unfolds in a typical, though not wholly determinate, way" (Murphy, 2006, p. 205). Let us see how he continues: "To explain a mental disorder, then, is to explain an idealized picture of that disorder, to show what causes and sustains it, abstracted away from many of the details of its realization in individual clients. The detailed forms that pathologies take in individuals are the focus of clinical project, not the scientific one" (Murphy, 2006, p. 205). Murphy calls these kinds of psychiatric theories *examplars*. Rosenberg (2000, p. 51), for his part, states that most ordinary and many scientific explanations are explanation sketches. There is no reason to presume psychoanalytic theories to be something more.

Conclusion

We can draw together the above considerations by stating that the main problem with psychoanalytic theories and explanations is that both psychoanalytic folks and the critics of psychoanalysis possess too high expectations towards them. Currently there is no single explanation for any psychic disorder, no paradigm of research/treatment that has created a wide ranging and commonly accepted model on the causes of psychic disorders, and psychotherapy research is not able to provide either the significance of therapist's background theory or the curative factors of psychotherapy. If one argues from this state of the art, in either a pro or contra psychoanalysis position, that a discipline should

or can present causal explanations for disorders (perhaps even based on a single principle), it is legitimate to state that he or she has completely lost contact of the study put forward in the academic world.

During the past few decades there have been endless debates on what Freud actually meant; whether or not recent findings support his hundred-year-old hypotheses, and whether case studies may provide a basis for scientific models. I think it is time to take leave of those discussions and in place reorient the field on the basis sketched above. In the philosophy of science the boundaries of science are studied under the heading of "demarcation problem". Debates around the scientific standing of psychoanalysis reflect the presupposition that scientific endeavours are good, and that non-scientific ones are dubious. In those debates the term "scientific" appears as a guarantee about something, or like an honorary title bestowed to distinguished efforts. However, this is a rather idealised conception of science (or, rather, of life). Namely, science has nothing to say about many extremely crucial issues of life for example: Should I marry Sophia? Would I be happier with my life if I began to study medicine? Is Jimi Hendrix better with guitar than Ludwig Beethoven is on keyboards? Should I move to Houston? Would it make sense and life better if I practised my golf swing for several hours a week?

There are at least two reasons why psychotherapy may be an affair that cannot be characterised as scientific—only *scientific enough*. First, psychotherapy is closely related to values (and most philosophers tell us that it is impossible to deduce values from the facts). Second, it is not possible to manage the huge amount of variables that affect psychotherapy (see Slife, 2004; Smith, 2009). Although psychotherapy could not be a science or scientific its background theories and methods may nevertheless depend more or less on the activities of science. Basically there are three requirements for a scientific-enough and practically adequate background theory of psychotherapy: it is based on academic study or at least does not contradict it; it directs therapists to act in such a manner that psychotherapy is useful for clients; and therapists can live with it. When a background theory aims at being something more it is easily in conflict with prevailing views of research.

CHAPTER FIVE

Psychoanalysis and philosophy of science: reply to Brakel and Talvitie

Edward Erwin

In this volume, Linda Brakel (Chapters Three, and Six) and Vesa Talvitie (Chapter Four) raise some important issues concerning psychoanalysis and philosophy of science. Brakel criticises arguments of the philosopher of science Adolf Grünbaum, whose work is generally thought to provide the most powerful critique of Freud's theories (Levy, 1996; Robinson, 1993). Although there have been many replies to Grünbaum, hers is among the most detailed available. Talvitie writes about a range of topics, including the concept of causation appropriate for discussing psychoanalysis, philosophy of science critiques of psychoanalysis, and its metaphysical status.

Brakel's critique

Brakel finds two overarching problems with Grünbaum's account: the first is that he takes psychoanalytic clinical theory to be the core of psychoanalysis, thus raising many problems that can be avoided by taking her theory of mind (Brakel, 2009) to be the core theory. The second problem is that he relies on Freud's statements to determine all versions of psychoanalysis, not allowing for more recent modifications. The result

is that many of his arguments have a straw man opponent, vitiating the overall importance of his critique.

As to Brakel's first point, talking about another theory does nothing to avoid the problems Grünbaum raises for Freud's theory. Brakel is just changing the subject, but is nevertheless making a good point. When people speak of criticisms of psychoanalysis, they often mean Freud's version. Contemporary psychoanalysts may rightly complain that some of these criticisms have little to do with the theory or therapy they support. This is a sound complaint, but it may not be valid for all such criticisms.

Although Grünbaum's work focuses on Freud's theory, it is primarily a work in applied philosophy of science. The epistemological arguments he gives might be relevant to other versions of psychoanalysis. I shall later argue that indeed they are relevant to all psychoanalytic theories and therapies (see the section on modern day psychoanalysis). The arguments cannot be avoided by talking about newer versions of psychoanalysis. As to her second point, Grünbaum does not assume that Freud's views determine the content of all of psychoanalysis. See his discussion of post-Freudian psychoanalysis (this volume, pp. 32–34, and his reference to Morris Eagle's discussion of other versions of psychoanalysis, Eagle, 1993).

The dynamic and cognitive unconscious

Brakel questions whether cognitive psychologists are right in asking for a clear separation between the cognitive and dynamic unconscious. Where, she asks, is the evidence that these are separate domains? Grünbaum, too, she initially suggests, has apparently accepted the implausible notion that there are radically separate instantiations of the unconscious, but she later says, rightly, that this is not his position.

Grünbaum, the psychoanalyst Morris Eagle, and I have all questioned whether studies of the cognitive unconscious provide evidence for Freud's theory of the unconscious, but not one of us presupposes the existence of two separate domains of the unconscious. The issue being discussed (Grünbaum, this volume; Eagle, 1987; Erwin, 1996, pp. 215–224) concerns theories not domains. Of course, if Freud's theory of the dynamic unconscious is true, then one may speak of the domain to which it applies, but it is still his theory that is the focus of interest.

If Freud's theory of the unconscious were to say merely that there are unconscious mental events and processes, then cognitive experiments supporting unconscious mentality would provide evidence for his theory, but this is not his theory. Despite what many have said, Freud did not discover the unconscious. Nor did he claim to have made this discovery. When discussing the work of Theodore Lipps, one of his contemporaries, Freud makes the point that what is at issue between Lipps and his critics is just the thesis that unconscious psychical processes exist (1900a, p. 614). Like many before him, including Helmholtz, Leibniz, Herbart, Schopenhauer, Eduard von Hartmann, and Nietzsche (see Sand, 2002 on Herbart; Zentner, 2002, on Freud's other predecessors), Lipps says that besides conscious processes there are also unconscious ones and they are mental. But it was not to establish this thesis, Freud says, that we have "summoned up the phenomena of dreams and the formation of hysterical symptoms". His "new discovery", Freud says, taught by the analysis of psycho-pathological structures, is that in addition to a preconscious, there is also what he terms the *Ucs.*, the unconscious, which is *inadmissible* to consciousness (pp. 614–615). This inadmissibility claim is central to Freud's theory of the dynamic unconscious, but it is not the whole of it. If it were, certain cognitivist theories in psychology and linguistics would also qualify as theories of the dynamic unconscious. On Chomsky's theory of generative grammar, for example, there are unconscious ideas that cannot be brought to consciousness. But the theory of the dynamic unconscious contains more than the thesis of non-recoverability. If the rest of the theory were deleted, many of Freud's other theories, including his theory of the aetiology of neuroses and how psychoanalytic therapy works, would collapse.

What is also central is a thesis about what keeps unconscious thoughts and wishes unconscious, mainly repression, as well as his therapeutic theory of how to bring to consciousness what had been temporarily inadmissible to it (or at least a close replica of it). In brief, his theory of the dynamic unconscious includes the following: unconscious ideas and wishes have a dynamic quality in that they have effects; they are also typically prevented from entering consciousness due to the operation of repression or other defence mechanisms; and they can enter consciousness only when the repression is "lifted", which requires overcoming the person's resistances through the use of psychoanalysis.

Preconscious ideas also have effects but they lack the other features of unconscious ideas.

In studies of the cognitive unconscious, mainly those studying perceptual defence, subliminal perception, and semantic priming, experimenters for the most part were not testing Freudian theory. Exceptions are Blum (1954) and the multiple studies done by Silverman and his colleagues. Blum's study is criticised in Erwin (1984) and the Silverman studies in Erwin (1996, pp. 198–209). Brakel cites newer studies of subliminal perception (Shevrin et al., 1992, 1996; Shevrin, 2012) that test psychoanalytic hypotheses. I discuss these below (see the section "Brakel's theory").

As to the non-psychoanalytic studies, one could argue that no matter what theory was being tested in the cognitive experiments, indirect support was provided for the theory of the dynamic unconscious, as Erdelyi (1985) suggests, but I argue (Erwin, 1996, pp. 198–209, 215–236; see also, Eagle, 1987) that Erdelyi is mistaken and that no support is provided for Freud's theory of the dynamic unconscious.

Grünbaum, contrary to what Brakel claims (Chapter Three, pp. 60–61), does not dismiss the concept of a dynamic unconscious. After all, we need to use it in discussing whether Freud was right or wrong. It is Freud's theory of the dynamic unconscious that Grünbaum is sceptical about, as am I. Perhaps this is what Brakel has in mind.

Because of the logical connections between Freud's theory of the dynamic unconscious and his theses about repression, slips, the latent content of dreams, and much else that Freud theorises about, it is hard to say in a short space what the arguments are that undermine the evidence for the dynamic theory without also discussing the evidence for or against these related theses. But something brief can be said. The arguments of Grünbaum's (1984), especially part II, if sound, undermine the Freudian grounds for the theory of the dynamic unconscious as well as the entire theory of repression, including the theory of slips, the dream theory, and Freud's theory of the aetiology of neuroses. For my criticisms of the Freudian evidence for the theory of the dynamic unconscious, see Erwin (2012).

A psychoanalyst could talk about a different theory of the dynamic unconscious, but if one is discussing Grünbaum's arguments, this would again be a change of subject. Both he and I were discussing Freud's theory, not some newer version: "One thing that should be clear, however, is that the subject is Freudian theory and therapy … not

later psychoanalytic theories, such as ego psychology, object relations theory, or self-psychology" (Erwin, 1996, pp. 7–8).

To summarise: neither Grünbaum nor I assume that there are two separate domains in the unconscious mind, the dynamic and the cognitive. We do hold that there are at least two different theories about the unconscious that need to be distinguished; both of us argue that cognitive studies of the so-called "cognitive unconscious" do not support Freud's theory of the dynamic unconscious; and both of us argue that the Freudian evidence, including the clinical and the experimental evidence (see Erwin, 1996 on the latter) also fails to provide any credible support for Freud's theory of the dynamic unconscious. Some of these points can be challenged by the Shevrin et al. studies referred to earlier, but, again, see the final section on Brakel's own theory.

Psychoanalytic theory as staple of Western culture

Slips

Brakel says (this volume, Chapter Three, pp. 62–63) that Grünbaum defines Freudian slips in an "idiosyncratic" way and that his arguments "seem to rest on Freud's own words". This is true, but Grünbaum is quite right to use Freud's own words; he is talking about *Freud's* theory of slips, not some commonsensical theory! Relative to Freud's theory, Grünbaum's account of a Freudian slip is hardly idiosyncratic. As Brakel says: He is using Freud's own words to say *what Freud* claims is a Freudian slip.

Even allowing for the fact that it is Freud's theory being discussed, what is the problem, Brakel asks, in Grünbaum's case of an alleged non-Freudian slip involving what she calls "the bosom-tracking man"? The man in the example is in a crowded party when he is confronted with a partial view of the breast of a woman he is speaking to and, feeling uncomfortable says, "Excuse me, I have to get a *breast* of *flesh* air". The man, Brakel claims, was almost certainly not aware of his motivation toward "pursuing the breast"; and if the pursuit occurred in a public setting, the motive would have been repressed. The basis for these certitudes is unclear. Perhaps Brakel failed to notice that this is not a real case. It is a made up case in which it is stipulated that the man did have a motive that influenced his slip, but it was not repressed; it was "transparent" to the person. Is Brakel claiming that such a case is logically impossible, that there is a contradiction in the very description

of it? If she is, she is clearly wrong. Yet if it is a possible case, then in this possible situation, there is no repressed slip. This is stipulated.

I am not sure, but Brakel may have misunderstood Grünbaum's point. He is not using the case to raise a problem for Freud's theory of slips, which, as Freud says, is not intended to cover all verbal slips. Grünbaum's point is merely that sometimes when someone makes a verbal slip involving sexual content, people interpret it as a Freudian slip whether or not it meets Freud's own criteria being explainable by his theory.

Brakel claims, however, that even in a slip that meets Freud's criteria there is a problem for Grünbaum's account. Although something needs to be repressed, it need not be a motive; as long as some psychological component is repressed, that is enough. She then makes up a case in which a man is aware of his desire to get a woman's breast and crashes into a glass sliding door. What was repressed, she asks? She replies that perhaps it was the intensity of his desire. I am not clear what is meant by repression of "the intensity of a desire". On Freud's theory, wishes and ideas can be repressed, but can the intensity of a desire be relegated to the unconscious?

Putting these issues aside, she can, of course, stipulate that in her case something has been repressed other than a motive. In the real world, however, we cannot just stipulate that repression occurs. We would need evidence of repression. To find it, it would not be sufficient to say, as Brakel does concerning Grünbaum's case, that there is something unacceptable and unpleasurable to the speaker. This would hardly be enough. We would also need empirical evidence that in most cases where a speaker finds something unpleasurable and unacceptable, repression occurs. As far as I know, there is no such evidence (see my discussion of repression in Erwin, 1996, Chapter Five and pp. 284–285).

Nagel's arguments for Freudian theory

The philosopher Thomas Nagel (1994a, 1994b) tries to defend Freudian theory in part by arguing that it is an extension of common sense psychology. He then appeals to an "understanding from within" principle to explain how common sense psychological causal hypotheses are typically confirmed.

Brakel challenges Grünbaum's critique of Nagel by arguing that her own theory about the core of psychoanalysis allows behaviour that

appears "bizarre" to be understood as regular and lawful, and in this sense ordinary. She thus claims to have rescued Nagel. Brakel is again just changing the subject. Nagel and Grünbaum were both discussing Freud's theory, not her theory, and what she means by "ordinary" has little to do with what Nagel means by common sense psychology. Nor does she even try to defend Nagel's "understanding from within" as a way of confirming either Freud's views or common sense psychology (for a critique of Nagel, see Erwin, 1996). There is no rescue here.

Freudian and post-Freudian psychoanalysis

Repression

Brakel takes Grünbaum to be trying to undermine the concept of repression by making the obvious point that many distressing experiences are remembered. She replies that repression of content is just one of the psychological mechanisms of defence Freud allowed (for a good discussion of the others, see Kline, 2002). Brakel's point is clearly correct, but it does not address Grünbaum's argument.

To begin with, Grünbaum is not trying to undermine the concept of repression if this means arguing that repression does not exist. What he is arguing is that a central assumption that Freud makes about the prevalence of repression is challenged if it turns out that the forgetting of distressing experiences is outnumbered by cases of remembering them. His argument runs as follows. A central tenet of the repression theory is that where there are negative-charged experiences and a forgetting of them, this is not just an accidental correlation. Freud is committed to a causal explanation of the forgetting. The forgetting occurs *because* of the negative affect. Now, suppose that the number of painful or forbidden thoughts or memories that are remembered is much greater than the number of those forgotten. This makes doubtful the idea that it is the negative affect that is generally responsible for the forgetting when this occurs. Given the ratios, which of course can be questioned, in most cases where negative affect is present, it does not cause forgetting. Given this evidence, it is reasonable to infer that generally when negative experiences are forgotten it is not *because* of their negativity. If the negative effect is not the key causal factor, then Freud and other psychoanalysts lack grounds for concluding that in cases of forgetting of unpleasant experiences, there is repression rather than mere forgetting.

This argument might be challenged, but more would have to be said than Freud allows for other defence mechanisms besides repression.

Brakel argues that Grünbaum is in error in assuming that if it can be shown that lifting repressions does not effect a cure, then it has been demonstrated that unconscious contents and conflicts do not have causal powers. This would indeed be a mistake, but not one that Grünbaum makes. He does hold that one argument Freud relied on, the Tally argument, would be undermined by the finding that lifting repressions generally does not bring about a cure, but undermining this one argument, as Grünbaum realises, does not rule out unconscious conflicts having causal powers.

A more modern understanding of psychoanalysis, Brakel claims, allows for the possibility that symptoms can persist even after their initial cause has been eradicated. Brakel is right, but Grünbaum is hardly unaware of this "more modern" claim. In Erwin (1978), I discuss the proposal made by some psychoanalysts to explain why symptom substitution often does not occur when symptoms are extinguished by the use of behaviour therapy. On this proposal (Rhoads & Feather, 1974), we need to distinguish between "ghost" and "non-ghost" symptoms. The former once served as an expression of an underlying psychic conflict that has now been resolved due to a maturation of the ego, but the symptom persists as a "learned habit", no longer serving a significant need. I argued that if this theory is true, there is a way to deal with the failure of symptom substitution, but at a cost. The standard Freudian explanation of the maintenance of most neurotic symptoms would have to be given up, as well as the Freudian rationale for criticising "superficial" therapies, such as behaviour therapy, on the grounds that they do not resolve unconscious conflicts that maintain symptoms. Grünbaum (1984, pp. 163–165) takes up the issue I raised and argues that there are additional costs for psychoanalysts who embrace the "ghost" symptom hypothesis. It is clearly not true that he ignores the more modern view.

Free association and psychoanalytic therapy

Brakel and Grünbaum agree, at least to some degree, about the therapeutic value of insight into unconscious conflicts gained through the use of free association. Merely knowing what has been repressed, Brakel agrees, has no curative power. She holds rather that both insight

and non-insight factors play a curative role in psychoanalysis, and she proposes that psychoanalysis works by providing, through multiple transference experiences, many instantiations of "safe here" and "safe now". There is a deep problem, however, with Brakel's attempt to explain how psychoanalysis works: there is no credible evidence that it does work, at all, for anything. See my review in Erwin (1996) and the more recent discussions in Erwin (2009a, 2009b). Without evidence for its effectiveness, there is nothing to explain here.

Brakel claims that Grünbaum does not have much to say about the capacity of free association to reveal, as opposed to curing. In fact, he has quite a lot to say about this issue, although not in the single paper she is referring to. Unless she has answers to the powerful arguments on the lack of free association's capacity to reveal unconscious conflicts (Grünbaum, 1984; Sand, 2011), she should give up her theory about free association or at least suspend judgement about its truth.

Brakel also says she need not be concerned about Grünbaum's rejection of inference to the best explanation because she is not claiming that any particular content revealed by free association is the only possible conflictual cause. The free associations and the content uncovered can lead to more than one possible candidate unconscious conflict. There appears to be a confusion here. Let it be true that a patient's free associating can lead to more than one possible candidate unconscious conflict, but how does the psychoanalyst know that even one conflictual cause has been revealed through free association? The analyst observes the patient free-associating and at some point concludes that some unconscious conflict has been revealed, but this is not something the analyst sees. An inference or posit is made, but on what basis?

Consider Brakel's case in which a patient mistakenly inserts "M.D." in a song and then free associates to this *parapraxis*, leading to the thought that his analyst is a doctor, and then to the thought that the analyst will soon be leaving for a vacation, and then to the idea that his father was a doctor and that his father often paid little attention to him. Now, he realises, Brakel claims, something he had been unaware of: that he has been quite angry at doctors, often without knowing his anger or its dangerous intensity. Given that the patient was previously unaware of his anger toward doctors, how does he know this now except by inference? It is not something he remembers. His conclusion must be inferred, but inferred from what? Brakel too makes an inference, as she must. She posits that his being angry at doctors figures centrally in a

causative unconscious conflict. She can posit this, but what is the basis for the posit?

In general, suppose that an analyst argues that her hypothesis about what the free-associating reveals explains the data available to her and that no other available explanation is as good an explanation; she then concludes on this basis that her conclusion is probably true. This is a common pattern of reasoning to be found in the psychoanalytic literature. Any analyst reasoning thusly is relying on inference to the best explanation, which as I demonstrated in Chapter Two (the present volume), is an invalid inference form.

Perhaps neither Brakel nor her patient were relying on an explanatory argument. If this were true, then her case would have no bearing on Grünbaum's point that *if* inference to the best explanation is relied on, the ensuing argument is no good. If it is not an explanatory argument that is being relied on at least implicitly, then, given her description of the case, it appears that both she and her patient were just speculating. This appearance could be shown to be deceptive by stating an argument, but none is given.

Transference

Brakel claims that Grünbaum relies for his criticism of the theory of transference on an outdated understanding of transference. He holds that on Freud's theory, the patient transfers to the analyst feelings or thoughts originally pertaining to some important figure in his or her earlier life. On a more modern and "accurate" [sic] understanding, according to Brakel, the transference reflects an attitude one feels in the present; one profoundly influenced by some similar past experience.

The section in which Grünbaum discusses transference is titled "Freud's etiologic transference argument". He is obviously discussing Freud's theory, not some non-Freudian modern theory. What is Freud's theory? Here is what Freud says about transference when discussing the patient's reaction to the analyst:

> On the contrary, the patient sees in him the return, the reincarnation, of some important figure out of his childhood or past, and consequently transfers on him feelings and reactions which undoubtedly applied to this prototype. The fact of transference soon proves to be a factor of undreamt importance … (Freud, 1940a, pp. 174–175)

Anyone asserting that transference can take place without the patient seeing in the analyst some important figure from his past either misunderstands Freud's theory of transference or has changed the subject.

The "modern" theory of the transference is really not so modern. As long ago as 1974, Laplanche and Pontalis (1974, p. 456) pointed out that Freudian theory requires postulation of the re-emergence of infantile prototypes, but that some analysts were using the concept of transference to refer to observable aspects of the analyst-patient relationship regardless of what caused them. There is nothing distinctively psychoanalytic about this newer theory. Even critics of psychoanalysis such as Grünbaum or the late Hans Eysenck can agree that "transference" occurs in this non-psychoanalytic sense, as when a patient in cognitive therapy develops feelings of dependence on the therapist and no psychoanalytic hypothesis is invoked to explain them. This non-Freudian and in fact non-psychoanalytic theory of transference was the subject of a paper by Lester Luborsky and his colleagues (1985) with the grandly misleading title "A verification of Freud's grandest clinical hypothesis: The transference". There was no such verification.

Instead of providing evidence for Freud's theory about the re-emergence of infantile prototypes, Luborsky et al. talk almost exclusively about observable behaviour patterns. There are two exceptions, but neither one has anything to do with Freud's theory of transference. One says that the patient is aware of some sexual impulses but not others; the second says the patterns of behaviour are "derived" from the patient's innate disposition and the influences brought to bear on him in his early years.

"Derived from" would most naturally be interpreted causally, but so interpreted, no evidence was provided for the second proposition. Luborsky et al., however, do not intend a causal claim (see Luborsky et al., 1985, p. 242). What is being claimed rather is that there are *similarities* between the patient's current attitudes toward the therapist and past attitudes toward his or her parents. There is nothing distinctively Freudian or psychoanalytic about this vague claim; as Eagle points out (1986b, p. 77), it is also not especially startling.

Brakel's account of transference, which she calls the modern and "more accurate" view, is the same as Luborsky's, and is rather trivial. In any sort of therapy, including cognitive therapy, rational emotive therapy, and behaviour therapy, patients are likely at some point to have certain attitudes or feelings influenced by some similar past experience.

Noticing that a patient is experiencing "transference" in this attenuated sense could be of some therapeutic interest in some cases, but that this is generally true remains to be shown. The phenomenon being referred to has nothing particularly to do with psychoanalysis.

Brakel's theory of mind

Brakel refers to her own theory as the "psychoanalytic general theory of mind". It consists of five propositions (pp. 5–8), which she calls "fundamental postulates" (2009, p. 4).

1. All of an agent's psychological events are regular/lawful in a particular psychological way for that agent; namely, every psychological event can be understood as psychologically meaningful to that individual (the psychic continuity assumption).
2. All psychological events have at least as one of their causes a psychological cause and can thereby be explained, at least in part, on a psychological basis (the psychic determinism assumption).
3. There exists a dynamic (psychologically meaningful) unconscious (the dynamic unconscious assumption).
4. Free associations function in a dual manner. First, they demonstrate apparent violations of psychic continuity and psychic determinism by revealing psychological events like *parapraxes* and symptoms that seem incoherent, with no psychological cause, and, second, they resolve the apparent violations in continuity and determinism by providing the psychological contents, which, when taken in conjunction with the assumption of a dynamic unconscious, can render what was seemingly inconsistent as quite continuous, now admitting of transparent psychological causation (the free association assumption).
5. There are primary and secondary processes that exist as two formally different types of mentation. Secondary process thinking is the largely rational, rule-following, ordinary logic of adults in the alert waking state. Primary process thinking is a-rational and associatively based (the primary and secondary process assumption).

Theses 1 and 2 would be challenged by anyone who believes, respectively, that psychological events are not lawful (there are, so far, no discovered laws in psychology, excluding physiological-based laws)

or that some psychological events have a purely neurological or other sort of organic cause. I will not discuss either thesis, however, except to note that neither of these very general propositions is distinctly psychoanalytic. They could both be consistently be accepted by someone who rejects all versions of psychoanalytic theory.

Although the terms "primary process" and "secondary process" appear in the writings of psychoanalysts, thesis 5 is true as long as the thinking of some people is sometimes logical and rational, and sometimes is a-rational and associatively based. The proposition is plainly true, but is also plainly not distinctively psychoanalytic. Propositions 3 and 4 are psychoanalytic, at least if by a "dynamic unconscious" it is meant the sort of unconscious postulated by Freud and widely discussed in the psychoanalytic literature.

What evidence does Brakel cite in her book for her assumptions? None. She says (p. 4) concerning all five of her basic assumptions that they are "taken for granted". I suppose that Freud or B. F. Skinner instead of arguing for their theories could have said: "These are basic assumptions which are taken for granted". The rest of us, however, can reasonably say: yes, but are any of these propositions evidence-based?

Brakel does cite evidence in her paper, in particular for unconscious conflicts and their causal efficacy. She relies on an early paper (Shevrin et al., 1992), a book (Shevrin et al., 1996), and an updated version of the early research (Shevrin, 2012). These are the studies I alluded to earlier. In the 1992 study, eight patients suffering from phobias and three from pathological grief were interviewed and tested using the WAIS-R, Rorschach, and thematic apperception test. The interviews and test protocols were audio-recorded and transcribed. A clinical team of three psychoanalysts and one dynamically oriented psychologist then examined the transcripts.

Each clinician was instructed to arrive at a psychodynamic formulation made up of three parts: (a) the patient's description of her symptom or complaint; (b) the patient's understanding of the complaint; and (c) the underlying conscious conflict causing the complaint. The clinicians then selected two groups of words: C-words capturing the patient's conscious experience of the symptom-complaint and U-words reflecting the unconscious conflict causing the symptom-complaint.

The clinicians then met and used the Delphi method to reach agreement about what C-words and U-words were to be used in the experiment. The agreed upon words were then presented subliminally and

supraliminally to the subjects. An analysis of their brain patterns, an ERP analysis, revealed that the subjects' ERPs classified the U-words better subliminally than supraliminally, while the reverse was true for C-words. The experiment and the statistical analysis appear to be technically sound, but what of the foundation of the experiment? The clinicians could confirm (a) and (b) empirically just by listening to how the patient described her complaint and her understanding of it. But what about (c)? This is crucial; for without knowing the underlying unconscious conflict causing the symptoms, there was no way of knowing whether or not the selected U-words reflected the unconscious cause of the symptoms. Yet, no evidence was presented for any of the patients that the clinician's judgement about the cause of the symptoms (or complaint) was true.

Instead, the experimenters relied on nothing more than the clinician's unsupported opinions. The rationale given is that they were experts, but without empirical evidence that they are generally right in identifying unconscious conflicts that cause the emergence of symptoms, calling them "experts" adds nothing of evidential value. There was simply no evidence for any of the clinicians' claims of an unconscious conflict causing a symptom or complaint. Without such evidence, there was no evidential basis for dividing up the words into C-words and U-words.

Here is what the authors of the 1992 report (Shevrin et al., 1992) say about this crucial objection:

> An objection can be raised regarding whether the specific unconscious conflict identified by the clinicians is in fact *the cause* of the symptom. We may have identified *an* unconscious conflict, but our results do not necessarily prove a causal connection. This is a sound objection. (p. 356)

It is a sound objection, but the situation is even worse than conceded by the authors. It is not just evidence of a causal connection that is missing. No evidence was provided for an unconscious conflict either. Here, too, the experimenters relied solely on the opinions of the clinicians. The experimenters' later work, Shevrin et al. (1996) and Shevrin (2012), also fail to overcome these problems. The 1996 book relies on the original experiment, but fails to provide evidence of causation of unconscious conflicts or even that there were unconscious conflicts (see pp. 133–143).

As reported in *Science Daily*, 2012, Shevrin argues in his 2012 paper that he has now overcome a problem with his first experiment: the failure to allow for directly comparing the effect of the unconscious conflict stimuli on the conscious symptom stimuli. In the new experiment, the unconscious conflict stimuli were presented immediately prior to the conscious stimuli.

What was the nature of both sorts of stimuli? As before, it was the words representing, respectively, the subject's conscious experiences and the unconscious conflicts causing the symptoms, words that were presented to the subjects subliminally and supraliminally. Once again, however, no evidence was provided, at least based on the *Science Daily* report, that what the clinicians believed to be the unconscious conflicts causing the symptoms really were the unconscious causes. Instead, the opinions of the clinicians were relied on. According to the report, the psychoanalysts *inferred* what the unconscious conflicts causing the anxiety disorders might be. Without some sound basis for the inference, this is no better epistemically then merely giving their opinions.

Even if, contrary to fact, it had been established that there were unconscious conflicts and that they had caused the symptoms or complaint, what was the evidence that the selected U-words reflected these alleged unconscious causes? Once again, it was the opinions of the psychoanalysts. For arguments that Brakel's free association and dynamic unconscious assumptions are not supported by credible evidence, see Grünbaum (1984, especially Part II) on free association and Erwin, 2012 (as well as Erwin, 1996, Chapter Five).

Talvitie's comments: the scientific status of psychoanalysis

Talvitie claims in this volume that the Freud war debates about the scientific status of psychoanalysis have stagnated. His suggestions for improvement include using criteria of science from the domain of psychoanalysis as opposed to some other discipline, such as physics or linguistics. He also raises a threefold issue about psychoanalysis' scientific standing. We should ask, Talvitie suggests, not only about psychoanalytic theories and models, but also ask about the scientific status of the psychoanalytic community and psychoanalytic professional practices.

Talvitie does not explain what he means by the "scientific status of psychoanalytic theory", but given his comments about relativising criteria of the scientific to particular domains, he appears to be addressing

the "demarcation problem". Philosophers of science, such as Karl Popper, Frank Cioffi, and others, had this in mind in their discussions of the scientific standing of Freudian theory, but those who have spoken about the "Freud wars" generally were referring not to this issue, but to debates about the Freudian evidence.

To see the difference, in case it is not immediately obvious, the Popperians could hardly complain about the lack of empirical support for Freud's theory without directing the same complaint against general relativity theory and Darwinism. On Popper's theory, all theories have zero empirical support; so, if he wanted to distinguish Freud's theory from Einstein's, he needed a different criterion besides lack of empirical support. His suggestion was that theories be discussed in terms of the demarcation problem: how do we distinguish scientific theories from pseudo-scientific theories? Popper claimed to have solved the problem with his falsifiablity criterion. A theory that is logically impossible to falsify—it implies no propositions that are potential falsifiers—is pseudo-scientific; otherwise, it is not. On this issue there has been no stagnation. The Freudians won; the Popperians lost.

Popper was never able to prove that Freudian theory was unfalsifiable in principle and neither has anyone else been able to demonstrate this. Sometimes the Popperians appeared to have made progress on this point, but only because they conflated a logical point with a sociological one. As two Popperian psychiatrists put it in their paper "Is Freudian psychoanalytic theory really falsifiable?": "One reason why a theory might not be falsifiable is that it conflicts with no conceivable observations. Another more common reason is that the theory's proponents refuse to acknowledge falsifications as such" (Notturno & McHugh, 1986, p. 250). If a theory when combined with auxiliary assumptions logically entails an observational prediction that is proven false, then the theory or one of the auxiliary assumptions is false. This is true as a matter of logic. When the auxiliary assumptions have empirical support, the theory has been falsified; if a proponent refuses to acknowledge the falsification, this is a purely sociological point. It has no bearing on whether the theory has been falsified. Popper, in fact, never showed that any theory at all was pseudo-scientific on his criterion. For an argument that all theories are in principle falsifiable, see my paper on the confirmation machine (Erwin, 1970).

One of Popper's followers, Frank Cioffi, tried a different approach. He appealed to his deceptive procedures criterion (1970), but the

criterion applies to what Cioffi called “enterprises”, not to theories. No matter how he tried over a period of many years, he was never able to bridge the logical gap between talk of enterprises and talk of theories (Erwin, 2000).

The attempt of Cioffi, Popper and others to discredit Freud by showing his theory to be pseudo-scientific was a lazy man’s approach. Instead of answering the clinical arguments of the Freudians or discrediting the 1500 or so Freudian experimental studies, the Popperians appealed to purely a priori arguments to discredit a contingent and anempirical theory. It should not have been surprising that the attempt ended in failure.

As to the question about the Freudian evidence, there has been no stagnation here either. For the most important arguments discrediting the Freudian clinical evidence and some of the experimental evidence, see Grünbaum (1984, as well as his later papers, including the one in this volume). For additional arguments about the weakness of the clinical evidence, plus a discrediting of the Freudian experimental evidence and the evidence for the therapy, see Erwin (1996, 2009a, 2009b).

Talvitie claims that Grünbaum and I argue that certain psychoanalytic theories or parts of them are false. This is true but potentially misleading. What we have mainly argued is that the Freudian evidence is very weak or non-existent. See Grünbaum’s three major conclusions (1984, p. 278) and my final verdict (Erwin, 1997, p. 296). The main exception to the restriction to a purely sceptical verdict is Freudian dream theory.

The metaphysical foundation

Talvitie claims (this volume, p. 87) that given the numerous psychoanalytic models, there is danger of reaching a dead end when aiming to say something about the scientific status of psychoanalytic theories in general, but the situation is not hopeless. We can at least talk about the shared metaphysical basis of these theories. Why is it hopeless to talk about the scientific status of multiple psychoanalytic theories? If we are asking about their being falsifiable, they all are; and there is no basis other than their being impossible to falsify for classifying any of them as “pseudo-scientific” in any sense that matters. But this issue is pretty much a dead issue anyway, at least in philosophy of science. If we are asking if any of them are true, or at least approximately true,

then we cannot avoid the evidential issue. Which if any are supported by credible empirical evidence? Some psychoanalysts have discussed the empirical status of some of the main ones, such as object relations theory, self-psychology, and ego psychology (Eagle, 1984), and there is no reason why they cannot all be discussed.

Compare the situation with respect to behaviour therapy. People outside the field of behaviour therapy often make general claims about this sort of therapy, ignoring the fact there are more than 100 different types of behaviour therapy. The result of such generalising is often superficiality or falsehood, but there is a way to avoid both without resorting to metaphysics. If the subject warrants it, discuss each kind separately.

As to Talvitie's proposed metaphysical foundations for psychoanalytic theories, I have two questions. Is the main principle that he proposes—that the mind has an unconscious part—really a metaphysical principle, and is it distinctively psychoanalytic? As to the first question, in the nineteenth century, various scholars, such as Helmholtz and Eduard von Hartmann, argued on empirical grounds that there are unconscious mental events and processes. Their arguments were challenged by William James and Franz Brentano, but also on empirical grounds. This was an empirical dispute, not a dispute about metaphysics.

One could take the position that no one in the twentieth century has ever provided empirical evidence for unconscious mentality, but this point is difficult to defend given the experimental work on the cognitive unconscious. If the thesis of unconscious mentality has been established empirically, why call it metaphysical? The thesis is very general but so is another one shared by most psychoanalytic theories—that repression exists and plays a causal role in explaining various psychological phenomena. Despite their generality, both are empirical theses. Perhaps the question of whether to classify a thesis as metaphysical or not is not very interesting, but this takes me to the second issue. The unconscious mentality thesis, in contrast to Freud's theory of the dynamic unconscious, is clearly not distinctively psychoanalytic. The psychologists and philosophers who defended the thesis before psychoanalysis was born were not psychoanalysts, and neither are such critics of psychoanalysis who also accept the thesis, including Adolf Grünbaum and Hans Eysenck.

Talvitie concludes that the existence of mind—he is not talking about the unconscious—has not been proven by the methods of natural science, that it falls in the domain of metaphysics, and it is an

unavoidable presupposition in most human activities. If he means the existence of a mental entity, the mind, a thing that in principle can survive the destruction of the body, then I agree that its existence has not be proven either empirically or by metaphysical arguments (Erwin, 1997, pp. 48–59). But it is not true that the existence of such an entity is an unavoidable presupposition in any human activity. Talk of mental events and processes are sufficient; no commitment to a mental entity is needed. Some psychoanalysts, including self-psychologists, talk of the self as a mental entity, but this too has no warrant and is not needed. Speaking of a virtual self will suffice (Erwin, 1997, pp. 57–59).

Perhaps Talvitie is not talking about a mental entity. He may be claiming that the existence of mental phenomena has never been established by natural science methods, but he gives no argument for this claim, and, if this were his claim, he would be ignoring much experimental evidence in psychology that helped defeat behaviourism; see, for example, the disassociative design experiments (Erwin, 1978, pp. 109–113).

Modern day psychoanalysis

Talvitie (Talvitie, this volume, p. 82) claims that Grünbaum and I restrict our studies to Freud's views partly because they are an easy target, or, at least, he "cannot avoid thinking" this. Perhaps he has trouble thinking of a different reason, but does he have any evidence for this thought? He does not.

Freud was one of the most influential thinkers of the twentieth century. As late as 1993, Friman et al. found that Freud's books and papers were being cited more than any other thinker in the history of humankind except for four other people: Aristotle, Plato, Shakespeare, and Lenin; far more than Chomsky and Skinner, and certainly far more that the minor figures in object relations theory or self-psychology. To suggest that in 1984 no one should have devoted a book to Freud's works, as Grünbaum did, is absurd; Freud was still very influential in many fields, including clinical psychology where Freud-inspired psychoanalytically oriented psychotherapies were widely used. It was also a time when many of his arguments had not been satisfactorily answered. There is no reason to assume that Grünbaum was looking for an easy target.

As to my own book (Erwin, 1996), numerous philosophers and psychologists had raised issues about the proper evidential standards for

judging Freudian psychology and about the alleged strength of the many Freudian experimental studies. Is it likely that there was no reason to write a book about these issues except to search for an easy target? Perhaps I should have taken on more formidable foes such as B. F. Skinner or Hans Eysenck—but I did (Erwin, 1978, 1988); or more challenging theoretical or therapeutic arguments, such as those given by Smith, Glass, and Miller (1980) or the cognitive therapist Aaron Beck, or Albert Ellis and the rational emotive therapists, or neuroscience arguments for Freud's theory, but I have done all of these things too (Erwin, 1997, 2009b).

Talvitie also claims that the Grünbaum battles do not necessarily have a lot to do with psychoanalytic therapy as practised by my present-day clinicians. He is wrong about this. Grünbaum's (1984) book is mainly about Freud's theory, but what he has to say about the need for a placebo control and his theory of the placebo (1985) is relevant to all modern psychoanalytic therapies.

My arguments in Erwin (1997, Chapter Six) apply to all contemporary versions of long-term psychoanalytic therapy, not just Freud's version. Some psychoanalysts have tried to get around the difficulty of doing randomised clinical trials with long-term psychoanalysis by adopting Seligman's (1996) consumer reports model. This was tried in the Freedman, Hoffenberg, Vorus, and Frosch study (1999) and in the well-regarded Stockholm outcome project (Sandell et al., 2000). I show (Erwin, 2009a) that neither of these studies provides any evidence of effectiveness, and that this way of avoiding the need for prospective outcome studies will not work.

Sometimes when people speak of psychoanalysis they confuse it with something quite different: psychoanalytically oriented psychotherapy. There are many therapies of the latter sort, by some count more than thirty. What they have to do with psychoanalysis ranges from a lot to no more than being in some vague way or other Freudian inspired. It is unclear, at least to me, that what they have in common is significant enough that they should be grouped together as a class of therapies. Nevertheless, what I argue in Erwin (1997, Chapter Seven) is directly relevant to all of these therapies.

There is another important point that has been continually overlooked by those who say that the arguments of Grünbaum and myself are relevant to Freud's work but not to modern versions of psychoanalysis. The epistemological problems raised for the Freudian evidence,

for the most part, arise for newer psychoanalytic theories and therapies. Start with the psychoanalytic therapies. I argued in (Erwin, 1996) that there was no evidence for the effectiveness of (long-term) psychoanalysis. Did I overlook some credible evidence? In 1999, the research committee of the International Psychoanalytic Association prepared a comprehensive view of the data concerning the effectiveness of psychoanalysis. What was their verdict?—The same as mine. They concluded that existing studies have failed to demonstrate that psychoanalysis is effective relative to an active placebo or to alternative treatments (Erwin, 2009b).

Has the situation changed materially since 1999? Where are the randomised clinical trials with a placebo control, double-blind controls, and validated outcome measures for any of the newer psychoanalytic therapies? For long-term psychoanalytic therapies, there are none, and very few, if any, for short-term psychoanalytic therapies. The problems I raised in Erwin (1996, 1997, 2009a, 2009b) apply to virtually all of the new psychoanalytic treatments, and they have not yet been overcome.

With respect to the newer psychoanalytic theories; there are many of these. Linda Brakel's theory is one; see my earlier comments on her theory. For obvious reasons, I will not try to review the evidential basis for all of these theories, but some general questions can be asked. If one reads the pages of the leading psychoanalytic journals, including the *International Journal of Psychoanalysis* and the *Journal of the American Psychoanalytic Association*, does one find reports of confirmation by experimental evidence? Very rarely, if at all. The evidence comes mainly from clinical case studies or sometimes from observational studies of a different kind. One leading analyst complained in 1998 that there is a persistent belief among analysts that "… conducting psychoanalysis in one's consulting room is a form of research, an attitude still reflected in the bulk of articles published in psychoanalytic journals" (Compton, 1998, p. 693). This attitude is still reflected in the leading psychoanalytic journals of today.

In so far as newer psychoanalytic theories are based mainly on clinical case studies, Grünbaum's (1984) critique applies to their evidence just as much as to the Freudian evidence. How do the newer theorists overcome the suggestibility problems, especially those raised by the verbal learning studies? (Grünbaum, 1984, Chapter Two; Erwin, 1996, pp. 95–106) Suppose that the suggestibility problems could be overcome within the confines of the clinical case study—are the new

psychoanalysts able to rule out all credible rivals to their theory without doing experiments? If they cannot, then they run afoul of the differential principle (Erwin & Siegel, 1989) and have no confirming evidence. Do their theories postulate unobservables, such as repression, unconscious motives, or unconscious conflicts? If they do, how do they obtain evidence that any of these things even exist? They cannot see them and their patients cannot discover them through introspection. An inference must be made, but what is the basis for the inference? If the argument is typical of what is found in the psychoanalytic literature—"We have considered all of the plausible theories we know of that might explain the phenomena; and ruled out all but one; and thus are inferring the likelihood of the remaining one, the best available explanation"—then they are relying on an invalid rule of causal inference, inference to the best explanation (see my critique of this rule in Chapter Two, this volume). See Grünbaum (1984, Part II) for further problems that arise for most non-Freudian psychoanalytic theories.

Causation

Talvitie contends that Grünbaum and I base our critique of Freud on the thought that every psychic disorder is caused by a similar cause and that there is a single cause for each disorder. Talvitie concludes that it is "a form of scientific mutilation to lean on a view that psychoanalysis could reveal *the* cause behind psychic disorders". Talvitie misinterprets what Grünbaum and I are saying. In psychiatry, clinical psychology, and neuroscience, virtually everyone writing in any of these fields recognises that when someone speaks of *a* cause of an event or *the* cause of an event, they are generally not talking about causally sufficient factors. They are not talking about an event that by itself is sufficient to bring change. Rather, they are discussing causally relevant factors, which means that there are other contributing causal factors needed to bring about the event; some may be psychological, but others may be physiological, genetic, environmental, or neurological. But there is no need to point out every time we speak of the cause of someone becoming clinically depressed or becoming less depressed that we are talking, of course, about a causally relevant factor, perhaps the most important one, perhaps not, but not *the* cause.

In the Shevrin et al. paper (1992) discussed earlier, the authors (p. 356) consider the objection that it has not been shown that an

unconscious conflict "… is in fact *the cause*" of the symptom (their italics), but the objection is not that there is no single cause; it is rather that there is no causal connection at all between conflict and symptom. Should the authors be faulted for suggesting that there is something that is *the cause* of the symptom? Of course not. Elsewhere they make clear that they are talking about contributing causal factors, not a single cause.

Grünbaum and I have repeatedly made clear that we are talking about causally relevant factors. Both of us have discussed different versions of a rule for identifying such factors (Erwin, 2009b; Grünbaum, 1990). Neither of us has ever discussed a rule for identifying *the* psychological cause of an event, for the obvious reason that neither of us believes that there is such a thing. Even if we occasionally speak of a cause or the cause, we clearly mean a causally relevant factor. There is no argument that either of us gives that presupposes *the cause* of anything.

What Freud believed is another matter. Yet he too, even when he spoke of repressed wishes as being the cause of neuroses and slips, never meant that there were no other causal factors in play. As Grünbaum points out (1984), Freud always assumed that in addition to the important psychological factors there are what he called "constitutional factors" that combined with the psychological ones to make a causal difference.

Freud, in fact, made an important contribution to the discussion of causally relevant factors, which is rarely taken into account by philosophers, psychiatrists, or psychologists. In his reply to Löwenfield he notes that there are multiple causes of psychological problems and he gives a very interesting analysis of different types of causally relevant factors, including preconditions, specific causes, concurrent causes, and precipitating causes (Freud, 1895f; see also Grünbaum (1984, pp. 167–168) for a discussion).

CHAPTER SIX

Two fundamental problems for philosophical psychoanalysis*

Linda A. W. Brakel

Introduction

This chapter consists of two parts, quite different from one another except in one important respect. They each address a foundational challenge to psychoanalysis—not so much psychoanalysis as a mode of treatment, but to psychoanalysis as a research method and as a general theory of mind.[1] Describing the second part of this chapter first, I will mount what I call "An argument for the very possibility of meaningful a-rational mentation". The problem, as is evident from this title, is that there are those who claim that only the rational (and conscious) can be representational, meaningful, and contentful.[2] If this were to be true, no systematic psychological understanding of non-rational

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* This chapter is a revised edition of three works by Linda A. W. Brakel. "Phantasy and wish: A proper function account for human a-rational primary process mediated mentation" (2002), *Australian Journal of Philosophy, 80, 1*: 1–16. Kind permission has been granted by Taylor & Francis Ltd for its use. *Philosophy, Psychoanalysis and the A-rational Mind* (2009), pp. 5–8; 63–83 and *Unconscious Knowing and Other Essays in Psycho-Philosophical Analysis* (2010), pp. 154–156. Kind permission for use of these two works has been granted by Oxford University Press.

mentation, including both a-rational and irrational instances, would be possible—even at the level of consciousness, much less when such mentation is unconscious. A satisfactory account of symptoms, then—including phobias, other anxiety disorders, depression, obsessive compulsive behaviours, as well as the milder problems such as slips of the tongue and physical parapraxes (mistakes in action)—would have to rely on some mixture of neurochemical imbalance, neuroanatomical regulation problem and/or some accidental occurrence at either the physiological or psychological level. But these issues are for the second part of the chapter. So, I will now briefly introduce the first part of this chapter, which concerns a very different, but no less essential, matter.

This first section titled "An argument for the standing of psychoanalysis as a regular science" holds that psychoanalysis as a research method and general theory of mind can be best understood as a regular science. Thus, without denying psychoanalysis its very valuable unique focus on first-person agency and subjective experience,[3] it is best and most properly considered a regular science rather than either a humanistic non-scientific enterprise or a science sui generis—that is, a "science-like" entity. The development of this view and then its advantages will be presented forthwith.

PART ONE

An argument for the standing of psychoanalysis as a regular science

Freud held the position that psychoanalysis is a natural science. But even (perhaps especially) in his day this was hotly contested. Freud (1925e, p. 217) lamented, for example, that, concerning psychoanalysis, most of his medical/scientific colleagues "… regard it as a speculative system and refuse to believe that, like every other natural science, it is based on a patient and tireless elaboration of facts from the world of perception." Now, of course a thinker can certainly be wrong about the nature of his or her own contributions. Further, just accepting Freud's characterisation of his work would be the lowest form of argumentation—if it is an argument at all—a mere appeal to authority—and therefore totally unacceptable. Thus, I offer Freud's view solely

as a point of interest to accompany the far better case to be made for psychoanalysis as a regular science, which follows just below.

The argument begins with the acknowledgement of an initial assumption: that all scientific theories, methods and techniques have a discrete number of basic underlying and necessary presuppositions. Then, I will present my account of the five foundational precepts that comprise the basic presuppositions of psychoanalysis. Part One of the chapter will close with two questions: 1) Can the psychoanalytic general theory of mind properly be considered a scientific theory? 2) If yes, how good a scientific theory is it?

That all truly scientific methods, techniques and theories have underlying basic assumptions is an inheritance they share from the scientific method itself. Progenitor to all the sciences, the scientific method has its own overarching necessary assumptions, carried over to all its progeny, along with the standards without which no science could accurately and reliably observe, measure, collect, and then compare data sets, supplying and then evaluating hypotheses. Among the most fundamental assumptions are those about our human interactions with reality. For example, it is presumed that significant aspects of reality can be understood through rational, causal (and/or correlational) explanation; this related to the even more basic assumption that observable parts of reality are continuous, consistent, and objectively perceivable by human observers.[4]

The assumptions of psychoanalysis have been described differently by different authors. For many (me among them) there is an important distinction between psychoanalysis as a general theory of mind, and psychoanalysis as a clinical theory. The basic and necessary assumptions I will set out pertain to the general theory only. Moreover, since I hold that the clinical theory is derivable from the general theory,[5] the foundational assumptions must be located here and here only. Although not all who write (or have written on this subject) are in agreement even on this most primary notion,[6] there is general consent about the following: no matter what presuppositions are regarded as foundational, each agrees that these are assumptions that are "… 1) taken for granted, 2) used to derive other psychoanalytic propositions and concepts, and when functioning 3) generate psychoanalytic data … [where] psychoanalytic data most broadly include everything reported and taking

place in psychoanalytic sessions—e.g., dream contents; psychological symptoms …; [changes in] … mood; sexual concerns; slips of the tongue and parapraxes …; phantasies and daydreams; and reports of strong feelings about past persons and persons in the present, including the analyst" (Brakel, 2009, p. 4).

The five fundamental presuppositions

On my account there are five fundamental presuppositions comprising the core of psychoanalysis as a scientific general theory of mind. Of the five, there are three assumptions, one methodological tool, and one corollary. I have described them all at length previously (Brakel, 2009, pp. 5–8) and since my views do remain (and I have not found a better way to present them), I will quote and paraphrase extensively from that work in what follows about these five foundational postulates.

> The first two assumptions, psychic (psychological) continuity and psychic (psychological) determinism are best considered together. Psychic continuity and psychic determinism are [the] psychology-specific versions of two general assumptions that are in place given any scientific theory [discussed above]. Continuity presumes some sort of regularity or lawfulness in the phenomena under study,[7] and determinism means simply that the operation of cause and effect is presumed. To assume psychic continuity then is to take for granted that all of any agent's psychological events—including those that look inconsistent or even incoherent, such as pathological symptoms (for instance phobias to water, benign animals, or of open places), slips of the tongue, and parapraxes—are regular/lawful in a particular psychological way for that agent; namely that every psychological event can be understood as psychologically meaningful [in a subjective, first-personal way] to that individual. Similarly, to assume psychic determinism is to presume that all psychological events—even those that look incoherent—have at least as one of their causes, a psychological cause, and can thereby be explained (at least in part) on a psychological basis. So for example a dream element, a delusion, a confabulation all can be presumed to be necessarily regular and lawful phenomena with physical and psychological causes such that these phenomena cannot fail to be psychologically meaningful to the dreaming, delusional or confabulating agent. Put another way, there is no dream element, delusion, or confabulation, in fact no psychological

> content possible for a particular person that will not have been caused by some aspect of that person's psychology and that will not thereby be meaningful to that person. For instance, suppose I have a physiologic derangement that causes me to have delusions. The content of each particular delusion I have must be psychologically caused and psychologically meaningful to me. Suppose now that you have the same physiologic condition and even the same delusional content. Still the psychological cause and psychological meaning of that identical content for you will uniquely be a function of your psychological continuity and determination, not mine. (Brakel, 2009, pp. 5–6)

"The third assumption of psychoanalysis is that there exists a dynamic (psychologically meaningful) unconscious. It is posited because without such a postulate many psychological events 'seem' [gappy, and] neither psychically continuous nor determined" (Brakel, 2009, p. 6). Freud (1915e, p. 166) puts it this way: "Our right to assume the existence of something mental that is unconscious … for the purpose of scientific work is … *necessary* and *legitimate* … it is *necessary* [Freud's italics] because the data of consciousness have a very large number of gaps …; both in healthy and sick people psychical acts often occur, which can be explained only by presupposing other acts, of which, nevertheless, consciousness affords no evidence." Returning now to my 2009 account, p. 6:

> Take a very common sort of slip of the tongue I experienced. I was in the backyard calling my husband whom I saw to be occupied with some yard activity. He did not respond, a fact that did not surprise me as much as the realization that I was using our dog's name to call him, not my husband's. I laughed. Now the names Art and Jet are not dissimilar—and that was a physical cause of the slip; but why would I (a neurologically intact person) confuse my husband's name with that of our dog? This slip of the tongue seems neither psychologically lawful nor determined until we posit a dynamic unconscious [meaningful] cause of this commonplace psychological event. My husband, whom I love, seemed to me as unresponsive as our dog, a canine whom I also love. I was unconsciously angry with him; and unconsciously, but ambivalently I insulted him—you are no better than our (wonderful, albeit very disobedient) dog. As with this simple example, the assumption of interceding unconscious processes and contents allows psychological

> determinism and continuity to be evident generally, even in those psychological events (such as neurotic symptoms, dream elements, delusions, and hallucinations) seemingly inconsistent to the point of frank bizarreness.

Before proceeding to the last two of the five presuppositions, let us take stock of where we are if we endorse the first three, particularly as opposed to where we would be if we did not. Previewing a contrasting account that I will take up in Part Two of this chapter, Donald Davidson (1967, 1970, 1973a, 1974a, 1974b), with his principle of charity, would have a quite different analysis of my "Jet/Art" slip of the tongue. Briefly, the principle of charity proclaims that in the event of a confused communication from a fellow human being, we should disambiguate the confusion in favour of presuming the rationality of the communicator. Thus, charitably presuming my rationality, my calling my husband by my dog's name would simply represent (beyond the fact that the words themselves are not dissimilar) a mistake—a psychological or physiological accident, or a one-off, non-meaningful event; at most reflecting distraction, if both dog and husband were present in the backyard at the time of my misnaming. But much would be lost in this more superficial explanation. The unconscious anger and ambivalence, later made conscious through the understanding of the slip of the tongue, would have remained underground. And, since in this case I myself was the agent with the parapraxis, I am in a position to attest that those emotions were real.

Another example might help demonstrate this point. One day my husband and I were walking along in Ann Arbor. A young man was walking in the other direction on our right. The man looked familiar, but I couldn't quite place him. I asked my husband, "Who is that man who just past to the right?" My husband answered, "I didn't see anyone" and started to hum a Simon and Garfunkel song called "Frank Lloyd Wright", in particular a verse in which the words were "architects may come and architects may go …" It then immediately occurred to me that the young man had been our architect's assistant. I told my husband, who still said he'd seen no one. When I asked him why the song had occurred to him just then, he answered very reasonably, noting that on our left we had just past the restaurant called "Frank's". On Davidson's principle of charity, Frank's restaurant is the entire explanation. And

it is an entirely rational and conscious explanation. On the account that posits meaningful unconscious mentation (including unconscious knowing) as per the assumption of a dynamic meaningful unconscious, we get not only Frank's, the restaurant, but the unseen but unconsciously known young architect. Further, there may have been non-rational reasons for my husband's negative hallucination[8] that Davidson's charity account would entirely miss.

The concept of a more complete explanation leads to another important philosophy of science precept. Indeed, whereas given the same explanatory effectiveness, the simplest explanation is to be preferred—and Davidson's charity principle may be simpler—it is the simplest explanation that best explains the most data. On this basis, even with non-clinical everyday examples such as those I've offered here, it seems evident that the explanation from psychoanalytic general theory, which includes the assumption of meaningful unconscious mentation, does far more explanatory work.

Resuming now with the fourth fundament, I return to Brakel (2009, p. 6):

> The one methodological tool necessary for psychoanalysis is free association. Free association as part of the foundational core of psychoanalytic general theory has an interesting status in that it functions in a dual manner. First, free associations demonstrate apparent violations of psychic continuity and psychic determinism by revealing psychological events like *parapraxes* and symptoms that seem incoherent [and/or gappy] with no meaningful psychological cause. Second, free associations resolve the apparent violations in continuity and determinism by providing the psychological contents, which when taken in conjunction with the assumption of a dynamic [meaningful] unconscious, can render what was seemingly inconsistent [and gappy] as quite continuous, now admitting of transparent psychological causation.
>
> Here is an example of the dual functioning of free association. A patient in analysis expressed anger at me for 'going on vacation so much and 'cancering' her sessions. Her slip of the tongue occurred as she freely associated; this speech *parapraxis* was a psychological event that seemed discontinuous. But, by assuming psychic continuity, psychic determinism, and a [meaningful] dynamic unconscious, we can hold that there was a psychologically relevant set of

unconscious contents causing this particular slip, and further, that if the patient could continue to free associate, including associating to her slip of the tongue, its causal contents could be revealed. My patient did associate to her parapraxis: she talked, as she often had, of fearing that I would go off, have a baby, and abandon her. She had never had a thought about my having cancer. She continued that her mother had been sick during her pregnancy with her younger sibling and therefore not very attentive to her; and that after he was born it was even worse, she really felt abandoned. She thought she probably hated her newborn brother and her mother. These free associations both revealed the unconscious contents that likely caused the seemingly discontinuous event, and then restored their continuity. The patient feared something inside me would grow and take me away from her. Consciously, whenever I went on vacation she worried that the past would be repeated—that I would leave her by becoming pregnant and she'd feel abandoned. Unconsciously, whenever I went on vacation she became so angry with me for leaving her that she wished me to be sick with a growing cancer instead of a growing baby. (Brakel, 2009, pp. 6–7)

For the fifth presupposition, I quote once again from Brakel (2009, pp. 7–8):

> The final element comprising the foundational structure of psychoanalytic general theory—the positing primary and secondary processes as two formally different types of mentation—is best described as a corollary to the other fundaments. The corollary status obtains because positing primary and secondary processes follows from and is demonstrated by the application of the three basic assumptions and free association.[9] The primary and secondary processes operating according to different principles can, for example, be appreciated in the examples above. Secondary process thinking is the largely rational, rule-following, ordinary logic of adults in the alert, waking state. Primary process thinking, in contrast, is a-rational[10] and associatively based. When the secondary processes predominate, psychological events look continuous, caused, explainable, and rational. Thus, if I'd been thinking at a secondary process level I would have called out my husband's name since I had wanted him to respond. Similarly, if my patient's

> functioning had been predominately secondary process, she might have directly experienced her anger at me for taking a vacation in the midst of her psychoanalytic work and even recognized that the strength of her feeling abandoned owed to her distant past. Primary process thinking, on the other hand, is clearly not rational. Instead it is a-rational. It is the type of mentation operative when I categorized my husband-who-does-not-respond with our disobedient dog, and displaced his name with hers. Likewise when my patient spoke of my 'cancering' the sessions, she demonstrated a primary process type of categorization. Tumors and babies are both things that grow inside and take mother figures away. Because my leaving on vacation felt like abandonment, she was angry and displaced the baby she feared I would have with a tumor she wished I would have.
>
> Primary process thinking is a-rational thinking. Its hallmarks, in addition to the absence of ordinary rationality, displacement, and the type of feature-based categorizing by resemblance shown above, include condensations (combining thoughts together that by ordinary logic do not belong together), categorizations by contiguity in time and/or space, and substitutions of part for whole. Dream elements perhaps provide the most obvious, plentiful and accessible examples of primary process contents demonstrating the effects of the operation (in all combinations) of these many a-rational primary processes.

Having set forth the five foundational presuppositions of psychoanalysis as a general theory of mind, it is time to address the central question: can this theory of mind indeed be properly considered a scientific theory? Then there is the secondary question: If psychoanalytic general theory is a scientific theory, is it a good scientific theory? I will take these questions up in order.

Is psychoanalytic general theory a scientific theory?

Robert Nozick's (1981, p. 121) discussion of the structure of scientific theories provides a very useful initial framework for understanding the sort of question a scientific theory must address: "… given that N is a natural of privileged state, why is it that forces of type F, not some other type F′ produce deviations from N[?]." Peter Lipton (1990) suggests

that a scientific theory ought to be able to answer such a question (p. 207), "… explain[ing] phenomena by giving their causes …", and adds that (p. 220), particularly in the case of non-deductive theories (such as psychoanalytic general theory), a scientific theory should do so fulfilling an "obviously pragmatic" requirement with "something new". And yet without denying the above, David Lewis (1986, p. 195) recommends that, "A good explanation ought to show that the causal processes at work are of familiar kinds; or that they are analogous to [more] familiar … processes …"

Psychoanalytic general theory does very well on these criteria. If N is the "privileged state" of seemingly regular, continuous consciously mediated mentation and experience, and deviations from N are the seemingly inconsistent, perhaps even incoherent, gaps, F forces are the a-rational primary process contents caused by a dynamic unconscious that fill in the gaps and restore continuity. This F force explanation does far better than F´ forces—accident or charity—could. Further, as Lipton requires, psychoanalytic general theory not only provides a causal explanation for the deviations from N, it does so adding something new—a dynamic meaningful unconscious. Finally, although it is new, the concept of dynamic and meaningful unconscious mentation is a close analogue to the concept of dynamic and meaningful conscious mentation, and thus satisfies Lewis's recommendation for good explanations.

Not yet discussed but perhaps the best-known criterion for any scientific theory is its capacity to be tested. Here, perhaps surprisingly to some, pace Popper's (1963) famous claim of its non-falsifiable nature, psychoanalytic general theory of mind does quite well. True, the initial two assumptions of psychic continuity and psychic determinism are not (and arguably cannot be) directly tested empirically, but certainly the third assumption of a meaningful dynamic unconscious; and the fifth, that there exist two differently organised forms of thought—called primary and secondary process by Freud but renamed in various ways by a plethora of "dual process" cognitive scientists—have each given rise to an enormous wealth of empirical studies.[11] Further, it must be pointed out that psychoanalytic general theory of mind, unlike aspects of the clinical theory (or theories), is a grand overarching theory in the manner of evolution—it organises and explains what could otherwise not be explained except with many disparate and conflicting part-theories. Let me back this up by offering a set of criteria for a good scientific theory in the section that follows immediately below.

Is psychoanalytic general theory a "good" scientific theory?

In addition to testability, there are several characteristics typical of good scientific explanatory theories. In Brakel (2010) I gathered a number of them (albeit for a purpose different from that in the current chapter), all agreed upon by many of the authors in Boyd and Trout's (1991) edited volume on the philosophy of science. These are (as quoted from Brakel, 2010, p. 161):

1. Explanatory power, including predictions with new data, and the capacity to generate new and related questions. See Laudin, 1981, p. 233; Fine, 1984, p. 261; Boyd, 1985, p. 349; Lipton, 1991, p. 59; Gasper, 1991, p. 289; and Trout, 1991, p. 605.
2. Simplicity and internal coherence. See van Fraassen, 1977/1991, p. 326; Thagard, 1978, pp. 85–89; Boyd, 1985, p. 350; Kim, 1987, pp. 234–235; Lipton, 1991, p. 59; and Gasper, 1991, p. 289.
3. Diversity of phenomena explained; and related to this consilience and systematic organisation, both with neighbouring fields and those with distant theories. See Thagard, 1978, pp. 79–85; Cartwright, 1980, p. 380; Fine, 1984, pp. 261–262; Kim, 1987, pp. 234–235; Lipton, 1991, p. 59; and Trout, 1991, p. 608.

With respect to the first set of items: the general psychoanalytic theory of mind wields enormous explanatory power by rendering understandable all manner of human behaviours that would otherwise seem incoherent, not only in persons suffering from various mental disorders, but in all of us, in so far as everyone at least occasionally (and usually much more often) experiences the minor symptoms of the psychopathology of everyday life. Psychological symptoms as diverse as phobias, obsessions, and major affect dysregulations, as well as slips of the tongue and other parapraxes all have psychological content (interacting with the underlying neurobiology) and this content can be best understood within the psychoanalytic general theory.

Psychoanalytic general theory also allows predictions with new data—albeit predictions of a general nature—namely that each person's seeming psychological continuity will be disrupted, and that the disruption will have as a partial cause (at least some of the time) an unconscious conflict, one, in principle, capable of being revealed through the free associative method.

As to the final issue in this group, new and related questions, the psychoanalytic general theory of mind stands up particularly well.

So, for example, if we look at the final presupposition, the corollary that there are two different types of mentation—primary and secondary processes—the following questions naturally arise: Under which circumstances do the primary processes predominate? Usually in adulthood, the secondary processes are more in evidence, but since the primary processes never disappear, do they have a continued functional significance? If so what is its nature? Do the primary processes have an evolutionary history? Does animal mentation resemble the primary or secondary processes, or a hybrid?[12]

Turning now to the second matter listed, simplicity and internal coherence of the theory, again the psychoanalytic general theory of mind performs very well on this philosophy of science criterion. It has, in my view, only five presuppositions. These are interconnected and form the basis for deriving any clinical theory of psychoanalysis.

Finally, taking up the third requirement for a good scientific theory, let's examine the diversity of phenomena that the psychoanalytic general theory can explain, and its consilience with cognate and more distant fields. Again, much about human behaviour of many sorts can be explained with psychoanalytic general theory as a tool—symptoms, different mental states, and moods. Perhaps the general theory can be applied to certain animal behaviours too. Focusing again on primary as opposed to the more standard secondary processes, there may be links between this aspect of psychoanalytic theory and very basic psychological operations that are not well understood—conditioning,[13] for example, operating throughout the animal world. Then there is the promise of consilience with the more closely related fields of neurobiology on the one hand, and philosophy of mind on the other. For neurobiology, one can imagine future measureable brain correlations involving each of the following contrasts: conflictual *vs*. not conflictual; conscious *vs*. unconscious mental tasks; primary *vs*. secondary processes. For philosophy of mind, with the use of psychoanalytic general theory, there is the hope of widening the scope beyond just the conscious and rational. In fact, Part Two of this chapter is an attempt at demonstrating this latter type of consilience.

Brief summary for Part One

Addressing a foundational challenge in regard to psychoanalytic general theory as a regular science theory, I have presented a three-part

argument. First, I held that the psychoanalytic theory of mind, like every scientific theory, has a few basic assumptions that are taken for granted. Second, I proposed five basic foundational presuppositions underlying the psychoanalytic general theory—three assumptions, one methodological tool, and a corollary. Third, I asked two questions, the first pertained to a set of criteria important for determining whether a theory is a scientific theory; the second evaluated whether or not a scientific theory is a "good" scientific theory. The answer to both, I concluded, is an emphatic "yes!"

It is now time to turn to the second part of this chapter, addressing a different sort of foundational challenge to psychoanalysis.

PART TWO

An argument for the very possibility of meaningful a-rational mentation

As one can tell from the title of this section of the chapter, there are those who contend that meaningful mentation can only be rational (and conscious). This presents a major challenge to the central assumption of psychoanalysis. Thus, this section of the chapter will consist largely of a three-part argument. After illustrating the case made by those who make the restrictive claim, I will first present counter-arguments; second, provide a different philosophical model by which a-rational mentation *could* be understood as meaningful; and third, demonstrate how, indeed, a-rational mentation does fit that new model.

Background for the argument

Following Freud's (1900a) conception, I hold that the primary processes, although clearly mental and intentional with representational content, lack one or more of the hallmarks of the rational. Thus whereas secondary process rational mentation 1) is tensed (i.e., is about what occurs in a specific real time—past, present, or future), 2) is reality tested, 3) originates from a single agent's experientially continuous viewpoint, and 4) tolerates no contradictions; primary process mentation demonstrates at least one, often several, and sometimes all of the following. 1) Primary processes are without tenses. They involve no conception of past or future, only a tenseless and unexamined present. 2) In primary

process mentation there is no reality-testing—no attempt to regulate representations for considerations of truth. More specifically, no distinctions can be made among what is; what is not but is possible; and what cannot be. 3) Primary processes do not originate from an agent's experientially continuous viewpoint. They operate at a developmental level at which there is yet to be a stable self, capable of grasping (in any fashion) continuity-in-experience—this being a minimal requisite for "self" to stand out against the protean background of not self. 4) Standard logic is not employed in primary processes. Most notably, contradictions are tolerated.[14]

Having set out some basic characteristics of primary process mentation, I want to make clear that I am arguing for the *coherence* of the notion of the primary processes as mental states with content, because the mere concept of primary processes as contentful but non-rational already raises a critical issue upon which two important contemporary views in the philosophy of mind diverge. The first and more popular of these views—attributionism—would deny the possibility of mentation that is a-rational yet representational. For attributionists of every stripe—certainly for Donald Davidson (1970, 1973b, 1974a, 1975, 1982) the most extreme adherent, but no less for the moderates, Daniel Dennett (1978, 1987), Richard Cherniak (1981), and Stephen Stich (1983)—mental contents are to be determined by interpretations based on attributing and then assuming holistic rationality. In so far as attributionism holds that mentation must be interpretable in order to be contentful, and must be largely rational in order to be interpretable, this view would prevent the theory of contentful a-rational primary processes from even getting off the ground.

The first task then in Part Two of this chapter—after presenting the attributionist objection—is to refute it. Next I will present an alternative account that can accommodate the primary processes as contentful, without losing ground to the problem of indeterminancy. The alternative account is Ruth Millikan's (1984, 1993) proper function naturalism. Millikan's view can be understood as adopting a different normative assumption from that of the attributionists' assumption of interpretable holistic rationality. Millikan's programme makes the normative assumption of selective success utilising evolutionary explanation. This undermines the attributionist claim that mental content is contentful exclusively in virtue of being rational and thereby interpretable, and opens the way for contentful states that are a-rational.[15] But to complete

the Millikan-style proper-function naturalistic case for a-rational primary process contentful states, a viable proper-function explanation for the primary processes must be added. Indeed, then, the third task of Part Two of this chapter is to provide just such a proper-function account for these a-rational aspects of human mentation.

Attributionism

The basic tenets of attributionism, regarded as the most prominent and typical philosophy of mind view on the matter of mental content, can be seen most clearly in Davidson's work.[16] Thus minimal attributionism holds that: 1) content is constituted only when content can be attributed, 2) in order for content to be attributed, interpretable beliefs must be ascribed, and 3) belief ascription can take place only in the context of holistic rationality. (Regarding these three points, see especially Davidson, 1974a, pp. 237, 231; and Davidson, 1973b, p. 259.)

Before I attempt to undermine the attributionism objection to the notion of contentful primary processes, I want to point out that attributionism is not without its appeal. It is the attributionists, and Davidson in particular, who, for example, have cogently maintained that the very concepts of a-rational, irrational and inconsistent mental content states *depend* upon a background of rational mental states against which they can be contrasted. Thus from his 1970b essay "Mental events" Davidson (p. 221) states: "Crediting people with a large degree of consistency cannot be counted mere charity: it is unavoidable if we are to be in a position to accuse them meaningfully of … some degree of irrationality. Global confusion … is unthinkable, not because imagination boggles, but because too much confusion leaves nothing to be confused about …"

But this leads to an important question, which marks the start of refuting the attibutionist position. What sort of dependence is Davidson really implying here? Certainly it seems incontrovertible that even Freud's initial recognition of the concepts of inconsistent, irrational and a-rational human mental states depended on the contrasting background of consistent and rational human mental states. But this epistemologic dependence is more modest than the ontologic dependence Davidson holds. For him, unless a subject can be shown to have a holistic background of consistency and rationality, belief ascription will not be possible; hence mental content will not be attributed. Then, because

mental content attribution is what constitutes mental content, not only will the mental content of that subject remain unknown—there will be no mental content for this subject at all. Note that by these criteria children under the age of around three years, primates, adults in dream states, and certainly mammals in other orders, etc., will lack mental content states.

And yet this central problem for the attributionist—that attributing mental content is ontologically constitutive of mental content—seems not to bother Davidson. He is worried, on the other hand, about how to square his account with aspects of the Freudian view he does want to embrace. Thus what follows in the next subsection are his compromises, along with the problems they entail.

Davidson's attempt to acknowledge Freudian views while maintaining attributionism

Davidson does acknowledge the existence of (what he must regard as infrequently occurring) irrational mental states. Holding in his essay on "Paradoxes of irrationality" (1982, p. 299) that "… irrationality appears only when rationality is evidently appropriate", he wants to extend his view of mental content in order to account for the irrational-but-mental. That this effort might prove problematic, he admits—stating that, on the one hand, if he explains irrationality too well he fears he will have reduced the phenomenon to rationality, yet on the other hand, if he finds that what is irrational is merely incoherent, then irrational phenomena will cease to be recognisable as mental, and will thereby on his view (in which the epistemic is constitutive) cease to be mental at all.

Nonetheless, Davidson (1982) fashions a compromise solution with the use of what he considers psychoanalytic, Freudian-style concepts. For Davidson, human agents are capable of irrational acts owing to a split in "mental structures" into two or more semi-autonomous mental structures. Some of these semi-autonomous structures can even be unconscious. What is vital for Davidson, whether a structure is conscious or unconscious, is that each such semi-autonomous unit is an internally coherent rational system of beliefs and desires, where within each such structure psychological reasons are rational causes. Between different structures, on the other hand, there are non-rational causes. These non-rational casual relations between different structures (and

between the contents of the different structures) produce and account for irrationality (Davidson, 1982, pp. 290–304).[17]

While Davidson does want to believe in a sort of Freudian unconscious—a semi-autonomous unconscious structure containing beliefs and desires that are irrational in relation to an agent's conscious attitudes—it is a Freudian unconscious much coloured by Davidson's own attributionism. Namely, the unconsciously sequestered mental contents are for Davidson not only rational within their own semi-autonomous unit, they comprise an internally consistent rational system of beliefs and desires. Thus, here no less than elsewhere, attributionism precludes Freud's notion of the primary processes, in which the primary processes, whether conscious or unconscious, are not rational. Moreover, while the concept of the unconscious for Freud was not static over his long career as a theorist, it certainly can be said that at no time did he characterise the unconscious as a coherent system of rational (secondary process) beliefs and desires.

So what motivates the attributionist to charitably attribute rationality?

Irrational and a-rational states occurring more than just very infrequently would, from Davidson's position, allow too many interpretative hypotheses—in other words, indeterminacy would threaten. His principle-of-charity argument, central to his attributionism,[18] clearly arises from this threat. At first, Davidson's argument for the principle of charity seems to aver to the need for some normative constraint to narrow the domain of eligible interpretations. Says Davidson (1967, p. 27) in "Truth and meaning": "we must maximize the self-consistency we attribute to him [an alien], on pain of not understanding him. No single principle of optimum charity emerges; the constraints therefore determine no single theory [of meaning]." And again in "Belief and the basis of meaning" (1974b, p. 154), without mentioning rationality he stresses that, "Each interpretation and attribution of an attitude is a move within a holistic theory, a theory necessarily governed by concern for consistency and general coherence with the truth …" (Both of these papers are in the *Truth and Interpretation* (1984) collection.) But it becomes clear that Davidson is really advocating for only one type of normative constraint—the charitable attribution of holistic rationality. In "Psychology as philosophy" (1974a, p. 231) he states that we charitably

and "… necessarily impose conditions of coherence, rationality, and consistency." Later (p. 237) he adds: "… if we are intelligibly to attribute attitudes and beliefs, or usefully describe motions as behaviour, then we are committed to finding, in the pattern of behaviour, belief, and desire, a large degree of rationality and consistency." Still later (p. 239) he states: "The constitutive force in the realm of behaviour derives from the need to view others, nearly enough, as like ourselves … [i.e.,] 'mostly rational'". Finally, in yet another 1973(b) article, "On the very idea of a conceptual scheme" (In *Truth and Interpretation*) one can see that for Davidson the notion of some charitable principle for providing normative constraint has collapsed into his particular choice of rational normativity charitably ascribed. He writes (p. 197): "… charity is not an option, but a condition of having a workable theory … Charity is forced on us; whether we like it or not, if we want to understand others, we must count them right [rational] in most matters."

Thus, for Davidson-style attributionism, this particular type of charity—in which rational normativity is attributed and then actually constitutive of mental content—must be endorsed in order to counter the ever-serious threat of indeterminacy of meaning that would occur without such charitable attribution.

Refutation of attributionism

There are three parts to undermining the attributionist position and therefore disarming the attributionist view that a-rational primary process content is not a coherent possibility. First, and perhaps least convincing, there are the misunderstandings of Freudian views of the unconscious. Next, and far more damaging, is the attributionist confusion of epistemologic dependence with ontologic dependence. Although the discovery of non-rational mentation (both a-rational and irrational) is dependent upon rational mentation, the discovery is an epistemological feat, and is unrelated to ontologic dependence, which, according to psychoanalytic theory, goes in the other direction, with primary process mentation developmentally prior to secondary. There seems to be no principled reason, and in fact it seems viciously circular, to contend that rationality-based mental content attribution is actually constitutive of mental content. That is, there is no reason *unless* Davidson-type rational normativity-based charity is the only way to prevent radical indeterminism; in other words the only way to ground meaningful content. In

the next sections then, I propose to demonstrate that there is another normative principle to prevent indeterminism, thus completing the refutation of attributionism. The other principle is evolutionary fitness normativity, and it is propounded forcefully in the proper-function naturalism of Ruth Millikan (1984, 1993). I turn now first to present this view, and then to demonstrate how primary process a-rational contentful mentation can and does fit with this naturalistic account.

Millikan's proper-function account of representation

To appreciate fully the Millikan view, (See Millikan, 1993, Chapters Three, Four, Five and Nine, pp. 51–121 and 172–192) it is useful to look at her concepts of proper function and "Normal" conditions as they apply to simpler organ mechanisms and states. The function of sweat glands is to secrete sweat. But with regard to "proper function", this is only part of the story. For the sweat glands to be indeed properly functioning, "Normal" conditions for the sweat secretion are required. Thus, the body being overheated, then secreting sweat, followed by the body's return to its regular temperature are among the Normal conditions under which sweat gland secretion can be considered to be properly functioning. On the other hand, if there is tumour at the temperature-sensing centre, such that a regular or even a low temperature leads to sweat gland secretion, even though the sweat glands themselves are fully operative, they are not functioning under Normal conditions. As Millikan (1993, p. 73) says, "… though it is a proper function of the sweat glands to secrete sweat, this is so [Normally] if and when the body is overheated." Now this distinction is important because only organisms with sweat glands properly functioning under Normal conditions, and not organisms with merely adequately functioning sweat glands, were (and are) aided in their proliferation—in part because of these properly functioning sweat glands. In other words, the Normal conditions are those conditions under which the inherited proper function confers the selective advantage.

Let's turn now to more relevant matters: belief-manufacturing mechanisms. Their function is to manufacture beliefs. But as in the case above, only organisms with properly functioning belief-manufacturing mechanisms, under Normal conditions, enjoy increased proliferation due to the proper function of these mechanisms. Now belief-manufacturing mechanisms function to produce beliefs, but, clearly not all beliefs are properly functioning under Normal conditions. Normal conditions for

beliefs, those that conferred (and confer) selective fitness, are those in which the truth conditions of the representation (belief) match those obtaining in the world. Thus only mechanisms manufacturing these true beliefs are those belief-manufacturing mechanisms that are properly functioning under Normal conditions. In Millikan's words (1993, p. 73): "… it is a proper function of the belief-manufacturing mechanisms in John to produce beliefs-that-*p* only if and when *p*, for example, beliefs that Jane is in Latvia only if and when Jane is in Latvia … To turn this around, a belief that Jane is in Latvia is, and is *essentially*, a thing that is not Normally in John unless Jane is indeed in Latvia." But note that for Millikan there is nothing a priori or necessary in believers holding true beliefs, or in coming by these beliefs rationally. Rather, true beliefs and the rationality they usually entail are contingently determined biological norms. The Normal conditions for beliefs—those conditions that confer selective advantages to belief holders—are those in which the truth conditions of the belief's representation are constituted by the truth conditions of the world. But, it just happens to have been the case (and presumably continues to be so) that our ancestors capable of having these rationally mediated true beliefs, and exercising this capacity enough of the time, had selective advantages such that the mechanisms supporting rationally mediated true belief generation were reproduced.

Actually, Millikan's view on rationality could not be more different from that of the attributionists. For the attributionists it is *rationality* that provides the underpinnings for intentionality and mental representation. Millikan (1993, p. 109) paraphrases the serious attributionist worry concerning her view thus: "How could anything exhibit intentionality … that was not, at least to an approximation, rational? Is not rationality, as Dennett claims, 'the mother of intention' …"[19] Millikan's various comments on toads and lead pellets can be used to address this worry in two steps. First, she can show that by her criteria toads, even though they cannot differentiate lead pellets from bugs—owing to a powerful reflex mechanism they will swallow countless numbers of both kinds—have contentful representations of bugs. She explains (1993, p. 94), and I paraphrase thus: At time *t* when the image [on a toad's retina] corresponds to a bug and not a lead pellet, the toad's representation producer properly functions and produces a fitness-enhancing representation of a bug. At time *t + 1* when the new image corresponds to a lead pellet, the toad's representation producer still functions properly

and a bug is represented again. But this time the representation is not fitness-enhancing because *abNormal* conditions (namely that lead pellets are present but not bugs) have obtained. Only those representations of bugs that have contributed to toad nutrition and thus to toad reproductive success are Normal-condition representations. But the representation of bugs when pellets are present are representations of bugs no less; the difference is these bug (pellet) representations are abNormal-condition representations.

In the second step, she can show that she has made no claim whatsoever for toads having rational capacities. Regarding a toad swallowing lead pellets, Millikan (1993, p. 76) holds: "His inner activity does not include separable states or features, one to correspond to his belief that the pellets are bugs, another to his desire to eat bugs." With her use of minimalist, naturalist criteria Millikan has thus been able to effectively divorce representations, complete with fixed determinate intentional content, from rationality.[20] Whereas for attributionists, content fixing is due to ascribing beliefs, charitably assuming general rationality, and then finding success in belief interpretability; Millikan resolves the indeterminacy problem and fixes content in a different way. The truth conditions of a representation are constituted by the Normal conditions for that representation—in other words the particular conditions under which fitness success just happened to be conferred. For Millikan, the normative assumption of selective fitness success replaces the normative assumption of rationality success; and her normative constraint narrowly determines the content of representations, resolving the indeterminacy problem with equal success. Thus toads narrowly represent both bugs and lead pellets as "bugs"—the former are Normal representations; the latter abNormal. In Millikan's system, toads have no wide, disjunctive indeterminate representation "bug or pellet".

Note that freed of the burdensome charity of presuming holistic rationality, Millikan (1993, p. 91) can state that: "… it is not necessary to assume that most representations are true. Many biological devices perform their proper functions not on the average but just often enough … it is conceivable that the devices that fix human beliefs fix true beliefs not on the average, but just often enough."[21] Further, Millikan has no trouble accounting for false representations. A toad representing lead pellets as bugs shows a frequent form of misrepresenting that yields false representations. The analogue with people and their beliefs is straightforward. False beliefs, a-rational and irrational beliefs are beliefs

for Millikan no less; and they need not indicate any problems in the mechanisms needed for believing, because false beliefs, a-rational and irrational beliefs can arise from various abNormal conditions, internal and external. False, a-rational and irrational beliefs on Millikan's account are beliefs that are incapable of performing their functions in a Normal way. This means they have not been and cannot be the sorts of beliefs that play a role in the selective evolutionary fitness of their believers. On the other hand, rationally mediated true beliefs—only because just enough of these states have had content vital to survival and reproductive success and hence have played a role in their believers' adaptive fitness—are the only beliefs that can be said to be properly functioning under their fully Normal conditions.

Having set forth a way to fix content and avoid indeterminacy without charitably attributing rational content—using selective fitness normativity rather than rational normativity, thus allowing for the possibility of a-rational primary process mentation—we have arrived at the last step in my argument. My task is now to demonstrate that a-rational contentful primary processes not only are possible, but that in fact the primary processes do have a Normal condition proper function capable of providing a selective advantage.

A proper-function account for primary process mentation

Whereas belief is the cognitive attitude most associated with secondary process, phantasy is its primary process counterpart. As was indicated above, a Normal condition for the proper function of belief that *p*, is that *p* will be believed-true just in those cases that *p* truly obtains now. It is clear that beings with properly functioning belief states have, under this Normal condition, selective advantages.[22] The particular belief states in which this is best demonstrated are belief states with contents vital for biological fitness—for example, true Normal-condition beliefs about the health of conspecifics who are potential mates. In order to understand the parallel situation for phantasising and particular phantasy states, the promised proper-function account is now due.

This begins by stating the proper functions of phantasising and phantasies. The proper function of phantasising is to produce a-rational primary process phantasies. These primary process states, I maintain, have fixed determinate content in the absence of rationality. In fact, phantasies are properly functioning only when they demonstrate one or more of the four a-rational characteristics of primary process

mentation enumerated at the beginning of Part Two of this chapter, and are summarised as follows. Properly functioning phantasies are: 1) without tenses—in other words they are phantasised only in an unexamined present; 2) not reality tested—they are phantasised without attempts to regulate representations for truth considerations such that, for the agent, phantasies are neither correct or incorrect; 3) not phantasised from an agent's singular and continuous viewpoint; and 4) phantasised in a manner that the phantasies admit of contradictions and other lapses in ordinary logic.

Having advanced these four proper functions for phantasies, there are two interrelated questions to address. First, how can the contents of such properly functioning a-rational phantasies be determinately fixed? Second, how can such properly functioning phantasies contribute to the selective fitness of beings that can and do phantasise? Just as the contents of beliefs are fixed by the conditions that are Normal for the proper functioning of beliefs, the Normal conditions for the proper function of phantasies will likewise fix their content. But this can only be the case if under their Normal conditions properly functioning phantasies confer selective fitness advantages for phantasisers. Thus to address both questions I turn now to demonstrating that there are particular conditions under which the proper function of phantasies do confer selective fitness advantages, and, as such, these Normal conditions determine and fix the content of the phantasies quite without rationality.

There are three fitness-conferring conditions for the proper function of phantasy that must be specified, all rather unlike any Normal condition for beliefs:

1. Whereas a fitness-conferring condition for belief is that *p* will be believed-true now, just in cases where *p* truly obtains now, for phantasy a selective advantage accrues only in the opposite case: that *p* will be phantasised-true now, only in those cases in which *p* does not now obtain.
2. Although, as just indicated, one of the fitness-enhancing conditions for the proper function of phantasy *p* is that *p* does not obtain now at time *t*; another condition for the properly functioning phantasy *p* to confer selective fitness success is that *p* will obtain later, say at time *t+1*.
3. Finally, the third condition under which the proper function of phantasy *p* provides fitness advantage: The phantasy with content *p*

at time *t* will have afforded some useful practice for the phantasizer, when at time *t+1*, some time after the phantasy *p*, *p* does obtain.

Are phantasies that meet these criteria actually seen in nature? Yes, we see such phantasies in action during play activity. Many species of birds and mammals, of course including humans, engage in play. Zoologists have observed that play activity is very often of a form that although it confers no immediate benefits now—in terms of nutrition, gaining resources, avoiding danger, or attracting conspecifics[23] does provide a practice for any or all of these activities, all of which will be very serious business later.[24] Thus, children, young dogs and, in fact, many species of mammals play at fighting, and they do so with beings who are not enemies. This sort of play, a phantasy lived out, meets the primary process criteria necessary to be considered a properly functioning phantasy: X is engaged in "fighting" (of the play fighting phantasy type) now, at a time when there is no real fighting, with an "enemy", Y, who is no real enemy. Or take three other instances, all with content relevant to biological fitness and all frequently encountered in animals, including (with only slight modifications) humans: 1) Z participates in play where Z mock chases mock prey, 2) P pseudo flees from pseudo predators, and 3) R mounts several practice mates, simulating copulation.

Thus, given then that: 1) there are conditions for the proper function of a-rational primary process phantasies that can provide selective advantage for beings so phantasising, and 2) that these conditions can thus be considered the Normal (although certainly not the typical) conditions for the proper function of primary process phantasies, the content for primary process phantasies can be fixed appropriately,[25] and in the absence of rationality.

Brief summary for Part Two

The mainstream and popular attributionist claim that rational normativity is necessary for content fixing has been refuted. Such a claim, if true, would have obviated the very possibility of contentful primary process mentation, something very central to psychoanalytic theory. I presented three steps in the argument against the attributionist view. First there were internal troubles for the view—1) the misunderstanding of the Freudian unconscious; and more importantly 2) the circular confound of epistemological with ontological dependence. Indeed the

position that mental content is actually constituted by the charitable attribution of rationality typifies this vicious circularity.

And yet, if charitable attribution of rationality were the only way to provide fixed content and prevent radical indeterminism, one might be placated. But this is not the case. Proper-function naturalism substituting fitness normativity for rational normativity allows for the possibility of content without rationality, such as is typical of the a-rational primary processes. The final stage in the argument for a-rational representation was a demonstration that the primary processes did in fact have Normal condition, fitness-enhancing proper functions.

Conclusions

Two challenges to the very core of psychoanalysis were taken up in two sets of arguments. In Part One a case was made for the standing of psychoanalytic general theory of mind as a regular scientific theory and a good one at that. In Part Two, I argued for the possibility (and then the actuality) of meaningful, representational a-rational mentation. Both of these conclusions allow psychoanalysis to flourish. The existence of primary processes with representational content allows a vast array of behaviours—including those that would otherwise be deemed incoherent—to be studied and potentially understood. Also, the very notion of primary process a-rational contents expands the domain of the philosophy of mind—beyond the merely rational and conscious mind, to include the a-rational and unconscious mind. Psychoanalysis as a regular science is no less momentous. The general psychoanalytic theory of mind can take its place as one of the grand world theories, changing our understanding of ourselves and the world around us by positing a meaningful dynamic unconscious as a central tenet. Moreover, as a regular (as opposed to a sui generis) scientific theory, psychoanalysis can participate in the world of sciences, benefiting from cognate discoveries, and contributing its own organising concepts.

CHAPTER SEVEN

The scientific status of psychoanalysis revisited*

Agnes Petocz

Introduction: three ways in which the question of the scientific status of psychoanalysis has been "done to death"

Almost everyone agrees that the question of the scientific status of psychoanalysis has been "done to death". It seems to me that this is indeed so in at least three interrelated senses: the open house, the doomsday cult, and the Cleopatra.

First, the open house sense. Impugning the scientific status of psychoanalysis appears to be open to anyone, regardless of background, qualifications, level of expertise, or familiarity with the primary sources. Accordingly, psychoanalysis boasts an enormously wide range of critics across the whole spectrum, from relatively heavyweight philosophers of mind and science such as Erwin (1980, 1988, 1993, 1996), Grünbaum

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(1979, 1984, 1993a, 2008), Popper (1963, 1986) and Wittgenstein (1932–1933, 1953), through serious scientific psychologists such as Eysenck (1985, 1986), Macmillan (1991) and Sulloway (1979, 1995), all the way down to standard textbook authors (e.g., Leahey, 2013) and a host of semi-popular writers from other disciplines (e.g., Crews, 1986, 1993, 1996; Hobson, 1988).

Second, in the doomsday cult sense, the charge heralding the demise of psychoanalysis keeps reappearing, announced with the same conviction as are the prophecies of Armageddon. When nothing happens, it is followed sooner or later by yet another purported coup de grâce. Eysenck considered himself to have destroyed psychoanalysis in the 1950s, but assures us thirty years later that "What cannot be doubted … is that psychoanalysis *is on the way down*, that it has lost any academic credibility" (1986, p. 207, emphasis added). Grünbaum's (1984) *Foundations* produced, according to Holt, "the most substantial indictment of Freud as a scientist that we have yet seen" (1986, p. 242) and "dealt psychoanalysis a body blow if he has not fatally wounded it" (p. 243). Crews wrote in 1988 that "we would be well advised to cease temporizing … waiting for Godot … and acknowledge the scientific bad faith of the entire Freudian enterprise" (p. 236), and a few years later observes that psychoanalysis has "simply been left behind by mainstream psychological research" (Crews, 1993, p. 55). Nor is this surprising, he adds, given Macmillan's (1991) "exhaustive" demonstration that "Freud's theories of personality and neurosis … amount to castles in the air" (Crews, 1993, p. 55). Sulloway (1995), having insisted that "Freud failed because he was a lousy scientist, not because he engaged in interdisciplinary theorizing" (p. 170), hails Kitcher's (1992) critique to be "the single best account of why psychoanalysis has failed so resoundingly as a theory of the mind" (p. 170). And Erwin (1996) concludes his book *A Final Accounting* with the pronouncement that there is simply not a scrap of evidence in favour of psychoanalytic theory.

The third way in which the question has been done to death is in the Cleopatra sense. With respect to the actual *content* of the criticisms against psychoanalysis, a latter-day Enobarbus might remark that age cannot wither it, nor custom stale its infinite variety. Consider, for example, the following list of assertions, each to be understood as preceded by the words: psychoanalysis is unscientific because … it is unfalsifiable in principle; it lacks significant experimental or epidemiological support; it is scientifically alive but currently hardly well, because its

clinical foundations are evidentially suspect; Freud was scientifically incompetent, and his powers of observation and analysis failed to function independently of his wishes; its elastic interpretative licence allows the analyst to be "right every time"; it has been falsified—Freud's therapy is no better than placebo, and worse than alternative therapies; it produces the phenomena it claims to be discovering; it does not submit itself to controlled and rigorous forms of research; its clinical data are fatally contaminated; it is unfalsifiable because its proponents refuse to acknowledge falsifications as such, and dodge refutation via immunising strategies; its predictions are untestable because they are of hidden psychological states and not of overt behaviour; hardly anyone today thinks it is a science anyway—it is rather an art, which deals with meanings rather than causes, and so is more accurately characterised as a hermeneutical system; Freud wilfully manipulated evidence—he fitted the evidence to the theory and ignored recalcitrant evidence; Freud fitted the theory to the evidence; Freud lied; and so on.

Of course, the defenders of psychoanalysis have been no less active and productive in the face of this critical onslaught, recruiting equally from across the board, matching each thrust with counter-thrust, each crucifixion with fresh resurrection. The result has been a widely acknowledged impasse, illustrated in the following polarised set of conclusions:

> [The Freudian legacy is] nothing but imaginary interpretation of pseudo-events, therapeutic failures, illogical and inconsistent theories, unacknowledged borrowings from predecessors, erroneous 'insights' of no proven value, and a dictatorial and intolerant group of followers insistent not on truth but on propaganda. (Eysenck, 1985, p. 202)
>
> As the century ends, a century that some have called 'the century of Freud', the evidence supports the following verdicts. Has the effectiveness of Freud's therapy been established? No. How much of his theory has been confirmed? Virtually none of it. These verdicts are likely to be final. (Erwin, 1996, p. 296)

In contrast:

> A reservoir of experimental data pertinent to Freud's work currently exists and, as we have shown in detail, offers support for a respectable number of his major ideas and theories … However, a sizable

> proportion of those observers who are presumably concerned with evaluating the standing of psychoanalysis have simply refused to acknowledge the existence, or accept the credibility, of such findings. (Fisher & Greenberg, 1996, pp. 284–285)
>
> After a hundred years of controversy, we can now put to rest the criticism of psychoanalysis that its most fundamental assertion—the importance of unconscious processes—is mistaken or without empirical foundation. The data are incontrovertible: consciousness is the tip of the psychic iceberg that Freud imagined it to be. (Westen, 1999, p. 1097)
>
> There is a cornucopia of empirical evidence in the cognitive neurosciences, attachment field, infant-observation research, developmental psychology, clinical psychopathology, and the therapeutic process that are corroborations, validations, extensions, revisions and emendations of Freud's contributions. (Mills, 2007, p. 540)

Must we not conclude, then, that the question of the scientific status of psychoanalysis has indeed been done to death? Since neither side appears to have convinced the other or to have had the last word, what point could there be in re-engaging in this endless and fruitless debate? In my view, there are four compelling reasons.

Four reasons for revisiting the question of the scientific status of psychoanalysis

The first reason lies in the difference in aim between philosophy and psychology. One response in philosophy has been to simply sidestep the question. For example, a collection of essays on the philosophy of psychoanalysis edited by Levine (2000) chooses "to put issues relating to the scientific status of psychoanalysis to one side and to explore the philosophical implications of psychoanalytic thought for contemporary philosophical issues" (Levine, 2000, p. 4). A similar sentiment is expressed by the editor of the recent republication of the Hixon lectures on the scientific status of psychoanalysis: "be it a science or not, it has to be reckoned with as a phenomenon in our present-day world" (Pumpian-Mindlin, 1952, p. v). This may be all very well in philosophy, but for psychology it will not do. Psychology aims to be the science of the mind and behaviour. Since its modern inception it has struggled (albeit mistakenly) to extricate itself from philosophy and become a genuine, mature science. Accordingly, the most damaging accusation that can be levelled at any psychological theory is that it is "unscientific".

And no psychological theory has been subjected to more criticism for its putatively unscientific status than has psychoanalysis. That is why mainstream scientific psychology continues to be so dismissive of Freud and psychoanalysis, and why the academic institutions that train psychologists in accordance with the so-called "scientist-practitioner" model (i.e., as scientists first and then as practitioners) (Belar & Perry, 1991) typically treat psychoanalysis as, at best, an archaic relic of little contemporary value. As Fotopoulou (2012a) notes, "Today, particularly in the Anglo-American scientific world, Freudian ideas are portrayed as irrelevant, if not actually dangerous" (p. 13). The recent minisurge of interest in the issue at the turn of the century (e.g., Frank, 2000; Rangell, 2000), and then on the sesquicentenary of Freud's birth (e.g., Pincus, 2006; Wallerstein, 2006), was largely restricted to psychoanalytic journals, which are not on the radar of mainstream psychology. So whatever contributions are made by psychoanalysis to philosophy are considered irrelevant to scientific psychology. Thus any psychologist who wishes to argue, as I do, that psychoanalysis makes important contributions to scientific psychology has no choice but to pursue the question of the theory's scientific status.

The second reason is that the question of what exactly "scientific" means has recently re-entered the picture.[1] According to Talvitie (2012), "Both present-day advocates and critics of psychoanalysis tend to ignore the recent views of the philosophy of science" (p. xii); the debate has failed to keep up with the changed view that there is no general method of science, leaving most of the critics and defenders in the "dark ages" of mainstream psychology's outdated positivistic scientism. Thus, attention is directed to post-Kuhnian developments in the philosophy of science and the philosophy of the social sciences where "science" becomes relativised, and the notions of objectivity, fact and truth on which traditional science rests are called into question (Kuhn, 1962). However, while there is much of value here in its challenge to psychology's conception of science, the danger is to throw out the baby with the bathwater. There is plenty of evidence to show that mainstream psychology is indeed shot through with misconceptions of science and is thus *scientistic* rather than genuinely scientific (e.g., Bickhard, 1992; Gigerenzer, 1993; Machado & Silva, 2007; Mackay & Petocz, 2011a; Michell, 1997, 2000, 2001, 2009a, 2009b, 2010; Petocz & Newbery, 2010).[2] However, these confusions notwithstanding, mainstream psychology justifiably resists the postmodernist message of scientific irrationalism with its conflation of the logic with the sociology of science (cf. Lambie,

1991; Stove, 1982, 1991). So, instead of attacking the mainstream for supporting science per se, the mainstream should be held to account for distorting science. What is required is a defence of science within reason, as Haack (2003) puts it, an exposing of the myths of science in mainstream psychology, and a return to the broader, realist view of scientific method as critical inquiry aimed at investigating natural systems (including human and psychological systems) while employing the best available error-detection mechanisms (Cohen & Nagel, 1934; Mackay & Petocz, 2011b; Petocz & Mackay, 2013). This is a defence of science that charts a course between mainstream scientism and postmodernist antiscience (Petocz, 2001, 2004), accepts the legitimacy of both quantitative and qualitative methods (Michell, 2001, 2004a), sees no sharp distinction for science between *Geisteswissenschaften* and *Naturwissenschaften* (Haack, 1996; Rustin, 2000; Schilder, 1935), and thus rejects the widespread view that "the naturalistic attitude breaks radically with a human scientific view of man as an intentional acting person" (Karlsson, 2010, p. 24). If mainstream psychology needs to adjust its conception of science along these lines, then this has implications for the criteria by which the question of the scientific status of psychoanalysis is to be pursued.

The third reason for revisiting the question is the fact that new developments in contemporary psychology, movements of expansion and integration that are currently sweeping through the mainstream, represent, ironically, an inadvertent return to Freud. Prominent amongst these movements are "second generation" cognitive science (Kövecses, 2005), embodied-embedded cognitive science (Clark, 1997; Gallagher, 2005; Varela, Thompson, & Rosch, 1991; Wheeler, 2005), radical embodied cognitive science (Chemero, 2009), enactivist, extended and ecological approaches to mind (Heft, 2013; Hutto & Myin, 2013; McGann, De Jaegher, & Di Paolo, 2013; Rowlands, 2010; Thompson, 2007), evolutionary psychology (Bouissac, 1998; Buss, 2005; Tooby & Cosmides, 2005), affective neuroscience (Damasio, 2000; Panksepp, 1998), developmental neuropsychoanalysis (Schore, 2003a, 2003b), cognitive semiotics (Andreassen, Brandt, & Vang, 2007; Donald & Andreassen, 2007; Jorna & van Heusden, 1998; Smythe & Jorna, 1998), and embodiment and conceptual metaphor (Gibbs, 2011; Hopkins, 2006; Kövecses, 2010; Lakoff & Johnson, 1999). These are all movements that involve an expansion of attention from an isolated cortex to its bodily, emotional and environmental context. But the return to Freud is both silent and disguised. This is not surprising, given the widespread belief that

psychoanalysis is unscientific. So strong is that belief, in fact, that even psychology's love affair with neuroscience and the psychological "neuromania" (cf. Tallis, 2011) that welcomes anything as long as it has a "neuro-"prefix (Cleeremans, 2010, p. 1) are not enough to admit the new discipline *neuropsychoanalysis* to the mainstream psychological party,[3] though it remains to be seen whether the latest repackaging (in Fotopoulou, Pfaff, & Conway, 2012) fares any better. As a result, while ever any attempt to draw attention to the Freudian origins, theoretical underpinnings and contributions to major concepts that are now taking centre stage is met with incredulity and dismissal, the door remains closed to investigating elaborations and implications and to potential successful unification in psychology.

The final reason it is worth revisiting the issue is that there is one major sense in which the question of the scientific status of psychoanalysis has *not* been done to death—has hardly, in fact, been begun. What is missing is a meta-analysis[4] of the issue. There is nowhere, to my knowledge, any attempt to subject the whole field of criticisms to a comprehensive, synoptic, synthetic treatment—one which, having drawn together into a single place all the material devoted to the question, presents the following three steps: (1) a qualitative thematic analysis leading to a descriptive taxonomy that identifies different categories of charge; (2) a scrutiny of the logical relations among the categories identified in the synopsis, such that any critic's stance across those categories can be properly evaluated; (3) an examination of the various within-category argument strategies and manoeuvres engaged in by the participants.

In the rest of this chapter, I propose to present this meta-analysis. There is not space to do this as thoroughly as needed, so I am limited to an illustrative sketch rather than an exhaustive treatment.[5] But it will be sufficient, I hope, to draw from the results of each step some conclusions for psychology that are both surprising and far-reaching.

Meta-analysis of the issue

Step One: synopsis, thematic analysis, and taxonomy

After collation of the disparate material related to the charges against psychoanalysis, thematic analysis reveals that what seems at first sight to be an enormously complex, unwieldy and overwhelming amount of material can actually be grouped into a relatively small number

of different categories. This highlights certain important but often neglected distinctions, and prepares the way for a proper critical evaluation, not just of each of the accusations themselves, but of the way they have been used in relation to each other in the debate as a whole. I have identified six basic categories, summarised in the following thematic propositions:

Category 1. Psychoanalysis is unscientific because it deals with meanings rather than causes and so is a hermeneutic rather than a scientific enterprise.

Category 2. Psychoanalysis is unscientific because it has been discredited by the behaviour of its proponents.

Category 3. Psychoanalysis is unscientific because it is in principle unfalsifiable.

Category 4. Psychoanalysis is unscientific (or, rather, continued adherence to psychoanalysis is unscientific) because it has been falsified.

Category 5. Psychoanalysis is unscientific because it relies on methodologically defective procedures, some of which make it unfalsifiable in practice.

Category 6. Psychoanalysis is unscientific because it has poor possibilities vis-à-vis observation, prediction, and control.

The conclusion of this first part of the meta-analysis is that the Cleopatra sense in which the question has been done to death amounts to a meretricious veneer over repetitive bombardment; the sheer volume and variety of the charges against psychoanalysis collapses into something more manageable and less overwhelming than might appear at first sight.

Step Two: relations among the categories and evaluation of cross-category stances

Clearly these categories are not all mutually independent, and the boundaries between them are sometimes fuzzy. Nor are they all transparent or straightforward; it is sometimes not immediately clear exactly what is being asserted, and/or the proposition may require "unfolding", insofar as it constitutes a kind of "umbrella term" for a set of separate assertions. For example, in the Category 5 claim that

psychoanalysis is unscientific because it is based on methodologically defective procedures, there is an assortment of procedures to which this charge may refer. Nevertheless, two points emerge from the second step of the meta-analysis.

The first is that the logical relations among the categories impose constraints on which charges can sensibly be levelled simultaneously. For example, if Category 1 is put forward, then Categories 3–6 are irrelevant; if psychoanalysis is not meant to be scientific, then its supposed scientific flaws cannot be a problem. Likewise, Category 3 precludes Categories 4, 5 and 6; either psychoanalysis is unfalsifiable in principle or it has been falsified, but it cannot be both. And so on. In contrast, Categories 2, 5, and 6, form a coherent combination; a critic might claim that the defenders of psychoanalysis, aware that it has poor observational possibilities, deliberately employ defective methodological procedures to avoid confrontation with unfavourable evidence.

The second point that emerges is that the two prominent critics of psychoanalysis who are typically invoked in the mainstream psychological literature, namely Popper and Eysenck,[6] occupy incoherent cross-category stances with respect to their famous critiques. Popper's stance vacillates between categories 3, 4, and 5. Eysenck, not to be outdone, spreads himself further.

First, Popper's cross-category stance. He says psychoanalysis is unfalsifiable (Category 3), but then, as Grünbaum (1986b) notes: "by Popper's own appraisal, the dream theory is not only falsifiable but had already been falsified by anxiety dreams, when Freud first proposed it!" (p. 268) (Category 4). Popper also vacillates between "in principle" unfalsifiability (Category 3) and "in practice" unfalsifiability (Category 5). In his original indictment, he claims the theories of Marx, Adler and Freud are pseudo-sciences, but that Marxism, in some of its earlier formulations, produced predictions which *were* testable and falsified. However:

> instead of accepting the refutations the followers of Marx re-interpreted both the theory and the evidence in order to make them agree. In this way they rescued the theory from refutation; but they did so at the price of adopting a device which made it irrefutable. They thus gave a 'conventionalist twist' to the theory; and by this stratagem they destroyed its much advertised claim to scientific status. (Popper, 1963, p. 37)

And Popper continues immediately with the following comment:

> The two psycho-analytic theories were in a different class. They were simply non-testable, irrefutable. There was no conceivable human behaviour which could contradict them. (Popper, 1963, p. 37)

Of course, if there is no conceivable human behaviour which could contradict psychoanalysis, then, a fortiori, there can be no actual human behaviour that would do so. Therefore, whatever happens, confrontation with the facts cannot be a danger to psychoanalysis. Yet, as Grünbaum (2008) points out, because Popper has also repeatedly claimed that Freud and his followers used the conventionalist stratagem to evade refutation (Category 5), his "two claims are evidently incoherent" (p. 577). Thus, when other critics (e.g., Cioffi, 1970) maintain that what makes psychoanalysis a pseudoscience is not that it is unfalsifiable, but rather that its proponents use methodological manoeuvres to avoid falsification and evade refutation, Popper fails to stand by his famous unfalsifiability charge and attack the misguided nature of such manoeuvres; he does not point out that Freud's defenders are simply wasting their time, such manoeuvres being futile in the face of an unfalsifiable theory.

Eysenck insists that Popper's application of the falsifiability criterion to psychoanalysis is mistaken and irrelevant, for psychoanalysis is indeed falsifiable. In fact, he adds, psychoanalysis has been falsified (Category 4); "what is true in the theory is not new and what is new is not true" (Eysenck, 1985, p. 123). Moreover, psychoanalysis has been discredited by the behaviour of its proponents (Category 2). But psychoanalysis is also a hermeneutic, not a scientific, enterprise (Category 1):

> Such an approach which stresses *meaning* is the exact opposite of the natural science approach which stresses the study of *behaviour* … Freud was one of those who hankered after the natural science of behavioural research, but whose major contribution is clearly on the hermeneutical side. (Eysenck, 1985, p. 194)

Finally, Eysenck supports and conflates Categories 5 and 3, circling back to the Popperian unfalsifiability charge that he had earlier rejected:

> Freud had a very clever ploy for dealing with disagreement. If the patient agreed with his interpretation, then Freud claimed that the interpretation was obviously correct. If the patient disagreed, however, Freud claimed that this was because of psychoanalytic 'resistance', which makes interpretation unacceptable precisely *because* it is correct; hence disagreement also indicates the correctness of the theory. Clearly, there is no way in which the theory could be disproved—a very fortunate state for a scientific theory to be in, one might have thought. In actual fact, of course, the opposite is true: if a theory cannot be disproved by any observable fact, then as Karl Popper has pointed out so many times, it is not a scientific theory at all. (Eysenck, 1985, p. 129)

In sum, the cross-category positions of both Eysenck and Popper are akin to Freud's famous "kettle logic" example; a neighbour accused of having returned a kettle in a damaged condition "asserted first, that he had given it back undamaged; secondly, that the kettle had a hole in it when he borrowed it; and thirdly, that he had never borrowed a kettle from his neighbour at all" (Freud, 1900a, p. 120). Therefore, my conclusion from the second step of the meta-analysis is that mainstream psychology's rejection of psychoanalysis, insofar as it appeals repeatedly to the claims of these two famous critics, rests on a case that is fatally entangled in contradiction. Psychologists are blissfully unaware of this situation, just as they are unaware that most of the major critics of psychoanalysis within the philosophy of science reject Popper's unfalsifiability charge. It is an irony, then, that the Popperian charge continues to be regarded amongst psychologists as the major knock-down argument against psychoanalysis.

Demonstration of incoherence at the level of cross-category stance shows only that the relevant critics' *overall* position cannot be taken seriously. It does not show anything about the success or failure of each *individual* charge. As a psychologist, Eysenck may have been influenced by the standard experimental practice of guaranteeing a significant result by casting the net wide enough and conducting a sufficiently large number of statistical tests. Perhaps not all the charges against psychoanalysis will stick, but there's a good chance one or two will. To examine this possibility, I shall turn now to the third step of the meta-analysis.

Step Three: evaluation of within-category argument strategies

In the Step Three evaluation of the within-category argument strategies I shall attempt briefly to provide descriptive clarification of each category, a selection of criticisms and standard responses, evaluation, and, finally, some provisional conclusion regarding the status of the charge.

Category One: psychoanalysis is unscientific because it deals with *meanings* rather than *causes* and so is a hermeneutic rather than a scientific enterprise

When Grünbaum (1986a) concludes that psychoanalysis is "scientifically alive" but "currently hardly well" (p. 228), Storr follows Eysenck and responds that Grünbaum is:

> flogging a dead horse. Only a few fundamentalist psychoanalysts of an old-fashioned kind think that Freud was a scientist or that psychoanalysis ever was or could be a scientific enterprise … Freud was a man of genius whose expertise lay in semantics. That is, he was able to take the apparently inexplicable problems with which his patients confronted him and make both sense of their symptoms and coherent narratives out of their lives. (Storr, 1986, p. 260)

More recently Mills (2007) reiterates this charge, accusing Grünbaum of committing a category mistake and of regurgitating across three decades "the redundant point that psychoanalysis is not a true science" (p. 539). Eagle (1986a) identifies the aspects of psychoanalysis that contribute to this view:

> Because of its purported emphasis on interpretation and meaning rather than causal explanation, reasons rather than causes, "narrative truth" rather than "historical truth", psychic reality rather than objective reality, psychoanalysis has been "selected" by hermeneutic thinkers to serve as a prototype for the hermeneutic approach to the "human sciences" or *Geisteswissenschaften*, as envisioned by Dilthey and others. (p. 231)

Undoubtedly, much of Freud's material lends itself to this popular hermeneutic reconstruction. Moreover, he failed to distinguish clearly between conventional and non-conventional symbolism, and

he overextended the analogy between meanings and language (see Petocz, 1999, pp. 178–197). This was partly responsible for the later distortions of psychoanalysis by semioticians and hermeneuticists—for example, in Lacan's (1966) thesis that language is *the* subject matter of psychoanalysis, or in Ricoeur's (1965, 1970) assimilation of the Freudian "archaeology" into a tripartite constructivist system in which desire "is constituted only in the movement of interpretation" (1965, p. 254). These distortions reveal that the treatment of the distinction between causes and meanings is simply the tip of a very large iceberg of conceptual confusion, encompassing the supposed opposition between meanings and causes,[7] and the supposed inability of science to handle the intrinsically elusive, relative, unobservable and multi-faceted "plurivocality" of human symbolic expression and action.

The standard response to this category, from both defenders and other critics like Grünbaum (1984, 1993a) and Macmillan (1991), is to point out that this kind of hermeneutic reconstruction of psychoanalysis is just a last-ditch attempt to salvage Freud, but that it fails; first because Freud himself placed his theory firmly within the scientific *Weltanschauung* (cf. Freud, 1912g, 1933a) and second because Freud's theory quite clearly shares with science an interest in discovering causes.

In terms of argument strategy, these assertions, while true, fall short. The first is irrelevant; just because Freud claimed his theory to be scientific, that does not make it so. The second accepts without scrutiny the widespread assumption, an assumption that is also well entrenched in psychology, that a theory that deals with meanings is thereby not scientific; Freud *was* interested in causes, and so his theory *is* scientific. But where does that leave the theory's focus on meanings? Must meanings come along for the ride as superfluous, nonscientific baggage? Do they haunt the theory and compromise its scientific status? Not at all.

With respect to this charge against psychoanalysis, I agree with Grünbaum (1986b, p. 280) that "the sense in which psychoanalysis is an 'interpretative' endeavour lends no support at all to the philosophical theses of those who offered the hermeneutic reconstruction". To reply to this charge, all that is required is to show: (1) that science is concerned with investigating (via description and explanation) the structures of natural (including human) systems, (2) that human systems include psychological states and meaning relations, and (3) that, therefore, scientific psychology must include both causes and meanings.[8] Hence, the attempt to "rescue" Freud's theory as hermeneutics fails not only

because science and hermeneutics are not mutually exclusive (cf. Holt, 1972; Wallerstein, 2006), but because scientific psychology necessarily includes both. Any psychological theory that excludes meanings forfeits its claim to science. By this criterion, psychoanalysis is scientific, whereas radical behaviourism is not—a conclusion that may come as a surprise to mainstream academic psychologists.[9]

Category Two: psychoanalysis is unscientific because it has been discredited by the behaviour of its proponents

This is a ragbag category of charge. Psychoanalysis is unscientific because it has led to bad things like "repressed memory syndrome" (cf. Esterson, 1998), and its proponents (including Freud) are schemers, liars, manipulators, factionalists, and so on. It might seem that this charge is the easiest to dismiss, since ad hominem argument is generally acknowledged to be invalid. Why, then, is this category so prominent in critiques of psychoanalysis, especially in psychology?

The answer is that it is like advertising—or, in experimental psychological jargon, evaluative conditioning. We may know very well that the sexy blonde posing on the bonnet of the car has nothing to do with the car's intrinsic properties or performance, and we may chuckle at the blatancy of the advertiser's attempt to manipulate us. But the "feel-good" associations are nevertheless set up in our minds and have begun their subtle persuasive work regardless of our conscious dismissal. And how much more successful is this type of non-rational form of persuasion when we are all too ready to be persuaded? So, then, a good way to cast doubt on the scientific status of psychoanalysis is to employ the implicit seduction of ad hominem argument while combining it with explicitly acknowledging the logical fallacy and embedding the charge amongst others that are not ad hominem:

> "Is psychoanalysis a science?" This question has reemerged in the past few years, particularly because of the discord around the secrecy surrounding the Freud archives and the biographical studies of Freud's changing position on the seduction theory. (Klerman, 1986, p. 245)

Why, we might ask, has the question emerged particularly because of these events? How is the secrecy surrounding the Freud archives

relevant to the question of the truth or falsity of the theories set out in the twenty-three volumes of Freud's published works? Then Eysenck (1985):

> The use of the *argumentum ad hominem* as a reply to critics is the last resort to those who cannot answer criticisms factually, and is not taken seriously in scientific discourse … Freud's theories have to be tested by observation and experiment, and their truth or falsity objectively determined … Historically and biographically Freud's background may be of interest, but from the point of view of the truth function it is not! … If it is true that he framed his theory of the Oedipus conflict on his own infantile experience, this is important and *relevant to a judgement of the theory*. [emphasis added] (p. 12)

How, we might ask, is the origin of Freud's Oedipus conflict theory relevant to a judgement of that theory? Perhaps, as Leahey (2013) claims, "having fabricated an early memory of sexually desiring his mother and fearing his father, Freud concluded it was a universal experience …" (p. 295). Perhaps Freud's "memory" was a false one, and nobody else has ever had any so-called oedipal conflict? But what is involved in judging this theory? First we subject it to logical test, asking whether it is clear and coherent. Here its origin is irrelevant. Then, if it passes the logical testing phase, we subject it to empirical test and ask whether it is true. That requires us to look and see; it requires evidence.[10] Here, again, its origin is irrelevant.

There are some quite subtle and apparently innocuous versions of this charge. Leahey (2013) refers to Wittgenstein and Kraus as "Two fellow Viennese who were never fooled by Freud" (p. 295). Grünbaum (this volume) begins "As Freud put it quite dogmatically", and then quotes Freud's (1901b) tentative statement "the *tendency* to forget what is disagreeable *seems to me* to be a quite universal one" (p. 144, emphasis added). Later, he adds: "Astonishingly, Freud thinks he can parry this basic statistical and explanatory challenge by an evasive dictum as follows", and then cites Freud patiently cautioning his readers not to misread him as claiming that everything negative is always forgotten. By then, of course, the carefully selected adverbs and adjectives have already done their job. Finally, Crews (1993) suggests that belief in psychoanalysis rests on faith in Freud as a rigorously objective

thinker, yet he "lacked the equanimity to act on his key methodological principle" (p. 59), and instead influenced the path of free association. He continues:

> To take note of Freud's unsuccess with individual patients … leaves the working assumptions of psychoanalysis largely uncompromised. There is always the possibility that Freud simply had little aptitude for therapeutically applying his perfectly sound principles. But if … we uncover grave flaws of reasoning or even outright fraudulence behind the cases that supposedly compelled Freud to adopt those principles, the stakes of the game are considerably raised. (Crews, 1993, p. 61)

Yes, the stakes are raised. But they were pretty low to begin with. So, Freud lied. He lied, for example, about the success of his therapy (the Wolf Man was not "cured"), and who (if anyone) had reportedly seduced his patients in their childhood. But even if Freud had lied about *all* of his intra-clinical observations it would mean only that what he claimed to be evidence for his theory from his own clinical practice was not in fact evidence. And in the unlikely event that he lied about *everything* (except, presumably, about what he believed were the propositions of his theory), we would still have a theory, but one not supported by any observations from Freud himself. And, of course, as many defenders hasten to add, Freud also appealed widely to existing observations in extra-clinical material. The point is that if the claims of the theory are conceptually sound, then it is an empirical matter whether or not those claims are true; and it is an empirical matter how much evidence there is for or against those claims.

I suspect that many critics who use this category have already concluded that the charge in Category 4 is proven. That is, Freud's theory is false, so we are interested in explaining why he came up with such absurdities; how he managed to recruit fellow believers, twist the evidence, etc.

Category Three: psychoanalysis is unscientific because it is in principle unfalsifiable

This Popperian charge is the most famous of them all. Ironically, although it is dismissed by the rest of Freud's major critics, all of whom

argue (albeit not consistently) that Popper is mistaken,[11] it has been kept alive in psychology and in popular circles.

There are three standard responses to this charge. The first is to note its frequent undermining by the "unfalsifiablity in practice" thesis (Category 5). The second is to point out that psychoanalysis is not a monolithic theory but a collection of hypotheses, some falsifiable, some not. The third is to list a number of occasions on which Freud abandoned hypotheses because of contrary evidence, showing that those hypotheses were indeed falsifiable.

The first response is illustrated in Cosin, Freeman, and Freeman (1982), replying to Cioffi (1970):

> There may seem to be something puzzling here. The 'pseudo-sciences' which Cioffi brackets with psycho-analysis, and which include clear cases of non-science such as a conceit of Dante's and a theological aberration of Newton's, are such that 'confrontation with facts' is not a danger for them (nor even imaginable in many cases). But for psycho-analysis, confrontation with the facts is admitted to be a real danger, which is continually warded off by conventionalist manipulation of theory. In this case, for conventionalist stratagems to be necessary, there must be some facts with which the theory *could* be confronted, which would entail its being empirical at least in some minimal sense. So why does Cioffi present conventionalist manipulation as an argument for the *non*-empirical nature of the theory? (Cosin et. al., 1982, p. 35)

Quite so. As Cosin et al. note, accusations concerning the use of conventionalist manoeuvres, also mysteriously levelled by Popper (cf. Grünbaum, 1993b), thereby forfeit legitimate appeal to the Popperian unfalsifiability charge.

Examples of the second response come from Farrell (1961) and from Kline (1972). Farrell argues that it is misleading to ask "Can psychoanalysis be refuted?", because it is not a monolithic whole, but a collection of hypotheses, some of which (for instance, the theory of instinctual drives defined in terms of their physiological sources) are falsifiable, and others (for example, the metapsychology of psychic structure in terms of the id, ego, and superego), in being a mere façon de parler, are indeed unfalsifiable, albeit not in the way the critics of psychoanalysis would wish. Kline (1972) agrees that psychoanalysis is not a unitary

theory, that it has many parts, and that while much of the Freudian metapsychology (e.g., the death instinct, the pleasure principle) is unfalsifiable, the rest consists of empirical propositions that are not.

This response, while accurate in pointing to the non-monolithic nature of the theory, concedes too much. For the metapsychology, insofar as it consists of metaphors, poses the problem of "cashing out" those Freudian metaphors (Petocz, 2006).[12] For example, on one reading of Freud's earlier views (cf. Maze, 1983, 1987; Petocz, 1999), the id and the ego are terms for sets of instinctual drive activities, which are ex hypothesi falsifiable.

Examples of the third response are Caplan's (1986) comment that "there are plenty of instances of falsified hypotheses to be found littering the Freudian and neo-Freudian corpus" (p. 229), and Gardner's (1993) list of cases that constitute "incontrovertible evidence of Freudian theory's hospitality to falsification" (p. 236).

This last strategy seems to me to get nowhere. For, to the extent that a falsified hypothesis is evidence for the falsifiability of the theory, it is also evidence for the *falsity* of the theory, which is not what Freud's defenders wish to maintain. Thus, any demonstration of falsifiability that proceeds via selection of actually falsified hypotheses must *either* allow that the theory is therefore false, or concede that the remainder of the theory, which now comprises "psychoanalysis", has not yet been shown to be falsifiable via having been falsified. And so on, in a vicious sequence that permanently leaves the question of the falsifiability of the (remainder of) the theory as yet undemonstrated.

How, then, to respond to the charge? First, Popper nowhere demonstrates its truth. Second, that is hardly surprising, since there is a readily available rebuttal.

When discussing falsifiability in general Popper appears confused. In a trenchant exposé of Popper as the fountainhead of the scientific irrationalist stance normally attributed to Kuhn and Feyerabend, Stove (1982) uses an example drawn from Popper's (1959) own writings. Take a statement like "The probability of a human birth being male = .9". Popper maintains (1) that some such propositions are scientific, (2) that none of them is falsifiable (i.e., inconsistent with some observation), but also (3) that only falsifiable propositions are scientific. Stove observes that Popper calls the conjunction of these three propositions a "problem", when, in fact, it is a contradiction, and that Popper's solution to the problem is to say what scientists "usually do" (viz. the methodological

principle of disregarding extreme improbabilities). Stove responds that, "as a solution to Popper's problem, this is of the kind for which old-fashioned boys' weeklies were once famous: 'With one bound Jack was free!'" (1982, p. 29). Yet it is true that "unfalsifiable" probability statements of the form in the above example are everywhere to be found in standard hypothesis testing procedures in experimental scientific psychology.

When it comes to justifying the charge against psychoanalysis, Popper (1963) does not talk of probabilities; instead, he presents a contrived example (p. 35) of a man who pushes a child into the water to drown it, and a man who sacrifices his life in an attempt to rescue the child, and he complains that each of these two cases can be explained with equal ease in psychoanalytic theory. If this is supposed to be a demonstration of the unfalsifiability of psychoanalysis, then, as Grünbaum (1979, 2008) has pointed out, the attempt is risible.

In any case, there is a readily available, straightforward rebuttal of the charge. As Gardner (1993) acknowledges, unfalsifiability is a logical feature of a theory. If the theory's assertions are tautologous or self-contradictory, then they are unfalsifiable in being consistent with any actual or possible observation. In contrast, any contingent statement is logically ("in principle") falsifiable in that by asserting something to be the case it rules out its contradictory, observation of which would falsify the statement.[13] All that is required of the defender is to show that the theory is couched in genuinely contingent assertions. Any example of a psychoanalytic proposition (such as that everyone is born with a set of instinctual drives) precludes its contradictory (not everyone is born with a set of instinctual drives), and so is logically falsifiable.

Given that psychoanalysis is indeed a collection of contingent statements, the question of the truth or falsity of those propositions, and the possible difficulties of determining the answer, are separate issues. This takes us to the remaining categories.

Category Four: (Continued adherence to) psychoanalysis is unscientific because it has been falsified

This charge covers two claims: the stronger claim that the propositions of psychoanalytic theory have been shown to be false; and the weaker claim that psychoanalytic theory is not supported by the evidence. The focus has been both on therapy, with Freud's so-called "tally

argument"[14] being the major target of Grünbaum's (1984) critique, and on experimental studies of Freud's theory. With respect to therapy, Eysenck says:

> When I first pointed out in 1952 that "the emperor had no clothes", I took it for granted that the relevance of the therapeutic outcome argument to Freudian theory would be obvious; clearly I was mistaken, and it needed Grünbaum's patient unravelling of the skein to drive the point home … I have always taken it for granted that the obvious failure of Freudian therapy to significantly improve on spontaneous remission or placebo … is the clearest proof we have of the inadequacy of Freudian theory, closely followed by the success of alternative methods of treatment, such as behavior therapy … This argument has not on the whole been favorably received by psychoanalysts, who continue to state that the theory may very well be right, even though the treatment may fail and other methods of treatment, like behavior therapy, may succeed … *It is difficult to see how psychoanalysts can continue this* argument. [emphasis added] (1986, p. 236)

My reply is that it is not at all difficult to see how psychoanalysts can continue the argument; they do so because it is valid; they recognise that therapeutic failure (or success) does not entail the falsity (or truth) of the theory, nor conversely (Petocz, 2004). Nevertheless, in popular evaluations of psychoanalysis there is a focus on cure (viz. removal of symptoms) as a validator of the theory, and a heavy concern with a neo-Grünbaumian attack. Freud did early on claim that his theory was vindicated by the success of therapy, though he later withdrew that claim, and, as Cioffi (1970) points out, did not place cardinal importance on the "tally argument". In my view, psychoanalytic theory offers plenty of material from which to derive the prediction that analysis may *not* be successful in some cases. And if the theory includes hypotheses about "what works", then part of that will be why placebo and suggestion, for example, may "work". But that is not to deny the evidence for its efficacy. Shedler (2010) has recently reported on a series of recent meta-analyses whose combined results suggest that (1) empirical evidence supports the efficacy of psychodynamic psychotherapy, with effect sizes "as large as those reported for other therapies that have been actively promoted as 'empirically supported' and 'evidence based'" (p. 98), (2) "Especially noteworthy is the recurring finding that the benefits of psychodynamic

therapy not only endure but increase with time" (pp. 101–102), and (3) "nonpsychodynamic therapies may be effective in part because the more skilled practitioners utilize techniques that have long been central to psychodynamic theory and practice" (p. 98). Shedler concludes that "The perception that psychodynamic approaches lack empirical support does not accord with available scientific evidence, and may reflect selective dissemination of research findings" (p. 98).

The second focus has been on experimental studies of Freud's theory. As illustrated in the polarised set of conclusions I presented earlier, the critics insist that there is little or no evidence to support Freud's theories, and the defenders insist that there is plenty of such evidence. According to Eysenck (1985), "when Freudian theories are subjected to experimental or observational tests, the results do not support them; they fail the test" (p. 14) (cf. Eysenck & Wilson, 1973). Erwin (1996) agrees that there is not enough experimental evidence to warrant thinking the theory to be true. In contrast, Fisher and Greenberg (1977, 1996), Kline (1972, 1988) and Westen (1998, 1999) argue that there is a wealth of experimental data—thousands of objective studies of psychoanalytic theories. Several of the respondents to Grünbaum (1986b) expressed dismay at his silence on the volume and accessibility of experimental evidence, some from clinical material, most from extra-clinical (including epidemiological and anthropological) sources, relevant to the merit of the theory, and this charge against Grünbaum continues. It is not my intention here to evaluate that evidence, although it is very likely that much of it, just like the counter-evidence, is flawed,[15] simply because much experimental work in any area of psychology is flawed.[16] However, in this particular area the goal posts appear notoriously movable. When the evidence supports the hypothesis (e.g., regarding the efficacy of unconscious processes, defence mechanisms, dreams, symbolism) the critics reply that there is nothing specifically psychoanalytic about the hypothesis (citing, e.g., Ellenberger, 1970). In contrast, when the evidence fails to support the hypothesis (e.g., regarding the repressed homosexuality etiology of paranoia, or female clitoral *vs.* vaginal orgasm) the critics seize the opportunity to take unreasonable inductive leaps (e.g., the theses of repression and of sexuality per se are undermined). Now, as Hopkins (1992) points out:

> Scientific theories seem in general to be supported by their capacity to explain the data they cover, so that when we judge that a

> theory is confirmed, we are making an inference to the truth, or acceptability, of our best explanatory hypothesis. On this account, we support a scientific theory by showing that it explains certain things well and better than any other. (pp. 9–10)

As Hopkins implies, if there is more evidence in favour of a competing theory, then it would be irrational, ceteris paribus, not to abandon the original theory in favour of its more successful competitor. In this context, we might consider Crews's (1993) claim (following Macmillan, 1991) that "psychoanalysis has been left behind by mainstream psychological research" (Crews, 1993, p. 55). If what he means is that mainstream psychological research has produced evidence against psychoanalysis and in favour of competing theories, then the claim is false. The relationship between psychoanalytic theory and the findings of experimental psychology is unclear for at least two reasons: (a) few mainstream psychologists ever relate their findings to psychoanalytic theory; and (b) if a distinction is drawn between (1) the data produced in such research and (2) the interpretations of those data—that is, the conclusions inferred from (1), psychoanalytic theory is required to be compatible only with (1) not with (2) and, so, even in the rare instances where an incompatibility is claimed, it is with (2), and the relationship with (1) remains unclear. Moreover, in the mainstream psychological research areas of cognitive psychology, social and developmental psychology, cognitive neuroscience, learning, perception, and so on, (cf. Westen, 1998), there is little clear specification of the theory being tested, let alone how it might constitute an alternative to psychoanalysis.

Here, Grünbaum argues (this volume) that the major differences between the unconscious processes investigated in contemporary cognitive psychology and those postulated in the dynamic unconscious of Freudian theory mean that the existence of the cognitive unconscious, for which there is substantial empirical evidence:

> clearly fails to support, or even may cast doubt on, the existence of Freud's psychoanalytic unconscious. His so-called "*dynamic*" unconscious is the supposed repository of repressed forbidden wishes of a sexual or aggressive nature, whose reentry or initial entry into consciousness is prevented by the defensive operations of the ego. (p. 5)

Grünbaum continues by noting that the cognitive unconscious cannot be brought to consciousness, since it involves "elaborate scanning or search processes". And he concludes: "By glossing over the stated major differences between the two species of unconscious, some psychoanalysts have claimed their compatibility within the same genus without further ado" (p. 6).

In my view, Grünbaum's position on this is a telling mixture of confusion and insight. To begin with he (along with armies of experimental psychologists) overlooks the fact that the unconscious of mainstream cognitive psychology includes and often switches across three versions; the first irrelevant, the second incoherent, and only the third coherent. The irrelevant version is the neurophysiological processes that underlie mental activity—what is identified by Oppenheim (2012, p. 283) as nonconscious, and by Northoff (2012) as the "principal unconscious (a)" (p. 271) or the "principal nonconscious (a)" (p. 273) (where (a) stands for lack of access); of course these go on outside our level of awareness, as do so many other biological and physiological processes. The incoherent version is the computational information processing mechanisms (scanning, accessing, extracting, comparing, transforming, inferring, etc.) that supposedly underlie all mental activity. These processes are the stock equipment of the representationist epistemology that mistakenly adopts the computer model of the mind (whether of the symbolic, neural network or dynamical systems type) and widely conflates physiological with epistemological mediation (see Bennett & Hacker, 2003; Mackay & Petocz, 2011a, 2011b; Petocz, 2011b, pp. 119–121; Petocz & Mackay, 2013). Since representationism is conceptually incoherent (cf. Bickhard, 1996; Heil, 1981; Maze, 1991; McMullen, 2001, 2011; Michell, 1988), there cannot be any empirical evidence for the unconscious cognitive mechanisms that it postulates. In contrast, the coherent version of the cognitive unconscious, for which there is much empirical evidence, is that of mental processes of which the person is unaware (e.g., my perceiving and reacting to a changed traffic light while my attention is directed elsewhere; my perceiving a subliminally presented word while remaining consciously unaware that I have done so).

I agree with Grünbaum that this coherent version of the cognitive unconscious of experimental psychology is at odds with Freud's favoured conception of the unconscious as one that comprises a special "system" with its own special characteristics and (so-called "primary process") modes of operation (Freud, 1915e). But I have argued when

discussing what I have called the "problem of the system unconscious" (see Petocz, 1999, pp. 151–177) that Freud's qualitative ("systematic") view of unconscious mentality, widely embraced and developed within psychoanalysis (cf. Edelson, 1972; Foulkes, 1978; Gardner, 1993; Jones, 1953; Matte Blanco, 1975), is conceptually untenable for a number of reasons, and cannot do the theoretical job of sustaining Freud's equally favoured "dynamic" concept of repression; it would sustain only the "structural" notion of repression. However, I have also shown that there is in Freud's material an alternative ("relational" or "epistemic") view of unconscious mentality (Freud, 1912g, 1915e, 1923b, 1933a), according to which "an unconscious process is one *of which we are not aware* [emphasis added]" (Freud, 1912g, p. 260). On this relational view, "every mental process … exists to begin with in an unconscious stage or phase" (Freud, 1916–1917, p. 295), because for it to become conscious requires an additional mental act of awareness (see also Boag, 2012). As I have summarised it:

> To identify a particular mental act as unconscious, then, is not to say anything about its intrinsic nature, but only to say something about its relations (i.e., that it is not known). (Petocz, 1999, p. 163)

Thus, there are no "special characteristics" of unconscious processes:

> it is not that the unconscious is 'timeless', 'exempt from contradiction', etc., but simply that the inexorability of time, the fact of contradiction, etc., are often unconsciously ignored or denied. (Petocz, 1999, p. 168)

In this respect, therefore, the psychoanalytic unconscious is consistent with the cognitive unconscious. However, while Grünbaum is correct that "evidence for the cognitive unconscious does not, as such, also furnish support for the dynamic unconscious as such" (this volume), that is not because the two types of unconscious are radically different, but because the dynamic unconscious includes a causal story about the motivational origin of the particular unconscious content. According to Freud (1923b), "the act of perception itself tells us nothing of the reason why a thing is or is not perceived" (pp. 15–16):

> The repulsion from unconscious ideas is only provoked by the tendencies embodied in their contents … every psychical act

> begins as an unconscious one, and it may either remain so or go on developing into consciousness, according as it meets with resistance or not. (Freud, 1912g, p. 264)

Of course, the psychoanalytic focus (in contrast to mainstream cognitive theory) is on the external question why something is unconscious, and it looks further at the kinds of content that a person has good reason to want to avoid. Here, then, the vast body of experimental demonstration of the existence of unconscious material whose content is unpalatable becomes evidence for the Freudian unconscious (see Westen, 1998, 1999).

So, then, when Eysenck (1985) asserts that "Freudian theories … have proved invulnerable to any amount of factual evidence demonstrating their absurdity" (p. 192), the mystery is what and where exactly is all this factual evidence.

Category Five: psychoanalysis is unscientific because it is based on methodologically defective procedures, some of which make it unfalsifiable in practice

This "umbrella" category covers three different procedures. The first (already mentioned) is the use of the conventionalist stratagem to make the theory unfalsifiable in practice (e.g., Cioffi, 1970). The second is the reliance on clinical data, which are "fatally contaminated" either by suggestion or by a priori theory-dependence (e.g., Crews, 1993; Grünbaum, 1993a; Macmillan, 1991). The third is the use of the technique of free association to identify causes, a method that either involves invalid reasoning (e.g., Grünbaum, 1993a, 2008; Wittgenstein, 1932–1933) or is derived from a faulty conception of causality (e.g., Macmillan, 1991). I shall consider each of these in turn.

(i) The conventionalist stratagem

This comes in two versions, a weaker and a stronger. The weaker version involves accusing Freud and his followers of persistent and wilful attempts to avoid confrontation of theory with facts—by, for example, adding auxiliary hypotheses, or maintaining that the tests were not subtle enough. The stronger version claims that such manoeuvres are enabled by the propositions of the theory—that is that insulation

against possible falsification is somehow built into the theory (e.g., in the concepts of resistance, reaction formation, and ambivalence), which inevitably leaves the critic or potential falsifier in a catch 22 ("heads I win tails you lose") situation.

The standard response to the weaker version of this charge is to point out that the use of the conventionalist stratagem, following the so-called "Quine-Duhem thesis", is often acceptable as good scientific practice, especially given the typical underdetermination of theory by data (Greenwood, 1992, p. 140). The fact that the stratagem is open to abuse does not prevent it from being used sensibly; often, its application is not part of some attempt at legerdemain, but what the theorists in question have learned to see as the sensible application of doubt.

This response, though sound, does not go far enough. For often what makes the doubt rational is the fact that an earlier step in the procedure has been irrational; that is, the prediction supposedly derived from psychoanalytic theory is not actually a genuine prediction; it is not derivable from the theory. Using the conventionalist stratagem, therefore, corrects the faulty reasoning that has been responsible for the initial test. I suspect that Grünbaum's reason for concentrating on the tally argument is that it is a clear example of invalid reasoning advanced by Freud himself. But, usually, it is the critics of psychoanalysis who set up the test or offer the supposedly falsifying evidence, and it is they who have made the "predictions". In many of these cases, defenders need only reply with the countercharge of ignoratio elenchi.

With respect to the stronger version of the criticism, the built-in immunisation in some psychoanalytic concepts, consider Eysenck's (1985) claim (cited earlier), regarding Freud's "clever ploy" of treating the patient's agreement as confirmation, and disagreement as resistance, and Eysenck's conclusion that: "Clearly, there is no way in which the theory could be disproved" (p. 129).

To begin with, what Eysenck says about Freud's tactic is false, since Freud claimed only that either of these possibilities might be operating under certain conditions (see Freud, 1937d, pp. 257–265). Hence, immunisation is hardly built into the theory; any psychoanalyst who did treat agreement as always confirmation and disagreement as always resistance would be misunderstanding the theory. Furthermore, Eysenck's argument is invalid. Even if Freud had made such a stipulation, it does not follow that "there is no way in which the theory could be disproved". All that follows is that this particular way (i.e., observing whether the patient

agrees or disagrees with the interpretation) cannot be used, in isolation, to confirm or disconfirm the theory. The same points can be made mutatis mutandis for the accusation of built-in immunisation in the concepts of ambivalence, reaction formation and resistance in general.

(ii) The appeal to "fatally contaminated" clinical data

The second item in the "methodologically defective procedures" category is the claim that clinical data cannot be used in the testing of psychoanalytic theory, because they are fatally contaminated. Here, too, there is a weaker and a stronger version. The weaker form is suggestion; the patient complies with the analyst in making the kinds of observation that the analyst wishes to hear (saying, for example, that he has been seduced in childhood, when he hasn't). The stronger form is the implicit assumption via the a priori interpretation of "raw data" in psychoanalytic terms of what is supposedly being confirmed by those data; whatever the patient says, it is guaranteed to be relevant and confirmatory.

In response to the accusation of suggestion, defenders point out that Freud made countless other observations that he drew from material over which he could not possibly exercise suggestion, so that suggestibility is peculiar to the clinical data. In any case, suggestion is just another possible confounding variable, but not an inevitable and automatic one, and certainly not a logically insurmountable problem; it is a matter of evidence (Spence, 1986, p. 259).

Another response to this accusation is to highlight the importance of context. For example, Glymour (1982, pp. 30–31) has argued that there is an identifiable strategy in Freud's case studies that allows us to accommodate the possibility of suggestion, but does not require complete abandoning of clinical data; it all depends on when and under what conditions the relevant material emerges. With respect to this important role of context, Wollheim (1993b) has provided the strongest rejoinder to the suggestion charge. He draws attention to Grünbaum's neglect of the psychological structure (the "infilling"), which, in the theory, mediates various putative correlations. Because Wollheim's points are worthy of more attention than they have received in the literature, I quote him extensively:

> One of the key reasons that Grünbaum has for thinking that Freudian theory is clinically unconfirmable is that it is impossible

> to free the evidence from the taint of suggestion by the analyst. Suggestion, or suggestibility, remains in as a possible explanation of—and note the different areas in which this hypothesis is entertained—why the patient recalls his past as he does, why he free-associates as he does, why he recounts his dreams as he does, indeed why he dreams as he does, and, most sobering of all, why he gets well if he does … suggestibility … gradually escalates. Its claims upon our credence grow: its content is inflated. Soon it appears as an alternative theory to psychoanalysis … Indeed, so powerful is this alternative theory, and so grotesque are the workings of the mind that it postulates, that Grünbaum readily slips from the term 'suggestion' to what he treats as by now its synonyms: 'indoctrination' and 'brainwashing'.
>
> But what is interesting is that, for all the serious attention that Grünbaum asks *us* to give this alternative theory, he never for a moment thinks that its plausibility requires *him* to give an account of how suggestion by the analyst would engage with the patient's psychological structure. He never proposes, nor feels the need for, any infilling when he invokes the possibility, indeed the likelihood, of suggestion as the real explanation for what the patient does or says. In the absence of such infilling, the situation is envisaged in the following way: (one) the analyst makes his wishes known; (two) the patient complies. It is a sign of just how far apart Grünbaum and Freud are in their conceptions of how psychological theory gains plausibility, that Freud, who also thought that suggestion could not be conclusively dismissed as the explanation of why the patient got better, never ceased to chip away at the problem of what the underlying mechanisms could be on which suggestion depended. Freud thought that, if suggestibility was to be an alternative theory to his own, it must be a theory, a psychological theory. (Wollheim, 1993b, pp. 110–111)

With respect to the stronger form of fatal contamination, Crews (1993) says:

> No one has been able to mount a successful defence against the charge, most fully developed in Adolf Grünbaum's meticulous *Foundations of Psychoanalysis* (1984), that "clinical validation" of

> Freudian hypotheses is an epistemic sieve; as a means of gaining knowledge, psychoanalysis is fatally contaminated by the inclusion, among its working assumptions and in its dialogue with patients, of the very ideas that supposedly get corroborated by clinical experience. (p. 55)

This is the clinical version of the related general accusation that psychoanalysis uses a "finalist" interpretative strategy (for example, in the interpretation of symbolism), one which masquerades as an unbiased journey of "discovery", but which is guided by an "a priori codification of the results to be obtained" (cf. Todorov, 1977, p. 253). To this I have replied that Freud's "finalism" is a realist, empiricist one, not an idealist, rationalist one, and that, provided the discovered meanings are given independent validation from a psychological theory of the cognising subject, their interpretation "is not a question-begging exercise (as it is in the case of the 'discovery' of the Holy Trinity via Patristic exegesis)" (Petocz, 1999, p. 257).

But here, too, Wollheim's reminder about the role of "infilling" in psychoanalytic theory serves as a reply to the charge of circularity:

> [I]f what the patient says or does is to be brought to bear upon the hypothesis under consideration so that it, the hypothesis, can then be said to have been tested on the couch, the patient's material will in most circumstances have to be subsumed under categories deriving from psychoanalysis. It cannot be left in a "raw" state, otherwise the two will not match up: what the patient says or does will neither confirm nor disconfirm the hypothesis except in marginal cases. In saying or doing what he does, the patient has to be identified as, say, presenting *anal material* on a massive scale; resorting to *phantasies of omnipotence*; *assaulting* or *fragmenting*, or *idealizing*, the analyst's interpretation; *acting out*; and so on. In other words, the patient's material must be subsumed under transference categories: that is, categories which capture what the person is doing *vis-à-vis* the analytic situation as he phantasizes it. In all this there is, of course, no circularity so long as that part of psychoanalytic theory which furnishes the categories under which the patient's material is subsumed is not identical with that part of the theory which is under test. Psychoanalytic theory,

> though an interlocking whole, is not monistic … (Wollheim, 1993b, p. 108)

In summary, there is neither inevitable nor fatal contamination in the clinical method:

> Grünbaum … has an inadequate view of what it is that must be clinically tested if anything properly called psychoanalysis or psychoanalytic findings can be said to have been tested on the couch. He has this inadequate view because he fails to do justice to the structure that Freud attributed to the mind and that, according to his theory, mediates the correlations upon which Grünbaum concentrates. (Wollheim, 1993b, p. 107)[17]

(iii) Using free association to identify causes

The third methodologically defective procedure is the use of free association to identify pathogenic causes. Following Wittgenstein (1932–1933), Grünbaum argues that, even if clinical data were not contaminated by suggestion, "the reasoning on which he [Freud] rested the main hypotheses of his clinical evidence was fundamentally flawed" (1986a, p. 220). Specifically, there are two flaws.

The first is that the move from associations to causal attributions is logically flawed because:

> Freud's conclusion as to the motivational cause had asserted an order of cause and effect that is the *reverse* of the causal order exhibited by the free associations. The fallacy was to infer that a repression which emerges at the end of a chain of free associations—as its *terminus ad quem*—was actually the original cause of the symptom that initiated the chain as a *terminus a quo* … There is nothing that justifies inverting the direction of causation. (Grünbaum, 1986b, p. 277)

Grünbaum repeats this charge (this volume), accusing Freud of the "fallacy of crude hypothetico-deductive pseudo-confirmation", for "it is unavailing to the purported *etiologic* probativeness of free associations that they may lift repressions, since Freud failed to show that the latter are pathogenic". The charge here appears to be a case of the "causation intervention" fallacy; if, for example, a child throwing a

tantrum stops when I give him a chocolate, I cannot validly conclude that my intervention targeted the original cause of the behaviour—that is, I cannot validly conclude that the tantrum was caused by chocolate deficiency. That's true. However, the two situations are quite different in an important respect. From the perspective of the child's tantrum and its cause, my chocolate intervention is an external fortuitous act, albeit based on my knowledge of children's liking for chocolate and my inductively formed belief that offering chocolate will cause them to stop crying. By contrast, the situation of "freely" associating is not an independent fortuitous event; it is analogous to the common case where, for example, I find myself humming a tune and, wondering what has caused me to do this, I trace back the sequence of thoughts to the original provoking stimulus. It is not a case of inverting the direction of causation, but, rather, of systematically recalling the causal sequence, beginning with the most recent.

This point is related to the second logical flaw, which is that free associations reveal, at best, "thematic affinity" and "thematic affinity alone is not evidence for a causal connection" (Grünbaum, 1986a, p. 225). Grünbaum takes this to be so serious that, when discussing the future prospects of psychoanalysis (this volume), he repeats his earlier (2008, p. 583) citation of Meehl's (1995, p. 1021) verdict:

> His [Grünbaum's] core objection, the epistemological difficulty of inferring a causal influence from the existence of a theme (assuming the latter can be statistically demonstrated), is the biggest single methodological problem that we [psychoanalysts] face. If that problem cannot be solved, we will have another century in which psychoanalysis can be accepted or rejected, mostly as a matter of personal taste. (Grünbaum, this volume, p. 35)

However, while it is true that mere thematic affinity does not per se license causal inference, Freud never proceeded as if it did; his causal inferences include aspects of the theoretical "infilling" discussed by Wollheim. Part of that is that there are different types of thematic affinity; in some cases the affinity has ipso facto prompted causal connection in the mind of the person; in other cases the affinity holds precisely because of a (contingent) causal connection. Grünbaum correctly (this volume) charges that "The 'hermeneutic' reconstruction of psychoanalysis slides illicitly from one of two familiar senses of 'meaning' encountered in ordinary discourse to another" (p. 30). That is, in

terms of types of sign-relation (Peirce, 1932), Freud deals with icons and indices (i.e., with similarity-driven substitutes and naturally occurring, causally based indicators), rather than with linguistic symbols and the relation of designation:

> Clearly, the relation of being a manifestation, which the symptom bears to its cause, differs from the semantic relation of designation, which a linguistic symbol bears to its object … For my part, in this context I would wish to forestall a semantic misconstrual of the perniciously ambiguous term 'symbol' by saying: In virtue of the similarity of shape, the snake *causally* evokes the unconscious image of a feared penis; thereby the snake itself becomes the dreaded object. (Grünbaum, this volume, pp. 29–30)

My own analysis of symbolism as involving a complex kind of motivated mistaken identity (Petocz, 1999, pp. 185, 195, 232–235) is in complete agreement with Grünbaum here. However, when discussing "thematic affinity", it is *he* who appears to be sliding illicitly between two different notions of "meaning" or "signification"; the type of thematic affinity that is revealed via free association is exactly the kind that is involved either in causally connected indicators or in causally connected similarity-based symptoms, and not in designators (the free association moves from snake to—either immediately or eventually—penis). For example, a footprint in the sand and the shape of the corresponding foot will display a "thematic affinity" for the very good reason that they are causally connected. As Grünbaum notes, we must, of course, have experience of that causal connection in order to act on the basis of the thematic affinity. But that experience is exactly what is exploited in Freud's treatment of thematic affinity. Thus, precisely because psychoanalysis does *not* deal with linguistic semantics but with icons and indices that involve causally connected signifier-signified relations and non-semantic motivated, mistaken identity based on perceived similarity of form or function, thematic affinity becomes a powerful indicator for the existence of causal connections in the mental realm (see also Opatow, 1999).

With respect to such mental causal connections, Macmillan (1991) argues that the material that free association provides cannot throw light on causes, because of Freud's faulty conception of causality. Reliance on the method of free association reflects "Freud's belief that psychological phenomena had internal determinants" (Macmillan, 1991, p. 551). Thus:

> for Freud, mental processes were continuous and governed by a psychic determinism in which associations, causal connections, and logical relations were identical. (Macmillan, 1991, p. 588)
>
> Freud does not identify the ideas recovered by free association as the causes of dreams, parapraxes and symptoms simply because he made a *post hoc ergo propter hoc* confusion and neglected to consider suggestion, as Grünbaum has argued … Freud was not quite so simple minded … what seems to have escaped Grünbaum is that free association produced only a putative cause which Freud then evaluated with his (deficient) adaptation of Koch's postulates. It was Freud's concept of determinism which had it that the ideas to which associations led were causes. (p. 594)

From what Macmillan says in this context and in the rest of his (1991) book, it is clear that he thinks ideas (viz. mental states in general) are not the sort of thing that can be causes. This difficulty with the notion of mental causation is widespread and is closely related to the issue of reasons *vs*. causes that lies at the heart of debates in the philosophy of mind concerned with treatments of rationality, intentionality, and the explanation of human action (e.g., Davidson, 1980, 1982; Goldman, 1970; Hopkins, 2012; Searle, 1980). Because the human sciences invoke reasons to explain behaviour, whereas the natural sciences deal only with causes, it is thought either that reasons cannot be causes, or that they cannot easily be accommodated in a world of causes (cf. Dretske, 1988).

Now, according to Holtzman (1985), "Reasons can be causes when such reasons make a difference to the occurrence of the event for which it is the reason" (p. 753; cf. Holt, 1981; Sherwood, 1969; Wallerstein, 2006). Grünbaum has no problem with reasons—even unconscious reasons—being causes, nor even with their not being ontologically reducible to neural states. *His* objection is the earlier one regarding thematic affinity, this time in terms of the content of the relevant mental states. In a cogently argued discussion of this issue, Mackay (1999) follows Hopkins (1988) in exposing Grünbaum's inconsistency:

> Grünbaum argues that because Freud's explanations do not fit the form of commonsense, rational explanation, they cannot get legitimacy from that quarter. Psychoanalytic explanations must then be judged by the standards of normal scientific theory and

> established inductively through evidential support. Moreover, Grünbaum goes on to cast serious doubt on the evidential support for psychoanalytic hypotheses about the unconscious determination of behaviour, and so confines psychoanalytic explanations to a sort of limbo status where they have neither cogency by virtue of being part of commonsense, rational explanation nor (at least so far) empirical scientific support … What Grünbaum does argue is that in the pseudo-rational form of explanation used by Freud … there is only thematic affinity—a similarity or linkage in content, though insufficient to meet the demands of the syllogism—to relate the hypothesized unconscious reasons to the dream symptom or behaviour to be explained, and this is not good enough for scientific explanation.
>
> Yet, it has been noted (Hopkins, 1988) that Grünbaum is prepared to identify the reasons given in properly constituted practical syllogisms as causes simply by virtue of their place in the syllogism, where the link between premises and conclusion *is* thematic. The explanatory value and causal nature of rational explanation are deemed transparent, and not in need of the sort of theoretically determined, inductive support that scientific hypotheses require to be counted as causal. (Mackay, 1999, pp. 7–8)

Yet, as Mackay goes on to discuss, the real problem is not *whether* reasons can be causes but exactly *how*. This involves the question "how a mentalistic theory would have to develop before its subject matter was admitted to the physical world in its own right" (Nagel, 1982, p. 238). As applied to psychoanalysis, it extends to concerns about Freud's dual commitments to a "quasi-neurological framework" and an "augmented Intentional psychology" (Pataki, 2014, p. 14), and Pataki rightly notes that in struggling with these commitments Freud is quite our philosophical contemporary. According to Mackay, "The common weakness in the literature on reasons as causes is that it is generally not clearly spelled out what reasons might be, nor how they might fit into the complex of causal relations that psychological theory requires" (1999, p. 9). He notes that the standard representationist account runs into difficulties by virtue of defining beliefs in terms of their propositional content, and he goes on to draw upon identifiable insights in Freud's psychoanalytic metapsychology supporting a realist approach to mentality and

its role in the explanation of behaviour (see also Mackay, 1989, 1994; Medlow, 2011; Petocz & Mackay, 2013).

Category Six: psychoanalysis is unscientific because it has poor possibilities vis-à-vis observation, prediction, and control

When J. B. Watson produced his famous manifesto for behaviourism (Watson, 1913) in the same year that Freud published "The claims of psycho-analysis to scientific interest" (Freud, 1913e), Watson announced that the subject matter of scientific psychology must be observable behaviour and its aims prediction and control. Ever since, it has been argued within mainstream psychology that, with respect to these requirements, psychoanalysis has poor possibilities. To begin with, since mental processes are supposedly unobservable in any case, how much more intractable are the elaborate internal mental constructs, both conscious and unconscious, postulated by Freud. Next, while logical deductions from the theory are possible, psychoanalysis cannot predict behaviour and a fortiori cannot be used to develop a programme of behavioural control. Finally, for these very reasons, psychoanalysis does not lend itself to controlled and rigorous forms of research of the type that is accepted scientific practice in psychology. As a result, because "it has not developed objective methods for testing the exciting ideas it had formulated earlier … psychoanalysis enters the twenty-first century on the decline" (Kandel, 1999, p. 505). I shall consider each of these points in turn.

First, the question of observational possibilities. Notwithstanding Grünbaum's dismissal of "the dubious, vague declaration that psychoanalysis is an 'extension' of common sense" (this volume), several theorists (Brakel, 2009; Davidson, 1980, 1982; Gardner, 1993, 1995; Hopkins, 1982, 1988, 1992, 2012; Mackay, 1989, 1999; Maze, 1983; Nagel, 1982; Pataki, 1996, 2000, 2014; Petocz, 1999; Wollheim, 1971, 1993a, 1993b) have argued that psychoanalysis is an extension of "ordinary psychological" or "commonsense psychological" explanation in ways that can be clearly and coherently specified. For example, Wollheim (1993b, pp. 94–102) describes in detail how Freud took the standard "desire plus belief" explanatory schema and extended it by deepening it (via the inclusion of unconscious mental states), elaborating it (via the inclusion of explanatory chains of associations, displacements, etc.), and contextualising it (via the inclusion of developmentally salient

constellations of beliefs and desires). In this way Freud uncovered the various interconnections between (conscious and unconscious) cognition, motivation and emotion, and showed systematically how the elaborations of the consummatory activities of the motivational structures, connected to the perceptual and cognitive apparatus and operating via symbolic substitutions and associations made by the person, can explain behaviour that would otherwise remain a complete mystery to the observer.

One of the stumbling blocks to appreciating this has been the way the concept of the unconscious is perceived, and, as I suggested earlier, Freud was partly responsible for this. However, Freud (1913e) also recognised that philosophers of mind seem to have difficulty with the idea of unconscious mental states (p. 178). For example, Talvitie (this volume) claims that the core feature of psychoanalysis is the concept of the unconscious, which rests on the assumption of the existence of mind involving the (dualist) conception of ordinary mentalism. Then there is Wittgenstein's view. As Wollheim points out, according to Wittgenstein "unconscious" is:

> no ordinary qualifier. It subverts the word it qualifies. It does so by snipping the word off from its standard evidential links with the world. There is, for example—and the example is Wittgenstein's own—no coherent way of understanding 'unconscious thought' as though it picked out a psychological entity that is just like a thought except that it happens to be unconscious. (Wollheim, 1993a, p. 91)

But, as I have argued (Petocz, 1999; and earlier in Category Four in response to Grünbaum's critique), that is exactly how an unconscious thought *is* to be taken—as the *Unbewusste*, the unknown. It is not, however, inherently unknown, or unknowable, but a relational unconscious (see also Boag, 2012); it can become known by means of a second mental act that takes the initial knowing as its object. On this view, unconscious beliefs and desires fit logically into the network of folk psychological explanation, as do their conscious counterparts, albeit not only in various subtle forms and complex combinations but also in ways that suggest extensions to the standard explanatory syllogism, and require detailed investigation. Nor is there any necessary connection between ordinary mentalism and dualism; although the radical behaviourists excluded mind from psychology because they were unable to conceptualise mind

in any other than a Cartesian dualist way, they were mistaken; there are materialist approaches to mind (e.g., Armstrong, 1968, 1997), including those that are non-reductionist, such as the realist relational approach (see Anderson, 1927; Chemero, 2009; Henry, 2009; Hibberd, 2009, 2010; Holt et al., 1912; Mackay & Petocz, 2011a; Maze, 1983; Michell, 1988; Petocz & Mackay, 2013; Tonneau, 2004).

Given these points about the psychoanalytic approach to mind and the unconscious, what observational possibilities are afforded to the researcher? Roughly, the same as those that are afforded to ordinary psychology. We make observations by asking people to report on what they believe, feel, do, would do, etc., by looking to see what they actually do and what happens to them, by asking others to report, by observing various physiological (gross and subtle bodily, neurophysiological, etc.) indices, and so on. We make use of available technological assistance to enhance our usual sensory abilities (audio and video recording, physiological and neurophysiological measures, eye-gaze equipment, etc.), and we analyse linguistic and paralinguistic phenomena (word choice, memory, intonation, etc.). Given the multitudinous ways in which unconscious motivations and social demand characteristics can enter the picture, the typical challenge in psychological research is to devise valid "implicit" forms of assessment. But, as Freud maintained, "He that has eyes to see and ears to hear may convince himself that no mortal can keep a secret. If his lips are silent, he chatters with his fingertips; betrayal oozes out of him at every pore" (1905e, pp. 77–78). Westen (1998) presents an extensive review of such "the body does not lie" research demonstrating support for what he identifies as the five major propositions of psychoanalytic theory.[19] This is not to imply that mental processes—even unconscious ones—are not directly observable and must always be inferred. If, as Pataki (2014, p. xiii) notes, "we cannot describe what is 'in' the mind without having to refer to what is outside the brain", then mentality *can* be directly observed (cf. Michell, 2011).[20] With all this, however, the difficulties in interpretation of data cannot be overlooked. As Gardner (1993) notes:

> psychoanalytic interpretation is seen to be directly in line with ordinary realism about psychological states, i.e., our view that their reality transcends our means of detecting conclusively their presence or absence … So, if we ever want to be able to say, as we do, that a piece of behaviour manifests a particular emotion but that it

> could have manifested something else, just as the emotion could have been manifested in other, diametrically opposed ways, then we have to accept enough slack and subtlety in our 'principles of reasoning' about other minds to warrant psychoanalytic interpretation. (Gardner, 1993, pp. 239–240)

The second complaint is that psychoanalytic theory cannot provide the kind of prediction that might lead to a program of behavioural control. Popper complains:

> The famous psychoanalyst Siegfrid Bernfeld (who was a friend of mine and of Freud's) once wrote: "Psychoanalysis cannot predict whether you will repress or sublimate. But whether you repress or sublimate will make all the difference to your behaviour" … Since a psychoanalyst cannot predict whether anybody will repress or sublimate, he cannot predict overt behaviour. On the other hand, a psychological theory can be put to the test only if it can predict overt behaviour. (Popper, 1986, p. 254)

On the basis of this failure of prediction, Popper puts Freud into the same camp as Darwin:

> Darwinism … is not a scientific theory, but metaphysical … because it is not testable … Darwinism does not really *predict* the evolution of variety; it therefore cannot really *explain* it … while we can explain a particular eclipse by predicting it, we cannot predict or explain any particular evolutionary change. (Popper, in Hopkins, 1992, pp. 24–25)

There are three points to make in response to this complaint. First, there are perfectly sound reasons why it is difficult to make the kinds of specific prediction that the critics unreasonably demand. Behaviourists may be able to predict the bar-pressing behaviour of their food-deprived rats, who have been conditioned to bar-press for food pellets. However, as Chomsky (1959) pointed out in his devastating review of Skinner's book *Verbal Behavior*, outside the narrow confines of the experimental animal laboratory, they have as little success with prediction of specific behaviour as does any other psychological theory.

Second, the subtlety and complexity of the conditions under which we typically engage in ordinary psychological ascriptions make it difficult to make *exact predictions*. In this sense, psychoanalysis shares much with the asymmetry of ordinary folk psychological explanation and prediction. We may predict with relative success that a long delay at an appointment will make a person feel impatient, but it is not so easy to predict that the impatience will be manifested in drumming the fingers on the arm of the chair. Similarly, we may be able to predict with relative success that a patient in psychoanalysis will develop transference feelings towards the analyst, but not so successfully the ways in which that transference will be manifested.

The third point is that, arguably, one of the major aims of science is *explanation*. And explanation is *not* (pace Hempel & Oppenheim, 1948) straightforwardly assimilable to prediction. On these grounds Hopkins (1992) argues that Popper's falsifiability criterion amounts to an irrational and deeply antiscientific "methodological short-cut" insofar as it:

> requires that all good explanations be assimilable to predictions of particular events … This seems a basic mistake: prediction requires certain specific information bearing on the occurrence of events (time and place); but this is clearly only a small part of the information relevant to explanation and understanding generally. (We understand lightning better for knowing that it is an electrical discharge, but this does not enable us to predict the flashes). (Hopkins, 1992, pp. 26–27)

And, with respect to Freud's explanatory modus operandi:

> Popper was right to assimilate Freud and Darwin and to contrast their work with physics. For both produced accounts of phenomena which had not been explained naturally before and which were similar in structure. (Roughly, Darwin showed that many features of plants and animals could be explained by regarding them as derived, by processes which he was the first to specify, from earlier, sometimes hypothesized forms which had not previously been taken as their origins. The same can be said of Freud, with 'mental life' substituted for 'plants and animals'.) To do this, both had to

> accomplish what seems the most basic task of explanation: that of exhibiting the data as instances of the generalizations and causal processes which were to provide the explanatory information about them. Accordingly, their main work was twofold: on the one hand, they described and specified the new modes of derivation; on the other, they linked the phenomena to these modes by showing repeatedly and in a wide variety of cases that they were as they would be had they been derived as hypothesized. (Hopkins, 1992, p. 25)

This basic similarity in the investigative and explanatory methods of Freud and Darwin takes us to the final complaint against psychoanalysis, which is that it does not lend itself to the controlled and rigorous forms of experimental research that are the norm in mainstream psychology. Freud's hypotheses, we are told, "require statistical research of a kind for which psychoanalysts generally lack training, much less experience" (Holt, 1986, p. 243). And:

> although Freud wrote a good deal about method, there is still no treatment of clinical testing which compares in clarity, let alone detail, with standard accounts of statistical hypothesis testing and experimental design. Without such a treatment, clinical testing is bound to seem mysterious and arbitrary to those nurtured on statistical methods. (Glymour, 1982, p. 14)

Westen (1990) provides a standard reply to this:

> Whereas the limitations of clinical data and inference are well known, the limitations of the experimental data base for personality research are less well established. One could make a strong case that several factors other than personality have been critical in shaping contemporary experimental research in personality. Two of these are the need for quickness (nQuick) and the need for large numbers of subjects for statistical analysis (nN). These two needs, driven by the higher-order factor, need for tenure (nTen), lead to an overreliance on questionnaire data. There is nothing intuitively sensible about the idea that the richness of human existence can be easily captured by 30–50 questions of the form "I am the kind of person who …" or the like. The correlation between nQuick,

> nN, and nTen on the one hand, and the use of questionnaires on the other, is probably far higher than the .30 that is typical in personality research. (p. 54)

Westen is quite right here, and his point provides some insight into the reasons for Freud's and other psychoanalysts' dismissal of experimental research supporting their theory. As Erdelyi (1986) notes, "Freud was not against experiments, only against silly experiments" (p. 234). However, Westen does not go far enough. For there is a steadily growing body of evidence demonstrating that psychology's experimental, statistical and data analytic practices, for the most part, are not only misguided but actually subvert its stated scientific goals (see Petocz & Newbery, 2010, for a more extensive discussion). Most psychologists are unaware that the adoption of statistical methods in psychology was driven not by scientific considerations—indeed, it was in the face of scientific objections that were repeatedly swept under the carpet (Michell, 2009a, 2010)—but by economic and social concerns (cf. Michell, 1997, 1999, 2009a, 2010). The emergence of post-Second World War economic conditions regulating research funding (Schorske, 1997; Solovey, 2004, 2013) meant that, in order to maximise funding opportunities under the new post-war dispensation, it was generally deemed wise to ape the quantitative rigour of the physical sciences, in which measurement was the primary goal.

As a result, psychology adopted the quantitative imperative (the false belief that measurement is a necessary condition of science—see Michell, 2003) and psychometrics became a "pathology of science" (Michell, 2000, 2008), characterised by "methodological thought disorder" (Michell, 1997). This involved a "two-step breakdown of scientific method" (Michell, 2000); first, the failure of the discipline to test the empirical hypothesis that its variables of interest (attitudes, personality traits, abilities, intelligence, etc.) are indeed quantitative; second, a disguising of that failure by the replacement of the classical realist definition of measurement with a non-realist, operationist definition following S. S. Stevens (Stevens, 1946; cf. Michell, 2002), according to which measurement is the "*assignment of numerals to objects or events according to* rule" (Michell, 1997, p. 360, emphasis in original). This custom-designed definition of measurement, in conjunction with the psychometrician's fallacy (the inference from order to quantity—see Michell, 2009b), allowed psychologists to claim to be measuring

any variables, whether or not they actually were *measurable*, and regardless of the fact that what psychologists appear instead to be dealing with are ordered attributes whose degrees of difference are intrinsically non-metric because they are qualitatively heterogeneous (Michell, 2010).

Psychologists are also generally unaware that the statistical techniques they use rest on a confused mixture of conflicting assumptions regarding the nature of probability and the evidential role of statistical inference. This was the result of an "inference revolution" (Gigerenzer & Murray, 1987; Halpin & Stam, 2006; Hubbard, Parsa, & Luthy, 1997) that occurred in the middle of the twentieth century and "saw the widespread adoption of a hybrid of frequently contradictory Fisherian and Neyman-Pearson statistical concepts as a single, uncontroversial method of inductive inference" (Hubbard & Ryan, 2000, p. 666). Specifically, this hybrid cobbled together Fisherian NHST (null hypothesis significance testing) with the competing views of Neyman and Pearson, together with their ideas about an alternative hypothesis, Type 1 and Type 2 errors, and statistical power, in a manner that neither camp condoned. According to Hubbard and Ryan (2000), "Despite serious misgivings on both sides, this hybrid methodology, with its emphasis on the rejection of the null hypothesis at $p < .05$ or better, gradually made its appearance in behavioural science textbooks on statistical methods" (p. 666). Since then, "four decades of increasingly trenchant criticism over the use and abuse of SST [statistical significance testing] has failed to reduce its visibility in the psychological literature" (p. 672). But there is more.

Grayson (1998) argues that the major statistical confusion in psychology (and other social sciences) is the blurring of the distinction between the Neyman frequentist view of long-run probability (which disallows the evidential use of probabilistic information in the single case) and the scientific issue of evidential inference. The frequentist view is favoured by scientific psychologists because it is objective. However, it is also scientifically sterile. According to Neyman and Pearson (1933), in terms of the nature of scientific evidence, there is no point to how we routinely perform statistical analyses in single experiments, because "as far as a particular hypothesis is concerned, no test based upon the theory of probability can by itself provide any valuable evidence of the truth or falsehood of that hypothesis" (p. 291).

Hence, there is nothing to be gained in a *single* piece of research by performing a statistical analysis and computing a *single* *p*-value. Nevertheless, psychologists are reluctant to move to a treatment of probabilities as numbers representing degrees of belief that can be quantified on single occasions (as in the Bayesian approach) (cf. also Gigerenzer, 1987), because that seems too *subjective*, even though this version is attractive insofar as it allows for evidential inference in a single experiment. Following Gigerenzer's (1993) Freudian analogy, Grayson (1998) maintains that, in mainstream psychology's statistical psyche, the superego is represented by rigorous, socially desirable frequentism, the ego is the confused yet pragmatic Fisherian significance testing, and the id is the Bayesian lustful desire for evidential inference, allowing us to draw conclusions about the probability of our hypotheses given the data. Furthermore, the unavoidable issue of evidential inference in a single case is left unaddressed by the increasingly popular recommendations (e.g., Osbourne, 2010, p. 3) for statistical reform and progress that challenge the dichotomous thinking associated with the accept/reject decision rule in NHST and urge moving instead to CIs (confidence intervals), calculation of effect sizes, meta-analyses, and so on.

Psychology's statistical and data analytic practices contain numerous other confusions and misconceptions (cf. also Grayson, 1988; Rosnow & Rosenthal, 1989; Rozeboom, 1960), such as those concerning "negative" results, against the reporting of which there is widespread scientific prejudice (Furedy, 1978; Rosenthal, 1979); the use of "protection" methods in multiple-inference procedures (Grayson & Oliphant, 1996); the misunderstanding of post-hoc testing (Oliphant & Grayson, 1996); the widespread practice of HARKing (hypothesising after the results are known) (Kerr, 1998) by adjusting predictions in the light of results; and the non-independence error in calculating correlations in fMRI studies, with the result that "a disturbingly large, and quite prominent, segment of fMRI research on emotion, personality, and social cognition is using seriously defective research methods and producing a profusion of numbers that should not be believed" (Vul, Harris, Winkielman, & Pashler, 2009, p. 285).

The important point for the present discussion is that, as Grayson (1998) points out, the issues typically are scientific, not merely statistical; the evaluation of statistical evidence is only one small part of

"the whole scientific enterprise of reasoning about and evaluation of evidence" (p. 342).

Since these are the "scientifically serious circles" in which, according to Crews (1993), Freud's doctrine—accused of "scientistic self-misunderstanding" (Habermas, 1968, p. 246)—has fared so badly, it is worth considering to what extent the investigative and explanatory methods of Freud and psychoanalysis, which were based on the accurate recognition that measurement is scientifically inappropriate in the absence of a ratio scale (cf. Bellack, 1993), fall short of the "controlled and rigorous forms of research" (Holt, 1986, p. 243) to which it must submit.

Summary and implications

I have argued that the three senses in which the question of the scientific status of psychoanalysis has been "done to death" (viz. open house, doomsday cult and Cleopatra) are superficial and inconsequential. In contrast, one respect in which the question has *not* been done to death (viz. a three-step meta-analysis of the issue) is serious and has profound consequences. Step One of this meta-analysis yielded six basic categories of charge against psychoanalysis. In Step Two, the cross-category positions of the two major critics (Popper and Eysenck) who feature prominently in the *psychological* literature as fountainheads for the standard criticisms was revealed to be incoherent. In Step Three, I considered in turn the within-category argument strategies of a wider range of both critics and defenders. Category 1 (psychoanalysis as a hermeneutic enterprise) shows that the inclusivist stance of psychoanalysis—both in subject matter and in method—stands as a scientific corrective to the restrictive *scientism* of mainstream psychology. The Category 2 charges amount to little more than *un*scientific ad hominem forms of persuasion. The Category 3 charge of Popperian unfalsifiability is, by all accounts, a complete fiasco. The Category 4 charge of the falsity or lack of evidence in favour of psychoanalysis is, on any objectively balanced consideration, yet to be upheld. The Category 5 charge of methodologically defective procedures, including that of the conventionalist stratagem, together with the Category 6 charge of poor possibilities vis-à-vis observation, prediction, and control, rest largely on a combination of double standards, selective straw manning, ignoratio elenchi, and scientistic self-misunderstanding. Taken together, these results suggest that, far

from the case against psychoanalysis having been established, in some respects it has not yet been properly formulated, and in others it has backfired. This should come as a surprise to those within mainstream psychology who have rejected psychoanalysis precisely on the grounds that it is unscientific.[21]

If my analysis is sound, we must conclude that not only is there is no good scientific reason for excluding psychoanalysis from mainstream academic psychology, but there are several good scientific reasons for including it. This conclusion has three significant interrelated implications.

The first is that it will take the *silence* out of the return of Freud. I mentioned earlier the number of integrative movements in mainstream psychology in which Freudian and psychoanalytic ideas are currently returning in conveniently disguised form. The problem with this is not so much that it fails to give credit where credit is due as that it effectively cuts off the relevant material from its explanatory groundings and informative connections and extensions. For example, a recent (2008) psychology honours research project on fine-tuning the relations between attachment style and the various dimensions of "emotional intelligence" (EI) drew the following critical comment from an (anonymous) examiner:

> Is it really a strength that attachment theory grew out of psychoanalytic theory? It seems to me that attachment theory has replaced one overarching hegemony with another. Attachment theory smacks of a one-size-fits-all approach to development that draws much of its inspiration from a failed project, and despite its 'biological' leanings never examines where attachment is supposed to fit from an evolutionary perspective.

This comment is completely understandable given how attachment theory is currently being pursued. Insofar as mainstream psychology has played down its origins in Freudian theory, it has forfeited its ability to spell out the biological and evolutionary context, with the result that attachment theory has indeed become a relatively empty descriptive recipe. Clearly, also, there is a perception that psychoanalytic theory has no basis in biology or evolutionary theory. Thus, it would be of considerable benefit for researchers in this area to no longer be (wittingly or unwittingly) constrained to distort the picture by presenting and

developing attachment theory as disconnected from Darwin and Freud. Related to this, work on the border between neuroscience and psychoanalysis (e.g., Fotopoulou, Pfaff, & Conway, 2012) could be considered within mainstream psychology alongside cognitive neuroscience, developmental neuroscience, affective neuroscience, and all the other hybrids.[22] A final example is clinical training. The open return of Freud would facilitate recognition of research that supports the efficacy of psychodynamic forms of intervention (Mace, Moorey, & Roberts, 2001; Shedler, 2010) and would obviate the need for calls such as the following to expand the training in psychotherapy to include psychodynamic approaches:

> We think it is time for those universities that have not already done so to move beyond the convenient but intellectually dishonest position that CBT is the only evidence-based psychotherapy. Undergraduate students should be taught the principles and evidence base for psychodynamic therapy, postgraduate psychology students should have opportunities for training in psychodynamic psychotherapy, and such training should be supplied by psychologists with genuine interest and expertise in the area. (King et al., pp. 4–5)

The second implication is that, with the silence taken out of the return of Freud, the synergies between mainstream psychology and philosophy of psychoanalysis (in addition, of course, to other related disciplines such as anthropology and sociology—cf. Mimica, 2007a, 2007b) could be more openly and mutually embraced. Philosophy would no longer need to offer its contributions by first setting aside the issue of the scientific status of psychoanalysis. And mainstream psychology, in turn, would thus benefit from the considerable work that has been produced in the intersection of philosophy and psychoanalysis. There is much here that is of direct relevance to psychology—work on topics such as: aggression within and between groups (Hopkins, 2004, 2012); racism and envy (Pataki, 2004); violence and prejudice (Young-Bruehl, 2004); psychology of religion (Pataki, 2007); repression and defence (Boag, 2012; Suppes & Warren, 1982); emotion (Panksepp, 1998); motivation and wish fulfilment (Newbery, 2011; Pataki, 2000, 2003, 2014); drives and affects (McIlwain, 2011; Solms & Zellner, 2012); conceptual metaphor, embodiment and symbolism (Hopkins, 2000; Petocz,

1999); psychoanalysis, psychiatry, and philosophy (Pataki, 1996); and irrationality and cognitive dissonance (Pears, 1982). This work is pursued in accordance with one of the major aims of philosophy, which is to make things clear via conceptual analysis, and its fruits for the psychological understanding of the human mind and behaviour should not be left unpicked.

Finally, there are signs that psychology is inching towards what might optimistically be described as a methodological renaissance. My conclusions concerning the scientific status of psychoanalysis converge with recent calls for unifying psychology via rethinking its conception of scientific method and the requirements of research methodology (Biernacki, 2012; Bryman, 1988; Haig, 2005, 2008; Lissitz, 2009; Lloyd, 2010; Machado & Silva, 2007; Michell, 2001, 2004a; Osbourne, 2010; Petocz & Mackay, 2013; Petocz & Newbery, 2010; Toomela & Valsiner, 2010). The striking thing about these calls is that the methodological flexibility, focus on conceptual analysis and shift in balance between qualitative *vs.* quantitative methods is, fundamentally, a case of old wine in new bottles; it is a return to a time when a variety of non-quantitative scientific styles of thinking (cf. Crombie, 1994) were predominant in psychology. It is also a return to a more appropriate scientific home for psychology in terms of the "big picture" view.

With respect to that "big picture" view, I agree with Brakel (this volume, Chapter Six) that psychoanalysis is "a grand, overarching theory in the manner of evolution", a general theory of mind and behaviour that is rooted in a Darwinian-inspired instinctual drive theory of motivation. Damasio (2000) complains that, until recently, "neuroscience and cognitive science have proceeded as if Darwin never existed" (p. 39). That is because psychology has long been trapped in the idealist hole dug for us by Descartes (Leahey, 2013, p. 147). The Scottish-Australian philosopher John Anderson noted that "to get rid of idealism we have to go back upon all sophisticated "modern" views and recapture the Greek directness" (1929, p. 60). However, as Stove (1991) remarked when discussing idealism, "No one thought seriously … of going right back *beyond* the Cartesian starting points, of dualism in metaphysics, and representationism in epistemology" (p. 102). Freud's theory follows Darwin's in helping us to do just that, from a line that originates with Aristotle. Darwin is belatedly returning to mainstream psychology. Hopkins (1992) shows that the parallels between Darwin and Freud are (contra Popper) a *strength* rather than

a weakness (cf. also Grossman, in Shevrin, 1995). Moreover, Darwin's model of science as critical inquiry is, like Freud's, Aristotelian in spirit. Although Darwin identified Linnaeus and Cuvier as his "two gods" he added that "they were mere schoolboys to old Aristotle" (1882, p. 427). Sulloway hails Kitcher's (1992) "trenchant critique of Freud's speculative enterprise" (Sulloway, 1995, p. 170) as having finally explained, with respect to Freud, why "when he *did* fail, he failed *big*—on a level with Aristotle" (p. 170). However, in view of recent revisionist discussion of Aristotle's scientific approach (e.g., Esfeld, 2000; Groarke, 2009; Hood, 2004; Sutcliffe, 1993), I think I side with Darwin rather than with Sulloway, and maintain that, in ways that are relevant to psychology, when Freud did *succeed*, he succeeded *big*—on a level with Aristotle.

CHAPTER EIGHT

Freud and Wittgenstein in the cuckoo's nest

Thomas Wallgren

Thanks to the recent breakthroughs in cognitive science and evolutionary psychology the temple of modernity that we call science is now more complete and glorious than ever. In this temple Freud and Wittgenstein may look alien; like residents sans-papier. The impression one sometimes gets is that the very difficulty of throwing out these strangers in our midst from contemporary academic debate is seen as inexplicable and irritating.

The overarching idea that drives this essay is that fruitful debate about what there is still to learn from Freud and Wittgenstein is only possible if our knowledge system is first rationalised and democratised.[1] I will give some substance to this grand proposal. In the first part I present some diagnostic remarks on the scientistic mood and metaphysical prejudice of our times. I then present a review of similarities and differences between Freud and Wittgenstein (Part II). The abstract ideas presented in the first part are illustrated and explicated in Part III of the essay in which I discuss some details in the debate between Brakel and Grünbaum, in this volume, about the scientific status of psychoanalytic theory. There is a concluding remark (Part IV).

One of my aims will be to show that the naturalist perspective that dominates contemporary philosophy of mind and psychology and

also much discussion of Freud, coming from both his supporters and his critics, is prejudiced and confused. (The meaning of naturalism as intended here will be explained below.) The debate between Brakel and Grünbaum illustrates well the kind of confusion that is unavoidable as long as naturalism about the mind is taken for granted. Using Wittgenstein as my main resource I shall argue that the problem with naturalism, also in debates about Freud, is not that it is logically, factually or metaphysically wrong or mistaken. The whole idea that an important goal is always to get the facts right as opposed to wrong when we talk about us, or the mind, or the human being, seems to me much overrated in contemporary debate. (Am I always, or usually, in the wrong about facts if I am confused or if I attend to this rather than that aspect of my relation to my friend?) The point I am making is easily misunderstood. It is not that I want to say that facts are *not* important and science does not give knowledge. I have no doubt at all that empirical research about people (individuals and societies) that takes its questions and methods from, for instance, neuroscience and evolutionary biology gives new knowledge. My misgivings are with the monopolising tendencies: with one form of understanding throwing out the others from the realm of legitimate discourse; with the scientistic cuckoo in our nest of understanding of what it is to be human and what forms of reason that are legitimate.

Part I

The interest many practising scientists and the public at large have in recent developments in neuroscience and evolutionary studies is, it appears to me, linked to the resurgence of scientism in the study and understanding of humans. "Scientism" as I use the term here is the notion that modern empirical science is (with or without the aids of clear-headed analytical philosophy of science) the highest authority in a comprehensive, universally valid knowledge system and that it gives us increasing knowledge about how things really are and how they work in terms of cause and effect.[2] With scientism in the study of humans I mean the notion that there is no limit to what we may learn about the human being through the application of the methods, concepts and norms of the empirical sciences and that, therefore, in a fully enlightened world, science will tell us everything there is to know about humans. We may say that scientism promises to reveal secrets (or, rather, to reveal that there really are no secrets), to overcome mysticism

and superstition and to help us find out, finally, the objective truth about us.

Scientism is rarely explicit and programmatic. People usually don't pronounce commitment to scientism. When I say that scientism has resurged, my claim is about the spirit of our times more than about explicit beliefs. The spirit of the time is meant here in a local and specific sense: I refer, simply, to the enthusiasm and lack of critical attention with which specific concepts, ideas, theoretical suggestions and scientific projects are welcomed that make sense only within a scientistic framework. I shall next provide three examples of how scientism is instantiated and how it affects our discourses and practices even when it is implicit and unprofessed. Before going to the examples I wish to make clear (at the risk of being repetitious) what I am driving at with them.

The first purpose of the examples is to give substance to my diagnostic claim that scientism is on the rise in our culture today. I succeed in this respect only if, or to the extent that, my readers accept my suggestion that the three cases I present are not odd exceptions but that they are *typical* of our times (i.e., examples in the strong sense of being exemplary expressions of how many people today (happily?!) see the world and understand their place in it).

The second purpose of the examples is to clarify what I mean when I say that it is integral to what I call scientism that it is today usually not explicit: people who are affected by scientism tend, first, to look at science not as *one* way of searching for understanding but to look at it as *the* way and, second, not to look critically at this way of looking at things.[3] I will call scientistic outlooks that share these two characteristics *cynical* scientism. So, my purpose is not, then, to say that scientism is false or wrong or that the people who figure in my examples are involved in something illegitimate. I do not propose here to take a stand on scientism at all. My purpose is to show that there is such a thing as what I here call cynical scientism that works in our culture, and (often unwittingly) on us, and to illustrate what I take this to mean.

My first example is from philosophy. Daniel Dennett once wrote (2003):

> One widespread tradition has it that we human beings are responsible agents, captains of our fate, *because* what we really are are *souls*, immaterial and immortal clumps of Godstuff that inhabit and control our material bodies rather like spectral puppeteers. It

> is our souls that are the source of all meaning, and the locus of all our suffering, our joy, our glory and shame. But this idea of immaterial souls, capable of defying the laws of physics, has outlived its credibility thanks to the advance of the natural sciences. Many people think the implications of this are dreadful: We don't really have 'free will' and nothing really matters. The aim of this book is to show why they are wrong. (p. 1, his italics)

Dennett's enthusiasm is unmistakable and charming. But the point here is not about him, it is about how he is placed in the debate. In our times Daniel Dennett's enthusiasm is not seen as the expression of a radical new idea or a bizarre fantasy. His words are greeted as a fine expression of a vision many of us today share, and a dream that we support. People like John Searle, Paul and Patricia Churchland, and Antonio Damasio, too, are not marginal eccentrics. They have all made big careers in philosophy and science when they have ventured to explicate the kind of dream they share with Daniel Dennett. There are of course disagreements about detail, and these will seem important from the insider's perspective. But most academic criticism of, for example, Searle's theory of qualia or Dennett's heterophenomenological method is based on a prior agreement that Searle and Dennett are right to say that with the recent advances made in the philosophy and science of the mind we experience a positive breakthrough in our worldview.

My second example is from science or, more precisely, from evolutionary biology. In *Biology Letters* Emma E. A. Cohen et al. (2010) write:

> Psychologically, endorphin release is experienced as a mild opiate 'high' … reflecting the role that endorphins play as part of the pain control system. Endorphins have been explicitly implicated in the process of social bonding … although the mechanisms involved remain unclear … . We tested for an enhanced opiodergic effect from behavioural synchrony in a group of rowers. (p. 106)

The quote rests on the assumption that readers will share with the writers the following presuppositions: (i) Psychology is about (subjective?) experience of causally effective inputs; (ii) There is a "pain control system" and we have a fair idea of what it is; (iii) There is a process of social bonding such that endorphins are a candidate for being "explicitly implicated in it", and; (iv) If we can show that behavioural

synchrony increases endorphin release then this will contribute to our understanding of "the process of social bonding." Therefore testing the opiodergic effect from behavioural synchrony is relevant for our understanding of (iii).

Number (iv) in my list is an interpretation. But I do not see why Cohen et al. would have done their study as they did and presented it as they do unless they assume that their test is relevant for (iii) or something similar to it.[4] But (iii) in my list strikes me as strange. What goes into the assumption? It is difficult to understand. One reason is that the notion of the "explicitly implicated" is unclear. Perhaps (iv) can help us understand what is meant. But what are we to make of (iv)?

Perhaps it helps if we ask: Is (iv) correct? In a sense: yes. People often like others with whom they have shared physical exercise, such as skiing or marching or dancing.[5] If we learn that endorphins are released more when people dance together or row together than if they dance alone or row alone, do we know something we did not know before? Certainly we do. But is the thing we learn interesting? Is it interesting enough to be worthy of the money and effort needed to find it out? Is it interesting enough to deserve to be published, and is the publication to be seen as a scientific merit? We know that from the perspective of a sociological observer the answer to all my questions is positive. The study undertaken by Cohen et al. and the publication in which they report and discuss their results is of a kind that today is taken as interesting.

Let me now suggest that we look at this sociological fact as contingent and surprising. Are the results presented by Cohen et al. not absolutely trivial? They seem to show that when people do something they like more (when people row together) than when they do something that they like less (when they row alone) then such-and-such happens in the brain. But how is that interesting? Did we not assume already before the experiment that brain states will be systematically different when we enjoy something more than when we enjoy it less? So this is not what we have found out. We do not even have, in the light of the study of Cohen et al. and other similar studies, a clear idea of what it would be like to study the alternative hypothesis that when people do something they like more than when they do something that they like less, then there will not be any difference in brain events corresponding with the differences in activity and attitude (mood, emotion, feeling). But if so, if we already have assumed that there will be some

difference in brain events, is their study really worth its trouble. Why and how? The issue is difficult and later discussion will serve to clarify it to some extent. Here, let me indicate some of the things I have in mind through a series of questions and observations: since the early times of civilisation young men in armies have been ordered to march together rhythmically. Since times immemorial people have danced together in rituals. Why did Cohen et al. find it interesting, worth reporting, that more endorphins are released when people row together (in synchrony, mind you) than when they row alone? Because we have been uncertain of what? Of the stimulating effect of opioids? Of the social importance of dancing and marching? Of the possibility to use social and pharmacological techniques to influence our moods and our social relations? Of what? What if the result of the experiment had been that endorphin levels are the same when people row alone as when they row together? Would that not have been *really* worth publishing? But with what lesson? That rowing together is not nice? Or that opioid levels are unimportant for social bonding? Or that there is perhaps nothing special going on in the brain when people start liking each other? Is it conceivable that studies such as the one reported on by Cohen et al. could convince us of any of this? Perhaps of the irrelevance of endorphin release for affection and social bonds? But not of the other two issues? Whence this hunch? Moreover: Will studies of the kind Cohen et al. present give us a new key to what social bonding is? To why it happens? To how it happens?[6] (If I assume, as I am inclined to do, that the answer to the last of these three questions is positive, to the question before unclear, and to the question before that negative, then what does it say about me?) Is there an intrinsic link between the questions Cohen et al. ask and the kind of answer they offer and instrumental notions of reason? That is, is the result they present of a kind—as I believe it is—that lends itself to enhanced technological imaginations of what we might do in the sense of how we might manipulate people? And is this technological bent linked—as I believe it is—to strategies of power and commercial interests that influence scientific policy, and to what kind of research is funded and what is not funded? Is it a part, then, conceptually and through its power effects—again, as I believe it is—to a progressive project that has, arguably, set us on a drift towards the sixth state shift in the earth's biosphere?[7]

The third example by which I want to illustrate the hold of what I call cynical scientism on the present mind-set is from scientific policy. In

January 2013 the European Commission announced that it had decided to select what is called The Human Brain Project (HBP) as one of its two Future and Emerging Technologies (FET) flagship projects. The cost estimate of the project is 1.19 billion Euros. According to a press release from the HBP, the goal of the project is "to pull together all our existing knowledge about the human brain and to reconstruct the brain, piece by piece, in supercomputer-based models and simulations. The models offer the prospect of a new understanding of the human brain and its diseases and of completely new computing and robotic technologies." The decision to fund it was, according to the same press release, explained by the fact that "the European Commission supported this vision."[8]

Most of my reasons for mentioning this third example of what I mean when I contend that enthusiasm prevails for scientism in the study of humans have been indicated in my set of questions to the second example. Here I want to add a sociological hypothesis. My hypothesis is that the decision to give 1.19 billion Euros to HBP was based as much on the hype around neuroscience created by the interplay between the bold, philosophical visions of people like John Searle and Daniel Dennett (who claim that neuroscience gives us new knowledge about mental phenomena) and science journalists (who give inspiring, easy-to-read popular interpretations of those visions) as on the results of peer reviews and other mechanisms for discussion within the community of scientists.[9]

Enough now of examples. The purpose of introducing them has been to provide some warrant for my diagnostic claim that there is in our times enthusiasm for scientism in the study of human beings and that this enthusiasm is today rarely programmatic or ideological as it often was in the nineteenth and twentieth centuries. People no longer say that they are committed to scientism. There is no enthusiasm for an explicit commitment of that sort. The enthusiasm comes out, indirectly, as interest in and support for philosophical theories, empirical research and research policy of the kinds I have given examples of. Such (often inadvertent) scientism is not an isolated phenomenon in our worldview. It is connected to the dominating metaphysical prejudice of our times. The prejudice is that we are all naturalists now.

I use the word prejudice here in order to underline that the term naturalism is more often invoked than explained. If that is true it is difficult and perhaps even impossible to understand what people mean when they speak of naturalism and why they think of themselves as

naturalists. Obviously, the implications are rather dramatic. If we typically think of ourselves as naturalists, and, typically, do not know what it means or what we are taking on if we claim to be naturalists it follows that in our metaphysical self-understanding we live in a state of confusion. Is such confusion our plight? I think the answer has to be affirmative. Let me provide some comments.

Naturalism is perhaps a natural concept only in philosophical debate. It is part of its current semantics that, also there it is mostly used promiscuously, as if we could all take it for granted that it means something clear and stands for truth. To illustrate this use of the term naturalism in contemporary philosophy I wish to provide two quotes. In his preface to the volume *Rorty and His Critics* (2000, pp. ix, xiv) Robert Brandom, a leading North American analytical philosopher of his generation, writes:

> Richard Rorty is one of the most original and important philosophers writing today. He is also one of the most influential beyond the confines of professional academic philosophy … Rorty's pragmatism is, like its classical antecedents, itself a form of *naturalism*. The background for the positive alternative he suggests to a picture of vocabularies as representing how things really are (a task some do better than others) is of vocabularies as *tools*, employed by natural creatures in a natural world. 'The world can, once we have programmed ourselves with a language, cause us to hold beliefs.' Different vocabularies equip us with beliefs that are of more or less use in coping with the environment in various respects. And to understand the sense in which we are 'in touch with reality' *all* we need to understand is that *causal* contact with the world, the sort of contact describable in the language of afferent and efferent physiology (underlying perception and action), in the context of an account of how we are (naturally) wired up and (socially) trained … In taking this line, Rorty insists that he is being more resolutely naturalistic than the fans of natural science among analytic philosophers. (Italics in original)

Similarly, in a recent essay on consolation, Axel Honneth, a leading German critical theorist, writes (2010, p. 298): "As members of Western culture we have by now all become pure naturalists with respect to the most fateful events in our lives … Whether we like it or not, whether we have religious faith or are convinced atheists, we do not seem to have alternatives to a naturalistic explanation of the causes of

our suffering on earth."[10] Another aspect of the cultural semantics of naturalism (and of its semantic sibling, "physicalism") in our times is that when it is made a topic of study the question typically asked is: What *kind* of naturalism is adequate? Other questions that are rarely asked include: Can we make sense of "naturalism"? What sense?—It seems to me that today "naturalism" is taken for granted as descriptive of what we believe, of how we see things, in a way that is similar to the taken-for-grantedness of Christianity that infuriated Søren Kierkegaard 150 years ago. Kierkegaard's question was: "Is there even one true Christian in Denmark?"—In a similar vein I suggest that we might ask: "Is there even one true naturalist in Europe or the USA today?"

Among contemporary naturalists Habermas stands out because he understands the urgency of my Kierkegaardian question. He is therefore not cynical about scientism in my sense of the term. Habermas claims to combine full respect for the epistemic authority of science and for its achievements with a criticism of scientism. Habermas's criticism of scientism is based on an elaborate neo-pragmatically conceived, de- or quasi-transcendentalised philosophy of language and communicative action (Habermas, 1981, 2012). Even more importantly, Habermas is explicit on two points, which typically are not addressed in what I call cynical scientism. He recognises the interdependence between scientism (with its implicit and hence rationally illegitimate epistemological presuppositions) and naturalism (with its implicit and hence rationally illegitimate metaphysical presuppositions). It is striking that Habermas's most recent explication of his philosophical outlook, presented as "post-metaphysical" and a "weak naturalism", ends with the self-critical note that "*of course*," a venture such as the one he pursues "floats in the danger of dressing up a metaphysically committed philosophy of nature in a post-metaphysical costume" (Habermas, 2012, p. 53).[11] Let me add, finally, that it is also a strength of Habermas that, earlier in his essay, he is open about the anti-relativism that is the moral source that drives his commitment to naturalism (p. 50f).

Part II

Apparently, then, most of the time naturalist metaphysics as well as scientism in the study of human beings do not function in our knowledge system as scientific hypotheses or as debatable assumptions that are open to criticism. They are perspectives from which we look at ourselves and the world. We may say that they serve as foundations upon

which we can place the building blocks of a scientific world view, but not as its achievements.

The deepest similarity between Freud and Wittgenstein is, as I want to suggest, not due to themselves as much as it is to how we look upon them, or more precisely, how they are likely to work on us if we take interest in them. (I would like to add: "If we allow ourselves to take interest in them.") Freud and Wittgenstein work for us and on us as sources for critical attention to and disquietude about scientism and naturalism. They destabilise our habitual confidence in the scientific world view, not by questioning its truth, but by inviting us to be concerned about its meaning and its legitimacy, as well as asking about who we are who are party to it. To get to this perspective on the kind of shared interest Freud and Wittgenstein have for us we must distance ourselves, I believe, from three other ways of relating them to each other that are, more visibly perhaps, on offer.

First, there is the question of difference. Freud's and Wittgenstein's fields of research and the quality of their interests strike most researchers as so different that it has been exceptional in research on either one of them to care much about scholarly discussion about the other. There is good reason for this state of affairs. For people with narrow specialist identities, Freud, the psychologist, and Wittgenstein, the logician and philosopher of language and mathematics, will appear to have contributed to different fields within the large, compartmentalised enterprise of science.

But also when more reflective ("qualitative") interests play a role Freud and Wittgenstein tend to be discussed in isolation from each other. Freud has found a keen reception in many of the main varieties of critical theory. One tradition, with the so-called Frankfurt School as its centre, has theorised the relation between Marx and Freud, claiming, for example, that Freud gives us means to understand the dynamics determining the "subjective conditions" for the revolution whose "objective conditions" are diagnosed in Marx's critique of capitalism.[12] In the hermeneutic and postmodernist traditions Freud is placed with Marx and Nietzsche as one of the three "masters of suspicion" (Ricoeur, 1970) of the visions of emancipation that dominate the theories and practices of liberal modernity. In comparison, critical theorists have rarely reached out to Wittgenstein as a resource for their purposes. They have, it seems to me, by and large inferred from the cultural conservatism that is one layer in Wittgenstein's remarks on his times—and

also in his personal lifestyle, habitus, and musical preferences—that his philosophy does not lend itself to critical purposes congenial to them.[13] Similarly, at the other end of the continental philosophy *vs*. analytical philosophy divide there has been an intense reception of Wittgenstein as a "critical" resource within the mainly anglophone analytical tradition where philosophies and theories of language, science, logic and ethics are main themes and treated as separable from cultural and political philosophy.[14] In these traditions Freud is a rare reference even when the philosophy of psychology and mind is the theme.

Hence, I think a case can be made for the following. There are visible "surface differences" between the topics discussed by Freud and Wittgenstein, and they certainly serve as one reason for the relative scarcity of scholarship that seriously engages with both thinkers. But in more reflective and critical discourse the sense that Freud and Wittgenstein are engaged in separate enterprises and have little commonality is due to contingencies in reception history more than to informed assessment. This is the first way in which the "real adventure" of relating Freud and Wittgenstein to our intellectual and moral needs has been lost.

The second way of losing the adventure may follow if we look at the most obvious rapport between the two. In harsh competition with giants like Robert Musil, Franz Kafka, Karl Kraus, Arnold Schoenberg, Adolf Loos, and others, Freud and Wittgenstein stand out as the most sensational geniuses born out of the feverish collapse of the Habsburg Empire. In the context of social and cultural history of fin-de-siècle Vienna the formation of the ambition, cultural values and life trajectories of Freud, Wittgenstein and others can be explained. However, such explanations will not be able to tell us much about the substance of their contribution.[15] But what, then, about a third approach in which we take the question of actual influence as our point of departure? There is to my knowledge no reason to think that Wittgenstein influenced Freud. But there is a serious case to be made for the influence of Freud on Wittgenstein, at least after the publication of Wittgenstein's early masterpiece *Tractatus Logico-Philosophicus* (1921).

There is a fair deal of circumstantial evidence for the possibility of Freud's influence on Wittgenstein. In the Wittgenstein family Freud's work was discussed and well-known. Wittgenstein's elder sister Gretl (Margarethe) was analysed by Freud. We know that Wittgenstein read at least some of Freud's work around 1930 and again in the 1940s. In

Wittgenstein's notebooks there are many occasional remarks on Freud. Wittgenstein appreciates Freud for "having something to say" (high praise to come from Wittgenstein) and he once gave *Die Traumdeutung* to one of his most beloved friends, O'Drury. Taking a different attitude, Wittgenstein also once lamented that Freud has caused "infinite harm"—he thought that Freud did not know himself, and he was ambivalent about the supposed greatness of Freud. Arguably, the relatively frequent comments on dreams in Wittgenstein's notebooks have been inspired by his reading of Freud. Freud also figures in some of Wittgenstein's notorious reflections on his Jewishness and on the role of Jews in European culture. Nevertheless, despite all these odd references, substantial remarks by Wittgenstein on Freud or relating explicitly to his reading of Freud are very few. Wittgenstein has some critical remarks where he claims that Freud fails to separate clearly between causes (of events) and motives (for human conduct). He also has some appreciative and philosophically remarkable remarks on how we can see in Freud's work on the interpretation of dreams how conceptual innovation may serve truth.[16]

We know, then, that Freud *may* have influenced Wittgenstein. But *was* there an influence? Freud's possible significance for the evolution of Wittgenstein's method and conception of philosophy is by far the most important case to consider. The key concept in this connection is therapy. This will be our topic in the remaining parts of this section. (The discussion should serve as a bridge between the remarks on scientism and naturalism above, and the remarks on our case study in Part III. I will not explicate the connections.)

Wittgenstein sometimes described his work as a kind of therapy.[17] Wittgenstein also proclaimed that he is "a disciple of Freud" (1966, p. 41). The idea that philosophy is a kind of therapy goes deep into everything that is important in Wittgenstein's philosophy. Similarly, Freud's abiding relevance is due, perhaps foremostly, to his elaborate development of a new therapeutic practice. I will comment on the relation between Wittgenstein's and Freud's notions of therapy from two perspectives. I discuss similarities between both, with a focus on similarities in the critical tension between Wittgenstein's and Freud's notions of therapy and scientism. I then provide some observations in support of my view that Wittgenstein's understanding of the second issue, which is also a form of self-understanding, is superior to that of Freud's. Before that I wish to make short shrift of the question of influence.

Wittgenstein hardly ever provides references and he is little concerned with explaining where his ideas come from. In this sense he was no scholar. In some cases we do have other kinds of evidence on which to base an assessment of influences. Sources for such evidence would include at least Wittgenstein's explicit mention in his notebooks, his letters, the sparse references in the *Tractatus* and in the *Philosophical Investigations*, Part 1, and also the notes taken by his pupils from his lectures, as well as the reports by various people who knew Wittgenstein from their conversations with him. We might also infer an influence from similarities in style, metaphors, examples, and concepts. Overall it seems to me clear that *we do not have evidence* of any of the foregoing kind on the basis of which it is possible to determine the extent to which similarities between Freud and Wittgenstein can be attributed to an influence. This lesson should also be kept in mind in debates about Freud's and Wittgenstein's conceptions of therapy. The questions of whether Wittgenstein derives the word "therapy" from Freud and to what extent he derives intellectual content from Freud's notion of therapy are, I believe, unanswerable. Nevertheless, the question of similarities (and differences) between Freud's and Wittgenstein's ideas of therapy deserves attention, irrespective of the question of influence.

When we look for similarities between Freud's and Wittgenstein's views on therapy two issues stand out. One has to do with method for arriving at truth, the other has to do with our understanding of what truth and knowledge is. In both cases Freud and Wittgenstein cohere interestingly, even if only at a very general level.

The interesting agreement in method is the use of dialogue in the search for truth. In modern science, and in many religious and mystical knowledge systems, dialogue is a dispensable aid. For prophets, inspiration works as a source of absolute truth that can be preached but not criticised. Similarly, in the case of science, one person working alone could in principle create the theories and do the experiments that are needed to test them and hence gather knowledge about how the world works. For Freud and Wittgenstein no single person working alone could find truth because the difference between the kind of true and false belief that they are interested in finds a place (i.e., has meaning) only in dialogue. Conceptually and epistemologically it may be controversial whether a notion of truth that is essentially dialogic can be legitimate. I will not discuss that now. (For discussion, see Wallgren, 2006.) Here,

my claim that Freud and Wittgenstein both use dialogue essentially as a means to search for truth is descriptive of their procedure only, and uncommitting in all other respects.

In Freud's case the description is hardly controversial. In Wittgenstein's case the surface evidence is also clear in one respect. His main late work, *Philosophical Investigations* has the form of polyphonic (or heteroglossic) dialogue.[18] Here we can leave unanswered the question whether and in what sense Wittgensteinian heteroglossy is essentially dependent on actual dialogue between real people or whether it can do its work even if pursued as internal dialogue only. Freud's and Wittgenstein's convergence in the use of a dialogic method in the search for truth is significant by making relevant, in the age of monologic science, a Socratic tradition in epistemology and ethics in which human conversation is related to knowledge and truth, not only as a vehicle for expressing and transmitting truth but also as the locus of truth in which people live.[19]

When we turn from therapeutic method to the conceptual content (meaning) of the term "therapy", three points of convergence between Freud and Wittgenstein are striking. All have to do with their vision of what kind of knowledge (or understanding or insight) therapy serves as a tool for. One aspect is the revival of the Platonic idea that the knowledge Freud and Wittgenstein promise is knowledge of what we already know but have forgotten, or perhaps rejected or repressed. Another is Freud's and Wittgenstein's agreement that the worth of getting to know what we already know lies in breaking the spell of illusion. It is in the liberation from false impressions of knowledge rather than from the overcoming of ignorance that the recovery through therapy of what we have always known finds its emancipatory value. The third point of convergence is that acknowledgement is criterial for the kind of truth we search for in therapeutic analysis (Freud) or philosophy (Wittgenstein). For Freud, truth about our past-life journeys and their relevance for us is not to be judged on the scale of empirical confirmation *vs.* disconfirmation but on the scale of denial *vs.* acknowledgement. The same can be said about Wittgenstein's (and also Socrates') view of truth about the meaning of concepts. It follows that for both Freud and Wittgenstein the search they were engaged in was one in which questions of the will and questions of truth are conceptually inseparable.[20]

Let us now go back to the question about scientism and naturalism. Here there is a surface difference between Freud and Wittgenstein that may hide from view the relevance of the similarities we have

just observed. Wittgenstein's motto for his posthumously published chef-d'oeuvre, drawn from the mid nineteenth-century Austrian playwright Nestroy, was: "The trouble about progress is that it always looks much greater than it really is." In his notebooks Wittgenstein wrote that the spirit of his times is marked by belief in the advantages of progress in science and technology and that he finds this spirit alien and unsympathetic.[21] Freud could not have disagreed more. In his "official," explicitly professed self-understanding he held views that are scientistic in the sense defined above. Freud also subscribed to metaphysical naturalism.[22]

It would, however, be easy to exaggerate the difference between Freud and Wittgenstein in this respect. Wittgenstein was not critical of the idea that natural science and technology are progressive endeavours and successful in their own terms. Wittgenstein first trained as an engineer. As a young man he conducted experimental research in aeronautics and later he followed with interest developments in science, including experimental psychological research of his times. He did say that scientific questions do not awaken as keen an interest in him as do philosophical questions, but he was certainly not disinterested in science nor did he think science is in any way "low" or morally suspect. It was the way attitudes to, and expectations of, science and technology function in our cultural and moral economy that he took issue with. Science, according to Wittgenstein, is fine as it is, but just as our words do not always get their meaning from a referential relation to the world, so knowledge, authorised by science, is not all there is in the house of knowledge. On the other hand, Freud's "official" naturalism and scientism is not always easy to fit together, with the leaps of imagination in his theories. In particular, the elements of rapport that I have recorded between Freud and Wittgenstein—the central role assigned to dialogue in the search for truth; the search for truth as inquiry into what we already know instead of as a search for new knowledge; the idea that repression makes truth difficult to attain; the idea that acknowledgement may be an intrinsic aspect of some forms of truth, etc.—are all difficult to accommodate within a scientistic framework. It seems to me that it can be liberating for our understanding of Freud that he might have learned from Wittgenstein that the methodological and conceptual framework and therapeutic practice that he developed may be legitimate even if no such accommodation is searched for. Wittgenstein's message, let me repeat, is not that science is illegitimate. But scientism and naturalism are no more granted their legitimacy by science than

are their opposites. Why would we then accommodate our findings in philosophy or psychoanalysis according to the requirements—the standards of reason—of naturalism and scientism? The very idea that we should do so has led, as I believe, to unsurpassable problems for Freud's self-understanding. It continues, as we will see next in our case study, to haunt discussion of Freud's achievement today. New, freer ways forward for approaching Freud and finding out how his work can serve the enrichment of our understanding of self, others and the world can be reached, as I will suggest, if we overcome the temptation to think that we can and ought to prove that Freud's theoretical contributions either are or are not defensible on the terms defined by empirical science.

Part III

Linda A. W. Brakel and Adolf Grünbaum debate the scientific status of psychoanalytic theory in this volume. They mention, but keep in the margins, the question of the therapeutic value of psychoanalytic clinical practices and of psychoanalysis as a research method. I will follow suit and focus on psychoanalytic theory "as a general theory of mind." The question I shall pursue is: Should we expect that Brakel and Grünbaum will come to an agreement about the scientific status of psychoanalytic theory?

Brakel and Grünbaum have a basic disagreement over the classic issue of whether there is anything like a "dynamic unconscious." They agree, it seems to me, that one condition for the feasibility of the scientific status of psychoanalytic theory is that the answer to this question is positive. Brakel thinks that the answer is positive and Grünbaum thinks that it is not so. It is, however, not easy to sort out what exactly the nature of their disagreement is. In fact, sorting it out may be impossible, as I will argue below. On the face of it that may not sound right, however. So in our first step let us try to locate an issue that Brakel and Grünbaum might both see as decisive and decidable, meaning: an issue such that when it is sorted out Brakel and Grünbaum would come to an agreement.

In view of the contributions to this volume, one candidate for a decidable issue is the question of whether there are causally active repressed contents (Brakel, Chapter Three, this volume, Grünbaum, this volume). The experiment Brakel reports upon in Chapter Three (pp. 65–66) is my

proposed candidate for a source that may bring Brakel and Grünbaum to agreement and that may be, therefore, decisive in my sense. Before going on, let us pause to make some notes about the scope and quality of my discussion. Brakel tells us that Grünbaum has reported in an email to a third party (Shevrin) that he is "satisfied" "concerning" the experiment and "its attempts to gain empirical evidence for causally active unconscious conflict" (Brakel, Chapter Three, p. 66). Following this, Brakel is quick to point out that, nevertheless, she does not think that Grünbaum will agree that the experiment and the results thereof that she presents will bring about an agreement between him and her. Brakel writes: "[I]f Grünbaum holds to the views expressed in the current volume, he cannot really be satisfied regarding the causal powers of unconscious conflicts, his recent seeming acceptance of empirical evidence notwithstanding" (Brakel, Chapter Three, p. 66). I do not know whether Brakel thinks Grünbaum ought, rationally speaking, to "be really satisfied." I also do not know exactly what Grünbaum's email statement that he is "satisfied" means, nor will I speculate about whether Grünbaum thinks that the experimental results reported to him give him a reason, perhaps even a decisive reason, to change his mind about causally active unconscious conflict and also about repression and the existence of a dynamic unconscious. Moreover, I will not discuss Brakel's own interpretation of why she thinks Grünbaum "cannot be really satisfied" with what he in his email claims to be satisfied about. All these issues are better addressed by Brakel and Grünbaum than by me.

I will therefore discuss something else, namely the question of whether Brakel, Grünbaum or anyone else could reasonably expect the experiment that Brakel reports on to be decisive for Brakel's and Grünbaum's original disagreement about the scientific status of psychoanalytic theory. I will assume, innocently I think, that causally active unconscious conflict is a case of causally active repressed contents. I will also assume, much less innocently, that if we agree that there are causally active unconscious conflicts we will also agree, therefore, that there is a dynamic unconscious and that, therefore, the psychoanalytic theory is a respectable general scientific theory of the mind. I therefore confine the discussion to the following:

Are the experiment and the results thereof reported by Brakel such that Brakel and Grünbaum should find them decisive for their debate about whether there are causally active unconscious conflicts?

Even this limited question is, as such, far too large for us to answer comprehensively. I will not go into any of what we learned from the sobering intrinsic criticism between the mid 1930s and roughly 1960 that clipped the wings of the early Vienna Circle optimism about the verifiability of scientific theories. Nor will I discuss our question in the light of the various important themes in the philosophy of science that we owe to the contributions by Kuhn, Feyerabend, Charles Taylor, van Fraassen, or others. We know today that "post-positivistic" resources in the philosophy of science will always allow anyone not to accept an experiment as decisive. But there is still, I shall assume, a difference between willing to trick yourself out of a problem in order to avoid agreement and not being willing to do so; that is, a difference between well-intended cooperative search for truth and discussion that is not of that kind. I will here assume that Brakel's and Grünbaum's discussion is a case of the former and try to look at our issue from close up.

Now, let us consider Brakel's report on the experiment. The objective data is about: (i) brain states and changes between them,[23] and; (ii) how words are organised by persons. The interpretation of the data is that there are differences in category (i) that correspond with differences in (ii) such that they give us reason to say that unconscious conflicts cause differences in (ii).

Brakel and Grünbaum agree upon a whole lot of assumptions that are needed to make the interpretation attractive. Given this agreement, what might be the interesting reasons for disagreement about the interpretation and its conclusion? Brakel and Grünbaum appear to agree that the heart of their difference is in their views about the mental level. Brakel thinks that some brain states correspond with something mental that we can call unconscious conflicts and Grünbaum doubts that, at least initially (i.e., before reading the reports he is reported to have found satisfactory). This local disagreement is preceded by and presupposes agreements on the following: that there are both conscious and unconscious contents of the mind; that there are brain states and events that correspond with the foregoing; that correspondence in the second case is a case of causation but not of identity. (Neither Brakel nor Grünbaum is an eliminitavist.) It is in the context of these points of agreement that Brakel and Grünbaum agree to disagree. They then articulate their disagreement as a disagreement about the kinds of unconscious contents that there are.

Oddly, Grünbaum and Brakel have different clarifications to offer of what it is that Brakel believes there is and what it is that Grünbaum does not believe there is. They still seem to agree that they disagree, but to an observer it is not easy to know what, after the different clarifications, they disagree about. In other words, it is unclear whether their agreement to disagree is an agreement about anything specific; about anything of such clarity that it is possible to find out the truth about that which they disagree about.

Let us try to make this more concrete. To do so we take a step back and ask: if Brakel says that there are causally active unconscious conflicts and if Grünbaum denies that, what is the locus of disagreement? It is not, as we have seen, about the causal power of mental states whether conscious or unconscious. (Grünbaum and Brakel agree that both conscious and unconscious mental states can be causally effective.) Nor is it about the correspondence between brain events and mental events. (Grünbaum and Brakel both believe there is a correspondence.) So what it is about? It seems to be about whether some unconscious mental states (which are "causally active") exemplify something we can with right call unconscious conflicts.

Do we now know whether the disagreement between Grünbaum and Brakel is of such clarity that it is possible to find out the truth about that which they disagree about? Let us consider the following candidate for a positive answer: it is a fact that sometimes there are causally active unconscious conflicts. What kind of fact is this supposed to be?

One possibility is that some brain states are causally active unconscious conflicts. To the eliminativist, however, this smacks of humbug. She would say, rather, that there are causally active brain states, period. The eliminativist may also accept the mental as a valid category. But such acceptance is to her a question of terminology, about what we call the facts (how we name them) not about the facts themselves. To her, the facts about the mental will be the same as the facts about the brain, regardless of whether she accepts the mental as a term or not. Application of Occam's razor will of course speak in favour of an austere terminology. But if we forget about the razor there is to the eliminativist no harm in accepting also any further divisions of the mental into various categories. She might decide to stop talking about brain states and start talking about causally active unconscious conflicts. She might also decide to use these two terms interchangeably, as synonyms.

For the terminologically non-austere eliminitavist no differences in terminology are of any consequence as long as we remain clear that none of the further divisions we may indulge in should fool us to think that there are any factual differences at work. It follows that if Grünbaum and Brakel would be eliminitavists then there would be no fact such that it could make a difference to them. Their disagreement, they would agree, would be about words only, with no consequence, and not about facts.

Now if it is true that neither Brakel nor Grünbaum is an eliminitavist can we then clarify what fact they disagree upon if they disagree about whether it is a fact that sometimes there are causally active unconscious conflicts. What kind of fact is this supposed to be if we are not eliminitavists? The candidate answer is: a mental fact (i.e., a fact about a mental state or event). With this candidate for an answer, Brakel is right if there are mental facts that are facts about causally active unconscious conflicts. If there are no such mental facts Grünbaum is right. Have we now located their disagreement clearly?

If Brakel thinks it is a fact that the series 7777 occurs in the infinite expression of π and Grünbaum thinks it is a fact that it does not occur in that expression, do they disagree about something clear and definite?—About a fact perhaps?—And is one of them right and the other wrong?[24] As long as we insist that all meaningful questions (especially perhaps in science!) can be answered (in principle at least) if we get the facts right, then the answer has already been assumed. But with what right? Is it rational to presume that it is possible to find, through empirical research, using impeccable scientific methods, facts that will decide the point about which, if our discussion above is correct, Brakel and Grünbaum agree to disagree? This is an important question for present purposes. I will need three kinds of comments to respond to it. (Even if only the third kind will help us progress, the progress will not be recognised as progress unless the results of the simpler, first points are accepted, at least for the sake of discussion.)

First, as we proceed, Brakel, Grünbaum or any reader of this chapter may suspect that the discussion that follows is possible only because I have misidentified the issue. The misidentification would, presumably, be of the following kind: it would consist in my attributing, falsely, to Brakel and Grünbaum the agreement that when they disagree about the kinds of unconscious contents that there are, it is defining of their disagreement that Brakel supports the view that there are causally active

unconscious conflicts while Grünbaum opposes that same view. Now, I think it is possible that my construction does not seem *exactly* right to Brakel and Grünbaum. (Can we be sure, for instance, that I, Brakel and Grünbaum use the crucial words—causal, unconscious, conflict—in the same sense or in a precise enough sense? But interestingly, in view of our earlier observation about the role of acknowledgement in Freud's and Wittgenstein's conception of truth, it seems that the criterion for the correctness of my construction is the acceptance of it by Brakel and Grünbaum.) But even if I have constructed the problem falsely or carelessly I would still maintain that for the discussion between Brakel and Grünbaum to be the kind of discussion they both take it to be—an empirical, decidable one—it must be possible to find some construction of the kind I have offered that identifies correctly a question about the kinds of unconscious contents that there are, in such a way that we can answer the question by getting the facts right. In other words, even if there is some mistake in my construction, the general principle holds that Brakel and Grünbaum are in agreement that it is possible (and must be possible) for them to find a factual question such that they agree that they disagree about their expectations concerning its correct answer but agree that they can find out the correct answer; the answer they ought to agree upon if both are rational and they have got the facts right. If Brakel and Grünbaum step back from this commitment—the commitment to the notion (the agreement) that it is possible to identify a factual question, the answer to which (as they agree) will decide their disagreement—then they will need to revisit their pre-given idea that they know that their disagreement is a disagreement within empirical science.

I turn to my second preliminary. Can Brakel, Grünbaum or any third party say that if there is no fact that can settle their disagreement then relativism follows, for example, because our argument leads to some post-Kuhnian paradigm-relativism or post-Foucaultian dissolution of the difference between power and knowledge? I will not discuss this point here.[25] But I do want to say, with some emphasis, that the idea that if I am on the right track in my comments on the debate between Brakel and Grünbaum then relativism follows is as much a target of my criticism below as is the notion that there must be a fact to settle the point about which Brakel and Grünbaum agree to disagree. Relativism and scientism (of the kinds relevant here) are, from the perspective I try to articulate and offer as the way out, not distinct alternatives such that

one is right and the other wrong. Both are caught in the same web of naturalist and scientistic prejudice and confusion that is typical of our times.

Having said this, let me come to our real issue: is it rational to presume that it is possible to find, through empirical research, using impeccable scientific methods, facts that will decide the point about which, if our discussion above is correct, Brakel and Grünbaum agree to disagree? Are there causally active unconscious conflicts? As I said, I assume now a prior agreement between Brakel and Grünbaum that there are causally active mental states (or events or contents).[26] Is it then a fact of the matter that the factual differences reported by Brakel about time frequency of ERPs (evoked response potentials) are differences between the workings of causally active unconscious conflicts and other kinds of causally active mental contents? Is it therefore correct that the findings Brakel reports are about causally active unconscious conflicts? This is a question about how we know (or what we know if we know) what the correct description of a mental content (state, event) is. Philosophers have been in the habit of discussing the same question as a question about pain. Let us look at the pain question first.

If I say truthfully that last night I had a distinct pain in my left thumb, was there a brain event and a mental event corresponding with this fact? (I set aside the question whether this mental event—the pain or experience of pain—was causally effective.) Is the pain the brain event or the mental event or both? Is this a question about facts? Or about concepts? Or both?

Consider two cases. Case 1: I have what I call pain and have no access to facts about my brain. Case 2: I have what I call pain and have access to facts about my brain. This we can consider. We know what it is to consider Case 1 and Case 2. (This may also be problematised, especially the "my" in "facts about my brain", but I skip that.)

But now consider Cases 3 and 4. Case 3: I have what I call pain and have access to facts about my brain and there is empirically verified, systematic coherence such that when I have what I call pain then facts of a particular kind about my brain always obtain. Perhaps I can now decide to use facts about my brain as criterial for my use of the word pain when I speak of myself (of my experiences, of how I feel, of what happens to me). Case 4: I have what I call pain and have access to facts about my brain and there is empirical evidence that shows that sometimes (perhaps in 85–90 % of the cases, or perhaps in 40 % of the cases) there is a coherence such that when I have what I call pain facts

of a particular kind about my brain obtain. In Case 4 I can decide to start using the facts about my brain states that sometimes occur when I have what I call pain, or what I used to call pain, as criterial for my pain (i.e., as a standard for my legitimate use of the word pain when I speak about myself). This conceptual choice will remove the difference between Case 3 and Case 4. I will now, in Case 4, call my experience an experience of pain only when the corresponding brain state is verified. So, after my new decision about how to use the word pain, there will be in Case 4 as well as in Case 3 systematic coherence between what I call pain and facts about my brain states. Are the moves we consider in Cases 3 and 4 moves from a less enlightened world (from a more subjective language and less scientific one) to a more enlightened world (to a less subjective language and more scientifically responsible one), as I believe Daniel Dennett would say? Or do they constitute a grammatical category mistake, as I believe P.M.S. Hacker would say? Or are we here (as the present author thinks it is) dealing with something else that is not described well either as a case of progress or as a case of conceptual error?[27]

In the above cases we can replace the word "pain" with the words "unconscious conflicts." Our questions remain the same. How are we to adjudicate in these cases? And how are we to find our way with the questions we asked a bit earlier? We asked: is it correct that the findings (about time frequency of ERPs) that Brakel reports are reports about causally active unconscious conflicts? This is, to repeat, a question about how we know (or what we know if we know) what the correct description of a mental content (state, event) is. This question appears to lead us into philosophical debates in which participants seem to talk each other by, with no end in sight.[28]

The relief will come if we give up the idea that this issue allows for a straightforward solution. But can we give up that idea? Can we give it up without leaving the house of reason? Without compromising our commitment to science? Let us now reflect on these questions; one is the question about naturalism and scientism. If we assume that metaphysical naturalism is correct and that it allows that there is mental causation (e.g., that my will can be the cause of my action such that the "because" in "I gave you five euros because I wanted to give you five euros" is a scientifically honourable "because") can we then give up the idea (or "the requirement") that the issue has a straightforward solution? Perhaps we can, but there is little need to do so. But as I said earlier: that choice only brings back, with a vengeance, the question whether

the idea that naturalism is correct (and whether it allows for mental causation) is an honourable, scientific idea.

But this is also, we must note, a point of debate at which it seems to me that Brakel, Grünbaum and many others are in agreement. They agree to accept naturalism and not to ask further reflective or critical questions about its scientific status. It is a habit of our times that this is what we academics do and what we agree upon. This habit is the essence of scientism. The habit—the habit not to ask certain questions—is deeply ingrained. Our discussion, so far, about the difficulty of identifying a point, such that the debate between Brakel and Grünbaum would emerge as decidable, may give us reason to change our naturalist and scientistic habits. But I think it is not likely to do so. To achieve a change of habits, then arguments and considerations of a different, more subtle and charming sort, is probably required.

This is one of the deepest reasons, it seems to me, why Wittgenstein begins his *Philosophical Investigations* as he does, with a quote from St Augustine where he explains what he thinks about how he learnt to speak. In the discussion of that opening quote the chief target of Wittgenstein's criticism is the representationalism that is the counterpart in the philosophy of language to naturalism in metaphysics.[29] Representationalism is, with a first approximation, the idea that language is in essence communication about facts: it is the idea that if we get a good theory of how language can represent the world as it is (a good idea of how by making sounds I can communicate to you what the weather is like in Helsinki today) we get a good theory of what language is. An important philosophical expression of representationalism is truth-theoretical semantics. Its basic notion is that we understand a sentence if we know what is the case if it is true.[30] It goes with this notion of language that if we know whether the conditions obtain or not we know whether the sentence, which we have already understood, is true or not.

Is representationalism true or false? This is one of the core issues in the philosophy of language. Here we have no need to discuss that issue. Two other comments are needed for the present purposes: first, that the inadvertently presupposed truth of representationalism sets the stage for the discussion between Brakel and Grünbaum. The truth of representationalism is a condition (not the only condition) for the debate to run the course it runs. This comment is probably not controversial. But our second comment may be so: representationalism is neither true nor

false. It is of course impossible to argue for this view here. (It is even better to leave it open whether "argument" is what is needed, and can be provided, to settle the matter about the truth and falsity of representationalism.) But the comment requires some explanation. What I will do is to put my cards on the table and list six things that we can learn, as it seems to me, from Wittgenstein's later philosophy about representationalism. The first is that there is both truth and falsity in representationalism. The second is that not all language is answerable to representationalism: there is legitimate language that is not representational and such language is no less central to the understanding of what language is than is representational language (if there is such a thing; or in the sense in which there is such a thing). The third is this: representationalism gives us a binary theory of meaning. According to this theory sentences divide up into those that abide with the restrictions articulated in (a correct) truth-theoretical semantics and that have, therefore, meaning, and others that are not sentences at all but complete nonsense. The fourth point we may learn is that Wittgenstein does not lead us towards a new binary theory in which a new "grammatical", "purely descriptive" theory of the limits of language or a "therapeutic" method whereby we can investigate concepts provide for us what representationalism, falsely, tried to provide; namely a way of dividing up language into that which has meaning and that which does not have meaning. The fifth point we may learn is that Wittgenstein's discussion of representationalism tells us that there are ways of searching for truth and making progress in that search that do not lead to new knowledge, resulting in a loss of interest in the question of whether we have gone wrong in terms of reason, because such ways have been pursued. The sixth lesson that we can derive from Wittgenstein's discussion is that it may be more rational to investigate debates about relativism, naturalism, scientism, etc. from the point of view of the truth of each and the falsity of each without insisting that there will or ought to be a conclusion at the end of the investigation (about, say, the truth/falsity of relativism, or about any other matters at hand).[31]

If the charitable reader accepts, for a moment, these ideas, then what follows for our effort to make sense of the debate between Brakel and Grünbaum? It follows that we can turn the axis of our investigation around. We will no longer look for empirical facts that can confirm or refute either party to the debate. That does not mean that we will lose interest in neuroscientific research of the kind Brakel reports. We

may still see her report as contributing something—even something satisfactory—to the debate between Brakel and Grünbaum. But we will not think that we have accounted for the contribution and the satisfaction it offers if we have taken a stand on the question of whether there are causally effective unconscious conflicts or not.

Part IV

Where does our discussion leave us?

Let us ask: If we give up the notion that naturalism, scientism and representationalism are true or false what follows for the Freud wars? The discussion above suggests that it follows that we will no longer think that anything has necessarily gone wrong if the war does not come to end. Our tragedy is not that we cannot find out the truth about the scientific status of psychoanalytic theory. Our tragedy is not even that we cannot find out the truth about the scientific status of neuroscientific theories of the mental or of, say, evolutionary explanations of behaviour. Our tragedy is that we think any of this is a problem for us. Only if we can break the spell of those prejudices of our age that force us to think that there is a choice for us to make, dictated by the demands of reason, for or against the scientific status of Freud's legacy, and another choice to be made for or against the neuroscientific paradigm in the study of the mind, can we hope to investigate freely what, if anything, there is to learn from Freud and from neuroscience in the study of the human being, of society, and in the search for self.

CHAPTER NINE

Psychoanalytic research with or without the psyche? Some remarks on the intricacies of clinical research*

Anna Ursula Dreher

Preliminary note

Beginning with Freud, psychoanalysis set out over 100 years ago to scientifically fathom a difficult research subject: the human psyche—a project that is far from finished. Some of the analytic ideas did not find everyone's approval, like the importance of libido and aggression, or the relevance of unconscious processes. Beyond the contents that were felt to be irritating, the scientific project of psychoanalysis has been criticised time and again. Above all it was pointed out that *science* does not have the appropriate means at all to investigate the psyche as psychoanalysis understands it: the possibilities of science would only suffice to examine human behaviour or the brain, but not the inner life of humans. In this paper the thesis is put forward that such a limited view can only be reached if one has a narrow understanding of science. If one is to acknowledge that there is not "the" science, but several competing, partly complementary views about what science is, then one

*Translation by Eva Ristl.

would easily find ways and means to accept the psyche as a subject of scientific research and to explore it. In this paper a number of themes are taken up that are being controversially discussed in the field of subject, methods and aims of science, as well as in the field of fundamental and applied research. I am aware that these are themes that may be far from clinical practice, sometimes even far from research practice, but decisions concerning these issues can have immense consequences on how clinical research in psychoanalysis is practised.

Psychoanalytic research and the analytic situation

What is the core competency of psychoanalysis? It is certainly not the analysis of brain or behaviour; these fields are cultivated by others. The core competency of psychoanalysis is, as the term implies, the analysis of the human *psyche*—even if this may sound trivial. While at first only the psyche of the patient was focussed on, in the times after Freud the way in which the psyche of the analyst is involved in the analytic process has become increasingly elaborated. A number of theoretical assumptions about the nature and functioning of the psyche have been inferred from analytic-therapeutic work, and these assumptions in turn make up the theoretical background of this work. Research in and around the analytic situation has always been and still is an important driving force through which psychoanalysis is essentially further developed. Not only clinical research with process and outcome analyses, but also extra-clinical research, like infant and developmental, ethno-psychoanalytic, or neuro-psychoanalytic, or also conceptual research—all have a constant reference to those theories and models that were and are continually being refined in the analytic situation.

Keeping this privileged status of the analytic situation in mind, the guideline for me when discussing clinical research is that psychoanalysis is more than just a psychotherapeutic procedure, even though this is the dominating aspect in the perception from outside. Freud had already pointed out by way of a triple reference that, for him, psychoanalysis referred to:

1. a procedure for the investigation of mental processes which are almost inaccessible in any other way,
2. a method (based upon that investigation) for the treatment of neurotic disorders and,

3. a collection of psychological information obtained along those lines, which is gradually being accumulated into a new scientific discipline. (Freud, 1923a, p. 235)

As (1) a procedure for the investigation of mental[1] processes, psychoanalysis uses a series of methods of which free association and dream interpretation have become the most popular. Concerning the almost inaccessible mental processes of interest, the focus is in particular, but not exclusively, on the dynamic unconscious processes within and between humans. These processes, which on the one hand constitute the indispensable "trademark", are part of the "brand" of psychoanalysis of which the criticsclaim from *their* scientific point of view that such unconscious phenomena in the understanding of psychoanalysis do not even exist. The range of indication for psychoanalysis as (2) a therapeutic treatment method is, meanwhile, seen as much broader than initially set out in Freud's 1923 quote; today we not only treat neuroses successfully but also other disorders. To become a (3) recognised and independent scientific discipline, however, is an aim that psychoanalysis seems to have only partly achieved; it is at least not securely anchored in academic life. Nevertheless, psychoanalysis possesses a series of models of how the human psyche functions and of interpersonal exchange, all of which have been successfully justified in clinical practice and in clinical research. The guiding metaphor of psychoanalysis connected therewith is the "investigation of the unconscious": it refers not only to the daily clinical work with patients but to the overall scientific project of psychoanalysis that builds upon it to develop models of the psyche and thereby create a new psychology.

For some analytic authors the term "clinical research" only refers to the analyst's activities in the analytic situation when exploring the intra- and interpsychic processes. I do not use the term "clinical research" in this narrow sense, but in addition I include all research activities focusing on the phenomena in the analytic situation, seen as the privileged place of discovery as well as of justification. That means: I think in clinical research that analytic researchers other than the treating analyst *alone* can benefit and also use other methods than the analytic ones alone. Any practising analyst may be called an explorer or an investigator, but I would not automatically see him or her as a researcher—at least not in the common use of the term "research" in a modern, scientific context where the discursive and thus social element is a necessary part in an

interdisciplinary field. In research, after all, we learn by experience *and* by criticism, not least from those who have a different opinion.

All methodological themes, whether they be theory, research, methods, or worldviews, are not genuine psychoanalytic themes, but belong to the core competencies of science in general. Whoever in psychoanalysis engages in such a theoretical area as scientific research is stirring up a hornets' nest (Bolognini, 2010). It is commonly understood that the secured assets of scientific knowledge are sedimented in theories, and research is seen as the motor for scientific progress. On the one hand, there is consensus about the fact that research is an indispensable part of every science; on the other hand, especially in the human sciences, there are quite controversial beliefs according to which criteria and with which methods scientific research should be conducted. The controversies are mainly based on the differing beliefs about what science is and how science should work at all (see Dreher, 2010). Research can be considered and organised from the perspective of its subjects, its methods, or its aims. Although today scientists often define their research activities by the canon of scientifically recognised methods, I do not want to proceed from the methods or aims, but from the *subject* of research, as only the subject allows us to infer which methods are at all possible and adequate, and which aims can be reached.

On the research subject

The subject of clinical research is, in short, the analysis of the psyche; that means the elucidation of the intrapsychic and interpsychic processes going on in the analytic situation in and between patient and analyst. It is remarkable that the noun *psyche* is often negatively connoted and seems to be used less and less, although the adjective *psychic* is commonly used. The psyche is a relative of soul and mind. I prefer the traditional term "psyche", because the term "mind", similar to the term "mental", nowadays tends to be cognitively shortened, and the term "soul" is often religiously charged. I am aware that the suggestion to consider research from the perspective of the subject "psyche" and thus with epistemological interest is rather old-fashioned, the more so as "psyche" is an old-fashioned term. The psyche, which, depending on the chosen etymology, is either a nymph, a beautiful daughter of a king, or—philosophically—the breath of life, is understood in psychoanalysis as the totality of the inner life of human beings. Freud in

the very beginnings in the topographical model first organised the psyche theoretically according to the degree of consciousness; later, in the structural model, in terms of the mental agencies, and since then, generations of analysts worldwide have been continuously elaborating these concepts. Psychoanalysts, in theory and in their clinical work, and consequently in their clinical research, are dealing with the structure and the dynamics, development and maldevelopment, normal and disturbed functioning of the psyche in its relationship with itself and others. (A comprehensive discussion regarding the plurality and complexity in the internal world in its involvement with the external world can be found in Bolognini, 2010.)

The emphasis on the psyche has a reason that is equally pragmatic and in line with reality: in our practice we do not see "mental disorders" with ICD or DSM codes, neither do we get to see "abnormal behaviours" or "abnormally functioning brains". But what we do see is people with their distress and problems, their sufferings and psychic pain; with their biographically grown personality; and they have their family, social, cultural, ethnic, religious and economic provenances and backgrounds—just like us analysts. Of course, patients can be classified as disturbed or mentally ill, or can show abnormal behaviour, and of course they have brains that may function in a deviant mode. But first of all they are human beings who meet with us analysts, that is, in the specific analytic situation—an interaction of two individual actors with their respective psychic modes of functioning. For the analyst during the analysis and also for the researcher who researches all this, clinically relevant phenomena may show in manifold modes, behaviours, gestures and expressions, in feelings and affects, libidinous, aggressive or other motives, dreams or associations, fears or intuitions, by following social and cultural rules, or by violating such rules. And last but not least, and this is not very astonishing in a talking cure, they show in syntax, semantics, the pragmatics of what was said; and in phonology, especially in the kind of prosody it was said with—and—in what was not said. Mainly, verbal data are generated in a stream of non-verbal cues. Consequently, the material that is relevant for psychoanalysis cannot be limited to individual channels or modes; it is, in a certain sense, a non-reducible multi-channel phenomenon. Psychic pain and suffering of the patient and the analyst's responsiveness to it and containing it play an essential role: these are phenomena that are characterised by the perception of conscious processes, but also

by sensations of preconscious and unconscious processes going on in and between the two protagonists. Of course, both have different roles, specified by a fundamental asymmetry. Analysts are to follow the rules of analytic neutrality and abstinence and should—this is the reasoned expectation—distinguish themselves by clinical experience and competence. Only when these conditions are met is the kind of space created in which phenomena can show and unfold and onto which we then direct our clinical as well as our research attention. As we know from criminal cases, everything, every minute detail, may be relevant and may, at some stage, develop significance; this is also true for the analytic process and, correspondingly, for the research process. What goes on between analyst and patient, like in any other human relationship, is a holistic event that cannot be split up into a controllable set of variables—without loss of substance.

One of the main aims of clinical research is to describe this analytic process as precisely as possible and thereby better understand and reconstruct what happens in the treatment, which we traditionally describe in terms of transference and countertransference, resistance and working through, etc. Besides this intention, our attention is focussed on the more general research aims, namely to strengthen the theoretical basis and expand it, to improve the use of concepts, and, finally, to further develop our models of the psyche. We are then also dealing with the consolidation of the psychoanalytic image of man, which attempts to do justice to the double nature of human beings. I agree with a classification of psychoanalysis that played an important role in the Green-Wallerstein debate; there, psychoanalysis is seen as a science between nature and culture (Green, 2005; Wallerstein, 2005a, 2005b). Since its beginnings psychoanalysis has always gained by moving in this field of tension between *nature*—that is to say, paying attention to human biology in the form of genetic predispositions, structures and programmes designed by evolution—and *culture* of human beings—that is to say, paying attention to the results of family, social and cultural socialisation, and to historical givens. It has always been of particular interest how both sources form the individual psyche and what this means for the psychic reality of each individual. The status between nature and culture has some important methodological consequences. This is basically very interesting, but, certainly, difficult localisation suggests that psychoanalysis may have the privilege to use scientific methods from all three domains; from the natural sciences, the cultural sciences, and

from psychoanalysis itself. And along similar lines, this image of man has a methodologically highly relevant consequence: there may be only one external reality that is the same for all people and for all scientists; but there are, strictly speaking, as many psychic realities as there are people. This uniqueness and diversity of the inner worlds cannot be ignored if one wants to focus on the phenomena in clinical research.

The psyche between brain and behaviour

In order to shed more light on the psychoanalytic exploration of the psyche, it may be helpful to look at two neighbouring disciplines that have become powerful in the last decades and that both study subjects related to the psyche:

- the *behavioural sciences* prefer behaviour as their evidential basis, often linked with the belief that behaviour can be objectively observed and reliably measured. The psyche operates secretly, it is at first not directly observable. And to what extent it can be measured is a difficult question;
- the *neurosciences* investigate the functioning of the brain, mostly through imaging procedures. Some even claim that one can see on these images how the mind functions. But how the functioning of the psyche depends on the functioning of the brain is an open question, too.

The huge interest of today's society in brain research can be perceived by the fact that, currently, research programmes in the billion dollar range are being rolled out; in the European Union, for instance, the Human Brain Project where the brain as a whole is to be simulated, and in the USA the Brain Activity Map, where the totality of neuronal activity of the brain is to be charted. The relevance of behavioural sciences can be estimated by the fact that cognitive behavioural therapy, which is derived from it, is the most successful competitor for psychoanalysis in many countries, both in the psychotherapeutic care market, and in the academic milieu. In mainstream research, both disciplines have a self-understanding that is mostly empiristic[2] and often based on an understanding of science that is derived from the natural sciences.

The proximity of the analytic research subject to that of both disciplines results from the fact that the activities of the psyche, Freud's

"psychic apparatus", show themselves at least partly in overt behaviour and need a brain in order to function. While the brain may be regarded as a cognitive organ that can be studied as isolated from the rest of the body, for psychoanalysis there is no such strict separation possible between psyche and soma. The analytic ideas on psychosomatics, on embodiment, and on the inner world of representations, which also include body images, are seen as a reflection of complex interrelationships between psyche and soma. Philosophers would put it this way: psychoanalysis prefers a different solution to the mind-body problem than, for example, the materialistic monists who lastly only know matter, Descartes' res extensa. The differences of the analytic research subject to that of both disciplines arise because there are some specific themes that specifically emerge in the context of the psyche. Mention should be made here only of three main topics: the inclusion of (1) the meaning of behaviour and action, (2) the role of dyadic interaction, and (3) the reference to the individual biography.

1. Psychoanalysis is by all means interested in the observable stream of behaviour to draw conclusions about state and the functioning of the psyche. It, however, takes a much broader approach: that which is recordable as a data stream (also with technical equipment) cannot be separated from the meaning of the behaviour and all actions in the current situation, especially not from the effects of the behaviour on others. As concerns both aspects, psychoanalysis adds a further specificity: both can have intentional as well as unconscious aspects. Psychoanalysis tries to capture this in the analytic situation, especially through the analysis of the transference "between" patient and analyst, as well as through the self-awareness of the countertransference "within" the analyst.
2. Therefore, in psychoanalysis, the patient is not seen as a single case and thus as a monad, but analyst and patient are seen as acting in a dyad; in a bipolar field within the framework of the so-called two-person psychology. However, for psychoanalysis, the psyche of both—above all in this dialectic relationship of transference and countertransference—can be in an exchange relationship of a special kind: the mechanisms of projective identification are mentioned here as only one example for possible psychic entanglements. In these cases an exact localisation of the respective phenomena to the psyche of one of the actors is difficult or even impossible; that means it is not

clear, sometimes, where the aggression, the wish, the fantasy, etc., initially came from.

3. Last but not least the reference to the individual biography: psychoanalysis claims that in the analytic situation there is not just a "patient with a specific disorder" meeting a "therapist with matching manual", but rather two human beings with their very own physicality, biographically grown personality; with their internal fantasies, wishes, dreams, daydreams, anxieties, and inner conflicts—that is, with their own individual psyche and subjectivity assigned to them. Therefore, the same applies for each dyad of patient and analyst as for snowflakes: in high resolution, none is exactly like the other, because each individual snowflake manifests the history of its origin and development.

The analytic investigation of these psychic realities has interesting interfaces with the investigation of brain and behaviour. But the psyche itself is not only an interface; it is a domain between biological hardware and the external world, with its own structure, mechanisms, dynamics, its own formative power, and it requires its own methodic access. As Carmeli and Blass pointedly remark: psychoanalysis is concerned "with the understanding of meanings and the role of interpersonal discourse" (2013, p. 391), and "areas in the brain are associated with functions, not with content" (p. 400). Take as an example the defence mechanisms: it could be of interest to know where and how defence mechanisms are represented in the brain and the manifold ways in which they may show up in indicators of behaviour and actions. But psychoanalysis focuses on how they function intra- and interpsychically and on which role and meaning they have for the individual in a special situation. Take as another example of psychic change: current neuroscientific findings about the plasticity of the brain are very interesting for us; however, we currently do not know how the plasticity of the brain is related to the plasticity of the psyche.

It can thus be summarised that despite the related yet not identical subject areas, psychoanalysis and the neuro or the behavioural sciences differ in substantial aspects. However, the gap between brain and psyche and the gap between psyche and behaviour are both of huge scientific concern: psychoanalysis investigates both gaps from the psyche's point of view and pursues its own specific access to the psyche—the points of contact or overlap with neighbouring disciplines on the level

of theories, methods, and findings, and which ones they are, remain to be fathomed (important considerations concerning the relationship between Freud's structural model and new findings about brain functioning are made by Solms, 2013). The orientation of psychoanalysis is traditionally integrative and interdisciplinary: what psychoanalysis postulates always touches on the domains of other disciplines and should therefore at least not contradict their secured knowledge. Freud and many analysts after him took into account the state of the art, but of course on the basis of the methodological ideas and the secured knowledge of *their time*. Many analysts have, again and again, made interesting references to the neighbouring disciplines of the time whenever constructive links emerge—for instance, links between ego psychologists and learning theory (e.g., Hartmann, 1964), and between object relation theories and attachment theory (e.g., Fonagy, 2001). There is also the example of neuropsychoanalysis that attempts to build a bridge to modern sleep and dream research as well as also to trauma research (e.g., Solms & Turnbull, 2002), just as ethnopsychoanalysis has attempted to do with ethnology and social psychology (e.g., Erdheim, 1982). It would be detrimental for the scientific reputation of psychoanalysis if its theory, today and in the future, would be based, unchanged, on those ideas and theories that had been formulated in the sciences in Freud's time.

Science(s) and psychoanalysis

The history of the relationship between psychoanalysis and other sciences is precarious; another hornets' nest, at least for analysts. Since its founding days psychoanalysis has been exposed to vehement attacks from the established sciences. Occasional recognition cannot cover up for the fact that psychoanalysis has often been devalued by giving it the devastating label "unscientific". The quality seal "scientific", however, determines public reputation and prestige, institutional power, as well as accesses to the academic and the health care system and thus to money. The hostilities from, above all, medicine, psychology and philosophy have not totally disappeared. Again and again we come across the well-known "Freud bashing": so, supposedly, Freud was a liar; psychoanalysis is a phantasm and has nothing to do with science; the psychoanalytic organisations are sects; the analysts are gurus or money grabbers; and, in any case, they are not scientists …

However, quite the contrary is right; all analysts have in fact undergone a double scientific socialisation: in their basic academic studies, which are mostly in medicine or psychology, they have been acquainted with the respective standards of scientific work and research. Depending on subject area, country, and culture, this may lead to different understandings of science. Thus medical doctors often have different views about this than psychologists, or pedagogues, or educators; natural scientists, again, have a markedly different understanding of science than many cultural or social scientists. These different views are not only to be found between but also within the disciplines. The English-speaking world with its evaluative distinction between "science, humanities, and arts" introduces an understanding of "scientific" that is historically and culturally determined in a way that many continental Europeans would not share. Which evaluations are meant becomes clear in Kernberg's attitude: he sees "cultural dispositions toward empirical research that dominate in the Northern Hemisphere, particularly the Anglo-Saxon and Nordic countries, in contrast to an openness to a more philosophically inspired, subjectivistically focussed attitude in Latin countries" (Kernberg, 2006, p. 921). Are Anglo-Saxon scientists really disposed toward empirical research contrary to Latin philosophers with subjectivistically focussed attitudes? Besides, Kernberg overlooks that empirical researchers also start from a number of methodological and ontological assumptions—that is, philosophical assumptions concerning the state of the world. For good reasons, if the research subject may require it, one could come to different methodological decisions on the basis of other philosophical assumptions—without leaving the domain of science. Nevertheless, no matter from which basic academic fields they come, with their additional psychoanalytic training analysts get to know a second discipline, which often is rather different from their first. And what is more, they are unfortunately confronted with questions like: which one is "the true" psychoanalysis, which school, which tradition, which theory, which analytic language game, which authority figure would be best to follow?[3] One conclusion therefore is this: in psychoanalysis, analysts face controversies already encountered in the sciences they originally came from. Everyone has to find their own personal way, then.

Double scientific socialisation of analysts in different academic cultures, characterised by different scientific worldviews, results in different beliefs about research, about its subjects and aims, and

especially about the canon of admissible methods and thus methods of "scientific" repute. Questions come up like: who awards the label "scientific"? Should psychoanalysis adopt rules and criteria that have been developed by other sciences? If so, which would be the best leading science for psychoanalysis? Medicine, or psychology, or no particular science, but better a special manner of doing science, like, for instance, the mode of neo-empiricism? We are familiar with discussions of whether psychoanalysis—if a science at all—would be a natural science, a hermeneutic, a human, social, cultural, or a life science, or if psychoanalysis is perhaps an autonomous science (although we know that independence does not work as a self-attribution). If it was unequivocally clear what "true" science is and what the label "scientific" meant at all, an answer would be easier.

I would like to point out a further complication: all scientific belief systems are in constant flux. What theories are, what the best methods are, how research works, and even what truth is—all of this is not only occasionally controversial, but also contingent. Unlike in the religions, there are no eternal truths in sciences and no final certainties. In the human sciences each so-called *turn* in the last century has generated new belief systems with respect to the logic of research; the behaviouristic turn as well as the linguistic turn, the cognitive turn, as well as the neuro turn in the recent past. And each turn has produced modifications in the selection of phenomena that were considered relevant, and as to what methods were judged admissible, and as to the aims that should be pursued. Futhermore, just as psychoanalytic theory and its concepts are subject to historic change, psychoanalytic research is too, whereby change does not happen continuously, nor without friction. Very often, the "new" does not like the "old", and vice-versa, and there are many so-called fundamental debates that actually are not only decided through argument, but, as Kuhn has so impressively demonstrated, by the "biological solution" (Kuhn, 1962). The old dies out; the new takes over, until such time as the new looks old and is replaced once more.

Summing up the arguments: there is a plurality of controversial, sometimes irreconcilable, belief systems, and unfortunately far too little constructive dialogue. Dissenting opinions always become problematic when one's own conviction is linked to the fatal tendency to devalue the opinions of others—"unscientific" as well as "unanalytic" are often used as fighting terms. As one knows, especially as an analyst, permanently suppressing or denying the conflicts is not really

useful. So, for example, as a reaction to the increasing complexity of worldviews, the harmonising wish for unity and for an ideal order often comes up with urgency; a longing for an Archimedean point, where one's own beliefs can find a secure base, free of further criticism, and where contention ends at last. In the 1930s, there was the unity-of-science movement; their neo-positivist representatives had once hoped that all methodological argument could be dissolved in a unified understanding of science, with physics, at the time, being considered the ideal leading science, mathematics and logic the ideal language games—and all other sciences had to follow (see Feyerabend, 1962; Kuhn, 1962; Nagel, 1951). Modern empiricism picks up these ideas, and also, in the analytic field, research is often understood in this neo-positivist sense, with operations of measurement, decomposition in variables, experimentation, causal explanation, use of (mostly linear) statistics, and testable hypotheses—emphasising a unity of research methods, so to speak. There are similar developments in the domain of psychoanalytic theory and practice, and many analysts not infrequently find themselves hoping that, in the face of all the controversies of the different schools, a common ground may be found, worldwide and for all times (an impression of the discussions in psychoanalysis can be found in Green, 2005; Wallerstein, 2005a, 2005b).

None of the strivings to come to an agreement about a uniform understanding have found a successful outcome. It seems to be making sense, then, to first of all learn to live with—and to recognise—the diversity of beliefs, even if it takes some effort to orient oneself, and even if controversy and dialogue constrain resources. Diversity may also mean creative potential, but one can only use this potential if there is cooperation, and cooperation requires acknowledgement. In science, the symmetry rules of recognition apply, so if we want to have others learn from us, we must be willing to learn from them. A prerequisite for this is an eye-to-eye dialogue. Mutual recognition, however, is often a problem for both sides. The reasons for the problematic relationship between other sciences and psychoanalysis are not only to be found on the part of the other disciplines. Some analysts do not want to be affiliated with any type of science at all, as, for instance, Green has formulated pointedly: "Psychoanalysis in my view is neither a science nor a branch of hermeneutics. It is a practice based on clinical thinking that leads to theoretical hypotheses" (Green, 2005, p. 632). Aside from the fact that it is not clear what Green means by "theoretical hypotheses", without reference to science, I cannot share his distance to science. His

stance may take some pressure off in the short run; in the long run it would be a mistake to withdraw into a kind of analytic siege mentality. Sooner or later, though, such an attitude would lead to a complete disconnect of the analytic discourses from all other scientific discourses, with the danger that psychoanalysis develops into what its sharpest critics have always maintained: an esoterically tinted and backwards-looking sect—a development that would be diametrically opposed to Freud's progressive aspiration. If we engage in a scientific discourse with others concerning our clinical work, we should succeed in making plausible and understandable what exactly happens in the analytic situation; what happens "in" the patient and "in" the analyst, and what happens "between" both. For only then could it become evident that our clinical work has nothing to do with magic, mysticism, or miracles; and that the analytic process of exploring the psyche, especially the unconscious, for curing purposes is indeed highly complex and methodically very difficult to grasp, but that it definitely can itself be elucidated conceptually and theoretically.

A spotlight on the psyche in clinical work—an example

How do we describe what actually happens in an analytic treatment? I would like to illustrate this with an example of a paper by Ogden, which is both clinical and theoretical. In his paper "On three forms of thinking: Magical thinking, dream thinking, and transformative thinking" (2010) Ogden wants to provide evidence for his general thesis that psychoanalysis has shifted from the understanding of the symbolic meaning of dream, play and associations to the exploration and understanding of the processes of thinking, dreaming, and playing. Ogden thinks that this shift from *what* the patients think to *how* they think has significantly altered our clinical approach; Winnicott and Bion being the most important contributors to this view of psychoanalysis.[4] Ogden shows how the conceptualising of thinking—the extension of the idea of dreaming into waking life, with the focus on transformative thinking—is of value to him when talking with patients; and how it is of value in understanding what is going on in a specific analytic situation, and he also shows how psychic change functions. He uses example sequences from two high-frequency, long-term analyses, and unfolds the cases in a lively manner, as a narrative interspersed with quotations, mainly reporting critical events in the analyses. When reading, we are drawn into Ogden's very

personal experience with his patients by his literary writing; in no way is it considered a disadvantage in a science if essays also display literary qualities.

Ogden's paper is both classic and modern. Classic, because he, just as many analysts since Freud, makes his theoretical frame of reference explicit and presents cases in narrative vignette form, demonstrating the constant interaction of theory and clinical practice. Even Popper would be content: Ogden meets Popper's dictum that all data, perceptions, thoughts and feelings are always collected in the light of one's own theory. Ogden's paper is *modern*, because he uses the currently prominent Bionian analytic language game, showing how he modifies it for his own clinical purposes. He works with fuzzy concepts that he sees in dialectic tension to one another, and he explicitly avoids exact definitions. The vagueness of his concepts gives space for an elastic concept use—very helpful in the analytic work, even though this may not facilitate communication.[5] Finally, Ogden's modern use of countertransference is remarkable: the description and reflection of his personal involvement in the dyadic analytic process, and his using the mode of reverie as a diagnostic indicator. How does Ogden explore this intra- and interpsychic field? I would like to demonstrate this with a short passage from the five-session-per-week analysis of Ms Q, whom he describes as drawn to omnipotence, with magical thinking taking up a large space. His analytic work with Ms Q often resembled some kind of Sisyphean task, but finally the analysis ended for the patient in a solid re-organisation of her world—to use Bion's concept: in a catastrophic change. The basis of such a change is initiated by "transformative thinking", which Ogden describes as something more than "ordinary" dream thinking.

Ogden writes about one episode of this year-long work with Ms Q:

> [My] understanding … created a psychological space in which a rêverie experience was generated (by the patient and me) … (Ogden, 2010, p. 327)

> [In one session] I closed my eyes for a few minutes … I suddenly became very anxious. I opened my eyes, but for a few moments did not know where I was, what I was doing, or whom, if anyone, I was with. My disorientation did not lift even after I saw a person lying on the couch. It took me a few seconds more to deduce where I was, who the person on the couch was, and what I was doing there (i.e., who I was). It took several more moments before this

> deductive thinking was succeeded by a more solid sense of myself as a person and as Ms. Q's analyst. This was a disquieting experience that led me … to become aware of my own fears of losing myself in the psychological-interpersonal experience in which Ms. Q continuously reinvented reality and reinvented herself and me. It seemed to me that Ms. Q was showing me what she could not tell me (or herself). (Ogden, 2010, p. 325)

> The type of dream thinking … involved a form of self-reflection in which I drew my own experience, and my conception of the patient's experience, into relation to one another, i.e., I made use of my experience of losing myself to make an inference regarding the patient's experience of losing herself. (Ogden, 2010, p. 332)

Ogden refers to "dream thinking" when he describes how he intuits and senses the psychic pain of his patient in the very special mode of reverie, when usual categories of rational thinking become semi-permeable. According to Ogden, this mode allows a bridge to be built between patient and analyst, between intra- and interpsychic processes.[6] He verbalises these processes, which hardly show in behavioural data. He infers them on the basis of non-sensory perceptions by this special sort of introspection; but he remains the sole authority for these data. It is also illustrated how he follows up his inklings; how these are densified to assumptions, abductions and hypotheses, and how this tentative search again and again leads to impasses or obstacles.[7] The paper elucidates all of these subtleties in a precise way. Ogden carefully elicits the interweaving of his concepts with his clinical data, describing the process in micro-moments, inviting the reader to join him in the development of his understanding. Ogden communicates all this in a theory-guided manner, putting it up for discussion. Doubtlessly, this is also due to his long-time clinical experience and on his having been embedded in the general analytic discussion for years.

Reading Ogden's paper gives insights into how an analyst thinks and works. This dependency of important data from the analytic process on which Ogden reports "from his psyche" is certainly a great challenge for research. Nevertheless, his report is a good example of how to describe the place where, traditionally, clinical research starts from. And this is where, from its beginnings, the gold of psychoanalysis has been mined.

The classic "conjunction between cure and research"

Ogden's text is a contemporary illustrative example of a clinical study. Such classic, single case studies remain "solitaires" in the field of systematic research approaches; they have unfortunately some methodological weaknesses, but definitely a number of attractors. This classic form of the essay has a long analytic tradition and has evolved for good reasons as a useful pattern of documentation and communication—as a specific holistic, "all-inclusive" kind of report that presents clinical phenomena illustrated simultaneously by empirical data, conceptual reflections, and theoretical ideas. This analytic standard of transporting our work into the public is definitely worth being maintained as an important—first—step in our research process. There is hardly any other way of grasping the work with patients except through such precise and condensed descriptions close to the source—"deep and thick descriptions", as the anthropologist Geertz (1973) called this method.

This classic attempt to scientifically clarify the context of discovery of clinical research is sometimes called conjunction research—an important research paradigm in the history of psychoanalysis. The self-understanding of this approach was that in analyses one could both "cure" and "research" at the same time, an understanding that to this day is still shared by many analysts. This belief is based on the idea of the "conjunction between cure and research" (Freud, 1927a, p. 256)—in German "das Junktim zwischen Heilen und Forschen" (Freud, 1927b, p. 293). What the German verb forschen meant back in the times when Freud formulated his famous "conjunction" was: exploring, searching, investigating, and studying, and not what we nowadays mean by "research". Maybe one should rather speak of a "conjunction between cure and investigation", and even Freud says somewhere else in regard to the development of his treatment method that "one and the same procedure served simultaneously the purposes of investigating and of getting rid of the ailment; and this conjunction was later retained in psycho-analysis" (Freud, 1924f, p. 194).

A convinced conjunction researcher will, for instance, give as arguments for his approach that the evidence of the research process—above all, the unconscious processes, which elude observation and measurement in a similarly nasty way like certain quantum particles in physics—can only be "detected" by the analyst in a state of evenly suspended attention or reverie. Because some of the clinically relevant

phenomena, verbal or non-verbal, are hidden and volatile, they can only be captured in vivo—that is, extra-clinically or under laboratory conditions.

Critics usually point out a series of methodological and methodic deficits of this conjunction research. Among other things, they comment that it only deals with single-case research and with qualitative data, that hypotheses cannot be statistically tested, and that the criterion of falsification for hypotheses is not fulfilled.[8] In addition to the danger of merely selective or distorted epistemically contaminated reports from the analytic situation (in Grünbaum's (1988) words), we are also made aware of the problem that such research is only a one-person endeavour. One researcher generates hypotheses, gathers and selects data, aggregates and interprets them, assesses hypotheses, and thus conducts the whole research process alone! All in all, analysts in the analytic situation should be free of memory and desire, as Bion's formulation would grasp this stance. A stance that, of course, cannot apply for researchers in a modern sense, because they do not only formulate their hypotheses but have clear research aims. From the perspective of a modern research understanding one would definitely wish for the analyst/researcher to also face up to the implications of this double role and discuss it, at least critically. However, conjunction researchers, in their understanding of research do not see it as a big problem to act in a double role, namely, as the treating and thus actively involved analyst, and at the same time as reporting researcher. But it makes a big difference whether one reports traditionally, as most conjunction researchers do, to a familiar analytic community, or whether one reports to a general scientific and interdisciplinary community. As research nowadays is a socially controlled endeavour, oriented on discourse and communication, conjunction research together with the essay no longer suffice to scientifically legitimate psychoanalysis. That is the way other sciences view this, and I think we should take some of their criticisms of the methodological deficits seriously.

Some methodological cliffs concerning clinical research

Everyone doing scientific research starts from a number of assumptions about the state of the world, and some of these are implicit and hidden. If a nomothetic worldview was of primary interest, then one would tend to look for the general in all cases, possibly formulated

in terms of decontextualised universal laws or principles. If an idiographic worldview was of interest, one would rather want to look for the particular in every contextualised, unique single case, and in a second step possibly aggregate these single cases to ideal-typical patterns or schemata—thereby maintaining the permanent tension between the unique and the general. Some researchers assume that everything in the world follows causal laws, which is why they only look for causal explanations.[9] With reference to the psychic phenomena, characterised both by nature and culture, it definitely makes sense to look for *causes* as well as for *reasons*. Certainly, not everyone will follow this suggestion; Davidson (1963), for example, rejects a systematic differentiation of reasons and causes. He understands so-called primary reasons as causes, but his examples are mostly simple and everyday actions. In his view, only causes would be relevant for scientific research. I, however, would definitely find it useful to speak of a realm of reasons regarding conscious intentional actions, and particularly regarding actions triggered by unconscious wishes and fantasies and to distinguish this from a realm of causes, which may be relevant for behaviour. Two everyday examples as an illustration: a cause for crying might be that someone has a grain of sand in his eye; a reason for crying might be that someone is mourning. A cause for laughing might be that someone is being tickled; a reason for laughing might be that someone just understood a joke.. When exploring social and inner worlds one has to consider not only facts and their causal explanations, but how to rationally reconstruct the meaning of these facts for a specific person, and to understand therewith what these facts mean for this person in a specific social context. That has a significance for all phenomena of the social or inner world: it is not sufficient to subsume them under natural laws only; for the reconstruction of meaning one needs the knowledge of cultural rules and the biographical background, too. In the social world we human beings can certainly violate cultural rules intentionally or unconsciously, we can develop guilt feelings or even land in prison. Whoever tries to violate natural laws can land in hospital.

Another cliff in this context concerns the truth question. The most current criterion of truth, that of correspondence, does not work in relation to inner worlds. We do not know about correspondences between outer and inner worlds, because there is no direct access for us to look "into" the psyche. All assertions are based on inferences or

interpretations. What remains, if one does not want to do without the truth question, is to use the hermeneutic criteria of internal and external coherence, as one could do for Ogden's study. He would probably claim the pragmatic criterion for it, namely that "it works". Further, one can use the social criteria of intersubjective consensus; however, in many cases this truth criterion can only gain acceptance regionally, in specific scientific or analytic communities, on the basis of already shared worldviews and language games (about the role of language games see Wittgenstein, 1953). Empiricists, for good reasons, prefer the criterion of correspondence, as in many sciences the comparison of theory and reality functions rather well—but just not in the domain of psychic realities. In human and cultural sciences the criteria of pragmatism, consensus or coherence can be used with benefit. And, when none of the truth criteria works, then the principle of charity can help: "We make maximum sense of the words and thoughts of others when we interpret in a way that optimises agreement" (Davidson, 1973b, p. 19). Clinical research has a choice; it does not have to commit itself one-sidedly to one appropriate criterion of truth.

Besides these general epistemological and methodological issues, there are again some further basic questions, some special cliffs, so to speak; their answering influences how one proceeds further:

- What actually is understood by empirical in clinical research?
- In which language game does one speak best about psychic phenomena?
- Which and whose perspectives are to be taken into account in clinical research?

There are in the sciences different understandings of what can be understood by empirical or empirical phenomena. In psychoanalysis, the meaning of "empirical" contains not only behaviour, actions, or brain activities. For us, the psyche is seen as the origin of subjectivity and intersubjectivity, and also as part of the social world, all intra- and interpsychic phenomena being, by all means, empirical, they are not metaphysical ideas. No realist would question that there are physical facts. Searle (2010) gives reasons why "social facts", the products of subjective attitudes of human beings, are just as real and objective as physical facts. In an analogous way it would be useful for psychoanalysis to also speak of psychic facts. As a supporting argument, I would

bring in that practically all of us know what it is like to be in a psychic state, like being angry, jealous, injured, etc., or what it is like to have feelings of attachment to a friend or what it is like to love or hate someone. Instead of assuming different ontological domains, different unrelated worlds, so to speak, I would join Searle, who speaks of one single reality for physical and social facts, and I would add psychic facts, and thus also speak of the one world in which the psychic realities psychoanalysis deals with could fit in (whereby one would have to make the important methodological remark that psychic realities are not simple representations of physical and/or social reality, but always an individual lifespan reworking of these realities). However, the possible methodological access differs, depending on whether one has physical facts, social facts or—like here—psychic facts as research subject. Notwithstanding this, the psyche, especially the systematic access to the psyche, has some unruly and nasty properties, but this should be seen as a challenge rather than an obstacle.[10]

Which language games, which concepts, are appropriate for psychic phenomena? Psychoanalytic concepts have been developed to grasp these phenomena; and to grasp, in particular, the point that an analytic treatment is always a process over time. We should, therefore, not only preserve and constantly improve our language games with respect to our models of the psyche, but also our diagnostic language games with which we discuss possible indications for an analytic treatment, as well as those language games in which we describe process and possible aims of an analytic treatment. The classic formulations of analytic treatment aims, namely, making the unconscious conscious, mitigating the superego's austerity, etc., tend to be preferably used to this day within the analytic community. Practically all our traditional treatment aims (see Sandler & Dreher, 1996) are little suitable to meet those criteria of the medical systems, which today must be used in many countries in order to prove the efficacy of psychotherapy. Outside of psychoanalysis our formulations hardly play a role, and at least until now they have resisted such operationalisations, which, for instance, want to assess the aims by psychometric questionnaires at the end of an analytic treatment. From an analytic perspective neither the end of analytic treatment nor the medium questionnaire are suitable—and the patient should not be the only source of information. Beyond that, it is somewhat paradoxical if in these contexts psychoanalysis should have to describe and classify which patients it can treat and which changes it aims for by using

"un-analytic" language games. Our therapeutic competence is "psychic change", not only "cognitive or behavioural change".

When the intra- and interpsychic phenomena in and between patient and analyst become the subject of research, one has to consider three possible perspectives: the two first-person perspectives of patient and analyst have to be grasped and reconstructed; this is equally the case for the two second-person perspectives on to the respective other, and also for the third-person perspective of the treating analyst. Psychoanalysis knows a lot about how, in particular, the perspectives of the patients' self-perspective and their perspective of the analyst can be distorted, and which or how transference and defence mechanisms can be active. The self-perception of analysts and their perception of the patients should be sharpened by their own analysis and trained by countertransference analysis. The interplay, the oscillating, and the analyst's ability to change perspective between the first-, second- and third-person perspective is thereby of particular interest. This multi-functionality and multi-perspectivity characterise the role of an analyst, which is different from, for instance, the role of a therapist in behavioural therapy: the treating analyst is simultaneously "actor", "instrument", "method", and "reporter"; the person cannot be divided from the treatment and the report. In an analyst's third-person perspective the analytic process is focussed upon, in a researcher's objectifying third-person perspective the research process is focussed upon. It can be reasonable and useful that other researchers support the analyst in a kind of maieutic function[11] and help to save, to structure and process the clinical material (see e.g., Thomä & Kächele, 2006).

Clinical research—with the psyche as its subject—has to bring things together in this methodological field of tension and has to balance them out: the ideographic *and* the nomothetic view, facts *and* meanings, reasons *and* causes, explanation *and* understanding, and it cannot commit itself to only one criterion of truth. Appropriate solutions have to be found for the empirical subject area, as well as for the following questions: With which concepts can one best describe the phenomena? Which perspectives have to be taken into account, investigated, and reconstructed? The reason that these fields of tension can at times become a hornets' nest is because methodological positions, sometimes, are not dealt with in discourse but are defended as dogmas—a remarkable attitude given the ever-changing research paradigms and scientific worldviews.

Some practical research issues

There are some further practical research questions to consider when planning concrete clinical research projects:

- What status do *single case studies* have?
- Are only *quantitative* studies useful or also *qualitative* ones?
- Does *randomisation* and the use of *control groups* function in psychoanalytic studies at all?
- Is data collection at *fixed points* in time possible?
- Can the results of analytic studies be *replicated*?

Single case studies: In the past, conjunction research was deemed the predominant paradigm—a mode for single-case research, which, by all means, offers room for improvement. But not only single cases are relevant for research; what is also needed for purposes of generalisation are findings based on larger groups. Currently the discussion about methodological access is dominated by research paradigms in the context of evidence- based medicine (EBM), and in some countries pressure is put on analytic researchers to adopt their respective standards not only of applied but also of fundamental research. This means that an investigation that wants to prove the efficacy of psychoanalytically orientated treatments should apply research designs, as they have been developed for medical treatments like drugs or surgical procedures. The methodological rules are based on the logic of experimental designs from the natural sciences. In EBM, randomised controlled trials (RCT) are the so-called gold standard. Studies of this type receive the highest level of evidence; studies of the Ogden type are awarded the lowest level of evidence, the tin standard, so to speak. In all sciences, single case studies are always subject to a restriction: they can only show that a phenomenon exists. When analysts report, for instance, from the treatment of a borderline patient, they can demonstrate what is possible in such treatments. However, it is considered inadmissible to make generalising statements about "all" borderline patients on the basis of single cases alone. Statisticians attempt to solve the problem of generalisation from a few to all cases by using parameters from homogeneous samples in order to estimate what the situation in a defined population may be. Indeed: only on the basis of behavioural indicators from the ICD or the DSM may a population of borderline patients be successfully defined

and samples be drawn. In psychoanalysis' understanding, however, a so-called borderline patient is defined, above all, by internal psychic indicators, like quality of structure and affects, prevalent defence mechanisms, type of conflict, or specific countertransference reactions. Whether it is reasonable and possible at all to speak of a "population or sample of borderline patients" in this analytic, diagnostic understanding, as is required for RCTs, is an open question.

What actually are those single cases that can be investigated on their own, or grouped into larger units like, for example, random samples? For a statistician the matter is clear: a single case is n = 1—that is to say, a single patient, thought of as a monad whose individual data disappear in the means and variances of samples. In psychoanalysis, where the clinical single case is a dyad, large varieties of variants exist. During its long history, psychoanalysis has amassed a huge treasure of such clinical single case studies and, with it, clinical knowledge, which until now has not been sufficiently systematically aggregated—a major problem in modern research contexts. Unfortunately, the various and changing language games of different analytic cultures make such aggregations difficult. This is definitely a strategic challenge. There must be better ways to document single cases and to stratify them into larger units, like classes, types, or ideal types. It certainly would make sense to aggregate not by behavioural criteria, but by those criteria that are oriented on analytic concepts, as, for instance, the operationalised psychodynamic diagnostic (OPD) is attempting to do (see OPD task force, 2008). The results of efficacy studies could work better and much more in favour of psychoanalysis if the patient collectives under study were able to be described with greater validity and classified better psychodynamically.[12]

The systematic investigation of single cases is also, by the way, not an end-of-range model in the sciences. EBM, like psychoanalysis, is equally interested in the care of individual patients, and one of the founders of EBM states that the aim "is the conscientious, explicit and judicious use of current best evidence in making decisions about the care of individual patients" (Sackett, 1996, p. 71). It is of interest that, today, also, medical science, through so-called personalised medicine, is reverting back to an individual perspective.[13] From a genetic and epigenetic perspective each person is unique, and this is not only because of differing DNA or fingerprints, but also with respect to the effectiveness of medication. There are already tailor-made remedies to target the cancer of

individual patients; in a certain way psychoanalytic treatment could be seen as tailor-made, too.

Quantitative and/or qualitative: The research material from analytic treatments is largely and predominantly words, that is to say, verbal data, be they written-down utterances of patient and analyst, or descriptions or thoughts. The data that are needed for RCT studies must be numbers, as a rule, for only on this basis statistical hypotheses can be tested. Variables are being operationalised and measured objectively and reliably[14] with appropriate tools, often questionnaires—a further procedure considered the gold standard of empirical research practice. The conviction prevails in many sciences that numbers are a priori exact, while words are deemed opaque, and thus bad news for a talking cure. Quantitative studies are therefore generally regarded as better than qualitative studies thattake into account subjectivity and meaning. In empiristic contexts there is even, often, the tendency to connect the label "scientific" only with quantitative-experimental methods, in which statistical hypotheses could be tested. Thus not infrequently the choice of "the right" methods takes on the function of Occam's razor for the supposedly clear separation of science from non-science. But luckily this conviction is not shared by all scientists; there are, in clinical research, a great number of qualitative methods being used, like diverse interview techniques, which can be evaluated with gain not only quantitatively, but also interpretatively (Leuzinger-Bohleber et al., 2013). The methods must fit the research subject and the data that the subject presents, and the subject should not be adapted to fit the methods.[15] Interpretative methods are beneficial, for example, in infant research where Stern (1986) demonstrates the usefulness of interpretative, theory-guided "inferential leaps" in order to infer from behavioural observation data the state of the psyche. Or, it is demonstrated by the ethnopsychoanalysts, who use the systematic analysis of countertransference in order to understand the "other"; the functioning of a foreign culture. Isn't it ironic that, especially with regard to the acceptance of interpretative methods, paradoxical things can sometimes be observed: analysts who matter of factly use interpretation in their clinical practice, and may, in a research context, reject qualitative methods with interpretative evaluation as unscientific, but then go about interpreting the results of their factor analyses or statistical evaluations both statistically and psychoanalytically.

Randomisation/control groups: To prevent biases, RCT studies require random allocation of cases to treatment groups.[16] A random allocation, however, may be irritating for the individual process of matching patient and analyst: both must find out whether they can imagine working with each other. This is an important initial condition of each analytic treatment. In addition, RCT studies require control groups that get "no treatment". In psychoanalysis we know of therapies of, for example, borderline patients, which extend in several stages over many years—often more than ten years. Psychic change sometimes just needs a long time. Would it be admissible in such cases, for purposes of comparability, to leave a control group untreated for such a long time?

Fixed points in time for data collection: Beyond that, the designs of RCT studies often orient themselves according to the schedules of pharmaceutical research, with fixed points in time for data collection—that is, "before" and "after" the treatment. We know, though, that in psychoanalysis the diagnostic process can take a long time, often even as long as the analysis itself, and the therapeutic success in the lives of patients very often does not happen with the end of the treatment—the treatment goals must sometimes be laboriously transformed into life goals first, and it can take time until psychic change can also show in life. Not without reason did Freud speak of analysis terminable and interminable.

Replicability: A research presentation should be comprehensible and traceable. Traceability refers to the clarity and consistency of the presentation, to the plausibility of the arguments, and to the degree of how conclusively results and consequences are being inferred and justified. Traceability is a necessary precondition of replicability. Already in psychology and the social sciences the replicability of results of studies is often limited because historical, economic, cultural and social constraints play a role; they change all the time and can never be reconstructed just as they were. In clinical research there can also be no replicability, because each dyad of analyst and patient is singular, and in addition all psychic phenomena are singular, too, among other things because they are constantly overworked through deferred action. Thus the criterion of replicability should be mitigated to achieve traceability. To be able to trace an argumentation does not simply mean agreement; dealing with criticism in a non-discriminating way should be possible.

Every assertion, every hypothesis, must be able to be refuted by experience as well as by logical arguments, as Popper (1934) remarked. And

the method of authority to support a statement in sciences is also only of limited use, as Peirce has shown (Peirce, 1931). Also, Freud repeatedly reminded us throughout his writings that all our knowledge is preliminary:

> Psycho-analysis is not, like philosophies, a system starting out from a few sharply defined basic concepts, seeking to grasp the whole universe with the help of these and, once it is completed, having no room for fresh discoveries or better understanding. On the contrary, it keeps close to the facts in its field of study, seeks to solve the immediate problems of observation, gropes its way forward by the help of experience, is always incomplete and always ready to correct or modify its theories. There is no incongruity (any more than in the case of physics or chemistry) if its most general concepts lack clarity and if its postulates are provisional; it leaves their more precise definition to the results of future work. (Freud, 1923a, p. 253f)

To conclude …

The suggestion to look at clinical research from the subject of the psyche—that traditional entity that subsumes the manifold intra- and interpsychic phenomena under a single concept—is rooted in the psychoanalytic worldview and its image of man. The respective phenomena are not only difficult to grasp clinically, they are also difficult to work on methodologically. Conjunction research does not satisfy the requirements of modern research in many aspects, especially concerning systematisation and control. While systematisation and control are certainly the strengths of the analytic research, which is orientated on the maxims of EBM, this type of research often does not do justice to the complexity of the subject matter.

I have attempted to describe the methodological terrain and to structure it from an explicitly analytic point of view, although I am aware of the great pressure from the medical system in some parts of the world to adopt an explicitly empiristic point of view. I have discussed some difficult methodological cliffs, which, unfortunately, cannot be ignored. Certainly, one can repress or deny them, but, as we know, they do not disappear. If one is to do psychoanalytic clinical research successfully, one has to negotiate the two critically—that is, one has to sharpen again and again one's own methodological position in order to do justice to

the subject matter between nature and culture. What results from these considerations is a broad understanding of research and research methodology in the human sciences. Why not bring this view more into play again?

Psychoanalytic research might even benefit from the use of different scientific belief systems and methodologies, because this would enable it to critically weigh and select the best of several scientific worlds. Our clinical research should above all remain varied and creative in terms of methodic access. And there are a lot of scientific methods that are suitable to investigate meanings, to reconstruct different perspectives, and to light up inner worlds interpretively. To put it concisely: interpretation and rational reconstruction should keep their traditional place in the analytic research landscape. Of course, one can also measure and experiment if it fits. The choice of appropriate methods should take into account the validity and the generalisability of the results, so that the transfer of research results into the analytic practice is then fruitful. Clinical research is actually not an end in itself, but should serve the improvement of the clinical practice and thus the better treatment of patients, and should contribute to further understanding of the human psyche.

And last but not least the psyche is a challenging candidate, also epistemologically, perhaps even the most difficult one known by human sciences. Besides the problems of access there is also the problem of self-reference: whoever explores the psyche of others is always exploring him- or herself, be that as an analyst or as a researcher. General scientific statements about the "psyche" or "behaviour" or "brain" also hold for the researcher's psyche, behaviour, or brain. So what is left in answer to the question posed in the title? In the same way as the core competency of psychoanalysis is the analysis of this psyche, there can be no psychoanalytic research without the psyche, if psychoanalysis wants to keep its identity.

CHAPTER TEN

Repression, defence, and the psychology of science*

Simon Boag

During the Battle of Copenhagen in 1801, Admiral Horatio Nelson, blind in one eye, is said to have been given a signal by Sir Hyde Parker to withdraw in the face of overwhelming Danish forces. In turn, Nelson is said to have ignored the signal, claiming: "I only have one eye; I have a right to be blind sometimes; I really do not see the signal" (in Gardiner, 1997, p. 181). This incident is said to have given rise to the saying "turning a blind eye" (or knowingly ignoring unpleasant facts). A teacher, for instance, might pretend not to notice a student cheating during an exam, or a parent might feign ignorance of a child's drug usage. In such cases, and as in Nelson's example above, the person turning a blind eye is, in fact, cognisant of the situation being ignored. Nevertheless, the individual would prefer not to know and minimises facing the disconcerting situation.

Freud similarly refers to a "blindness of the seeing eye", but, unlike the cases above, such blindness involves an apparent paradox with

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respect to both knowing and not knowing some fact simultaneously. The first reference to this is found in the *Studies in Hysteria* (Breuer & Freud, 1895d) in the case of Miss Lucy R, who had come to Freud suffering from a variety of complications including olfactory hallucinations (a smell of burnt pudding). Freud's analysis traced the symptoms back to an event where "opposing affects had been in conflict with each other" and the smell had been contemporaneous (Freud in Breuer & Freud, 1895d, p. 115). Miss Lucy R had decided to leave the children she had been caring for as a nanny and return to her mother, but she also loved her employer and desired to become a replacement mother of the children. This desire had been repressed and the smell subsequently became a "symbol" of the event.

In discussing this repression, Freud notes that Miss Lucy R could recall the initial "act of will" in which the love for her employer was repressed. For a time she had been aware of these desires but after realising that these were unfulfillable "she decided to banish the whole business from her mind" (Freud in Breuer & Freud, 1895d, p. 118). Miss Lucy R. subsequently agrees with Freud's interpretation of the repressed desire, leading Freud to ask: "But if you knew you loved your employer why didn't you tell me?", and she responds: "I didn't know—or rather, I didn't want to know. I wanted to drive it out of my head and not think of it again; and I believe latterly I have succeeded" (in Breuer & Freud, 1895d, p. 117).

The discussion above has led some authors to conclude that repression may occur consciously (e.g., Erdelyi, 1990, 2006; Macmillan, 1991), in contradistinction to the commonly held position that repression is necessarily considered an unconscious process (e.g., A. Freud, 1968). However, Freud himself indicates another possible interpretation with respect to Miss Lucy R's apparent knowledge and ignorance of the same event. Commenting on the apparent paradox, Freud writes: "I have never managed to give a better description than this of the strange state of mind in which one knows and does not know a thing at the same time. It is clearly impossible to understand it unless one has been in such a state oneself" (Freud in Breuer & Freud, 1895d, p. 117*n*). He continues:

> I myself have had a very remarkable experience of this sort, which is still clearly before me. If I try to recollect what went on in my mind at that time I can get hold of very little. What happened was that I saw something which did not fit in at all with my expectation; yet I did not allow what I saw to disturb my fixed plan in

> the least, though the perception should have put a stop to it, I was unconscious of any contradiction in this; nor was I aware of my feelings of repulsion, which must nevertheless undoubtedly have been responsible for the perception producing no psychical effect. I was afflicted by that blindness of the seeing eye which is so astonishing in the attitude of mothers to their daughters, husbands to their wives and rulers to their favourites. (Freud in Breuer & Freud, 1895d, p. 117*n*)

While Freud does not elaborate further, what can be surmised is that some fact repulsed Freud so much that his *attention could not attend to it*. Nevertheless, the fact must have been *known*—for he was repulsed by it—even if this occurred unknowingly. In other words, Freud unconsciously knew some fact without knowing it: his unconscious repulsion to some fact prevented recognising that same state of affairs.

The scientific standing of repression

The examples of turning a blind eye and Freud's "blindness of the seeing eye" point to the possibility of varieties of ignoring instigated by a common motivation to avoid facing unpleasant circumstances. However, while no one would dispute the possibility of turning a blind eye to certain unpleasant situations, the possibilities of repression and the "blindness of the seeing eye" are much more contentious. For instance, the association between repression and the dynamic unconscious is judged to be problematic (e.g., Grünbaum, this volume), and while turning a blind eye is purported to occur "consciously", the very possibility of unconscious mental processes still receives sceptical treatment even today (see Talvitie, 2009, 2012). Furthermore, both the evidential standing of repression (e.g., Erwin, 1996, this volume, Chapter Two; Grünbaum, 1983) and the logic of repression theory (e.g., Macmillan, 1991)[1] are questioned. The case for repression is not made simpler by the apparent paradox entailed in simultaneously knowing and not knowing the same event, which, at first glance, makes repression appear inconceivable (see Maze & Henry, 1996). It is little wonder, then, that even those sympathetic to the theory of repression are uncertain of its scientific value. For instance, Nesse (1990) writes that although clinically important, "[repression] remains an anomalous and awkward concept that has kept psychoanalysis apart from the rest of science" (p. 262).

These question marks over repression are all the more striking in light of Freud's declaration that the "theory of repression is the

corner-stone on which the whole structure of psycho-analysis rests" (Freud, 1914d, p. 16) and that it "is possible to take repression as a centre and bring all the elements of psycho-analytic theory into relation with it" (Freud, 1925c, p. 30). The centrality of repression is related to the causal role attributed to repression with respect to the psychoneuroses. Grünbaum (1983) here writes that "[the] repression-aetiology of psychoneuroses … is *the* major pillar of the Freudian structure" (p. 149, his italics) and should repression then be found to be indefensible, then psychoanalysis as an explanatory framework for the psychoneuroses also fails.

However, while the apparent question marks over repression may suggest that there is very little scientific merit in pursuing the topic, there is no logical problem with Freud's theory of repression and the "blindness of the seeing eye". The issue is resolved simply by postulating that repression *prevents knowing (or acknowledging) that the repressed is known* (Boag, 2007b, 2012). An anxiety-provoking state of affairs might instigate a repressive response, preventing reflection on the fact that the distressing event is known. Allowing for the repressed to be known (but in such a way that reflection on this act is prevented) further permits explaining resistance, objectless anxiety, and "after-pressure" (secondary repression), all of which involve anxiety with respect to knowing the repressed (but not knowing that the repressed is known)[2] (see Boag, 2012, Chapter Eleven). Furthermore, there is no logical difficulty with unconscious processes (see Boag, 2008b, 2012) and there is no shortage of evidence for unconscious knowing (see Brakel, 2009).

Additionally, far from repression keeping psychoanalysis out of science, this chapter instead proposes that repression—as motivated ignoring—is one of Freud's most important contributions to the psychology of science for the simple reason that it highlights the possibility of motivated blind spots in scientific enquiry. To demonstrate this, this paper elaborates, with the help of Erdelyi (1990, 2006), Freud's account of a series of defensive activities, where repression can be seen within the context of a variety of everyday processes involving motivated ignoring. Following on from this, Freud's contribution to the psychology of science is discussed in the context of the relationship between repression and defence and the issue of resistance to scientific progress. The motivational basis of repressive processes is discussed in terms of mental reactions instigated by pain avoidance, leading to motivated ignoring that may give rise to both self-deception and blind spots within

any field of enquiry. Here a psychodynamic account of human nature is important because it addresses the systematic connection between motivation, affects, and cognition, as well as indicating how these factors may combine to contribute to motivated ignoring in scientific research. An example of where varieties of motivated ignoring may be contributing to error in psychological science is then developed in the context of psychometrics and "pathological science" (Michell, 2000, 2008).

The concept of repression and defence

The concept of repression itself appears fairly straightforward. Freud writes that "*the essence of repression lies simply in turning something away, and keeping it at a distance, from the conscious*" (Freud, 1915e, p. 147, his italics). Here repression is simply a form of motivated ignoring (selective inattention), and repression initially takes on theoretical significance within psychoanalytic theory due to its explanatory value. A consistent theme in Freud's account is that repression does not destroy the repressed. Instead, the repressed remains causally active and persists in the direction of conscious thinking (Freud, 1900a, p. 577; 1915e, p. 166; 1919g, p. 260; 1933a, p. 68), and the symptoms of the psychoneuroses (and other phenomena such as dreams) are explained in terms of repressed wishes acquiring substitutive outlets (e.g., Freud, 1926f, p. 267).

However, the apparent simplicity of repression as motivated ignoring betrays a hidden complexity with respect to understanding the relation between repression and defence. Madison (1961) writes that the "most difficult and persistent problem in the theory of repression is the relation between repression and defence" (p. 15). As is generally well known, Freud appears to initially use the terms "repression" and "defence" synonymously; for example, writing that "sexuality seems to play a principal part in the pathogenesis of hysteria as a source of psychical traumas and as a motive for 'defence'—that is, for repressing ideas from consciousness" (Breuer & Freud, 1895d, p. xxix). A decade later, however, Freud almost exclusively uses the term "repression": "… accidental influences receded still further into the background as compared with 'repression' (as I now began to say instead of 'defence')" (Freud, 1906a, p. 276). Later still, in 1926, "repression" becomes a distinct mechanism of "defence": "[Defence] can cover all these processes that have the same purpose—namely, the protection of the ego against

instinctual demands—and for subsuming repression under it as a special case" (Freud, 1926d, p. 164). This latter, special case of repression is characterised by hysterical amnesia standing in contrast to other defences such as reaction-formation, isolation, undoing, and projection (Freud, 1926d, p. 163).

The view that repression is a special instance of defence has become the prevailing view (e.g., Arlow & Brenner, 1964; A. Freud, 1968; Nesse, 1990; Willick, 1995), and Sjöbäck (1973) claims that given this revision, rather than "repression", Freud "would have called the *theory of defensive processes* the cornerstone on which the whole structure of psychoanalysis rests" (p. 1, his italics; cf. Willick, 1995, p. 485). However, even after this attempted clarification Freud himself remains inconsistent. As Madison (1961) notes, Freud "was never able to maintain the distinction he proposed in 1926, in which 'repression' was subordinated to 'defence'—not even within the very volume in which he announced it" (p. 16). For example, although the term "repression" should apply only to hysterical forgetting, Freud speaks of the obsessional neurotic's attempt to "try to repress … by motor means" (Freud, 1926d, p. 120). Similarly Freud speaks of "repression by means of *reaction-formation* (in the ego)" (1926d, p. 102, his italics). Moreover, it appears that Freud continued to use repression and defence synonymously (e.g., "repression (or defence)"—Freud, 1926d, p. 123). The legacy from this is that terms such as "repression" and "defence" are used idiosyncratically within the literature. For example, B. P. Jones (1993) postulates five varieties of "repression", only one corresponding to Freud's narrow definition of hysterical amnesia.

While the different meanings for repression and defence invite confusion, there is nonetheless a common element with respect to motivated ignoring. Madison (1956), for instance, writes that all defences include motivated ignoring and that which distinguishes the various defences is the *target* that is denied awareness: memories are denied in hysteria, logical connections in obsessional neurosis, and ownership of thoughts and impulses in projection (Madison, 1956, 1961; cf. Freud, 1894a). Hence, argues Madison: "Repression (in the sense of unawareness) is not *one* of a number of defences, it is the essence of all defences" (Madison, 1956, p. 78, his italics). So conceptualised, repression can be seen as the basis of all defences (Gillett, 1988; Kinston & Cohen, 1988).

However, while motivated ignoring may be involved in all defence, there are also variations in the *degree* of ignorance involved in any defensive activity. Furthermore, such variations of unconsciousness

also appear to impact upon whether certain processes are considered plausible or not, a point demonstrable by comparing repression with ordinary "suppression". Both repression and suppression involve motivated ignoring, but while repression remains controversial, suppression is universally accepted (see Boag, 2010). Erdelyi (1990, 2006) believes that the difficulties associated with accepting repression follow from the belief that repression is necessarily unconscious. Erdelyi notes, however, that if repression can occur consciously then there is no dispute that repression exists in terms of ordinary selective attention: "If repression = suppression, then everybody believes in repression. The myth morphs into the obvious" (Erdelyi, 2006, p. 500).

Evaluating Erdelyi's claim depends, in part, upon what is precisely meant by "consciousness" (see Boag, 2008b, 2010). However, taken literally, the claim that repression *equals* suppression could be seen as an oversimplification, since even if repression involves consciousness (however contrived), there may be other features distinguishing repression from suppression. For instance, while turning a blind eye and blindness of the seeing eye may have a common element in terms of motivated ignoring, they appear nevertheless to be distinct phenomena, since turning a blind eye would not appear to have the same psychoneurotic consequences of Freudian repression "proper". This notwithstanding, the spirit of Erdelyi's suggestion here is the important matter. If repression is looked at broadly in terms of selective inattention (i.e., motivated ignoring), then there is no disputing that varieties of processes involving motivated ignoring exist. Turning a blind eye and blindness of the seeing eye, then, simply reflect varieties of motivated ignoring.

One objection raised by Erwin (1996) is that if Erdelyi is correct here then there are many such cases that could be described as "repression" (e.g., thinking about something pleasant to avoid an unpleasant situation), but that such instances are not distinctly "Freudian" (pp. 221–222). However, what Erdelyi is drawing attention to is simply the possibility of a series of activities involving motivated ignoring, as posited by Freud himself. Freud (1905c), for instance, writes that "[r]epression may, without doubt, be correctly described as the intermediate stage between a defensive reflex and a condemning judgement" (p. 175). Similarly:

> Between repression and what may be termed the normal method of fending off what is distressing or unbearable, by means of recognising it, considering it, making a judgement upon it and taking

> appropriate action about it, there lie a whole series of more or less clearly pathological methods of behaviour on the part of the ego. (Freud, 1936a, pp. 245–246)

Thus we can follow Freud's lead and posit a series of repressive activities (all of which involve motivated ignoring) some of which can be postulated as causally significant in the generation of psychoneurotic symptoms, and others not. While some may still dispute "strong" cases of repression (e.g., blindness of the seeing eye), clearly other instances of motivated ignoring are uncontroversial (e.g., turning a blind eye). Consequently, varieties of motivated ignoring exist, and determining which instances of repressive defences occur is a perfectly legitimate topic of scientific enquiry.

Resistances to scientific progress

The significance of postulating a series of repressive activities is seen in Freud's discussion in The resistances to psycho-analysis (1925e) where he addresses a variety of factors that might impact upon the scientific assessment of theories. There Freud discusses the role of unpleasure and how it contributes to scientific evaluation: unpleasure underlies (but is not limited to) a range of reactions, including a general fear of novelty, "blows to narcissism", racial prejudice, and specific objections to the subject matter of psychoanalysis (e.g., infantile sexuality). According to Freud, all of these primitive, defensive reactions have the motive of unpleasure in common, and Freud believes that "the strongest resistances to psycho-analysis were not of an intellectual kind but arose from emotional sources" (p. 221). The significance of Freud's account here is that considerations of motivated resistance are relevant to all areas of scientific enquiry, since such resistance may subvert the scientific process. In other words, since we know that motivated ignoring occurs, it follows that it is necessary to attend to the possibility of such "resistance" in scientific enquiry.

However, it is this very issue of using repression as an *explanation* of resistances to Freudian theory that raises a variety of problems. Claiming that criticisms of one's theory are due to emotional resistances can, of course, be construed as a very convenient means of denouncing any critics of a theory and inoculating the theory against criticism. Here Grünbaum (1980) draws attention to what he refers to as "logically

circular *self*-validation" with respect to using repression to dismiss all scepticism of the theory of repression (p. 78): "it is illicit simply to *assume* the theory whose truth is first at issue, and then to invoke this very theory as a basis for a psychologistic dismissal of *evidential* criticism of its validity" (Grünbaum, 1980, p. 77, his italics; cf. Eysenck, 1985, p. 185). Furthermore, the history of psychoanalysis demonstrates that the early proponents of psychoanalysis—including Freud himself—appeared to all too easily dismiss one another's theories in terms of such emotional reactions (e.g., father and brother complexes—see Lieberman & Kramer, 2012). Freud (1925e), in fact, even notes that a certain degree of scepticism benefits the scientist: carefully and cautiously evaluating claims is necessary for avoiding the uncritical acceptance of assertions that may be false (p. 213).

Nevertheless, even critics of repression theory do not appear to dispute that there may be motivated resistances to accepting theories, and Grünbaum (1980) himself notes that scientists may have emotional biases that may motivate one to either confirm or deny a theory. Grünbaum adds, however, that despite such biases, any researcher may nevertheless come across contradictory evidence and become convinced of the opposing view. However, of course, while this *may* occur it also may not, and so there is no guarantee that any motivationally biased researcher will accept disconfirming evidence should it be found. The important point, then, is that the reality of motivated ignoring and resistance requires clear consideration in any psychology of science.

Science and human nature

As already noted, the possibility of motivated ignoring could be expected to be a major concern for any scientist, since it could interfere with the aim of discovering and accruing objective knowledge. Not only are our perceptual systems fallible, but errors and biases may also possibly arise when faced with "unpleasant facts". Humans, for instance, experience disgust towards all manner of things, including moral issues (Chapman, Kim, Susskind, & Anderson, 2009), and disgust is but one factor that may potentially interfere with the objective assessment of any disgust-provoking stimulus. Given, then, that we know that emotional and motivational factors may, to varying degrees, bias our conclusions, the activity of successful scientific enquiry involves acknowledging facets of human nature that are potentially antithetical

to the aims of objectivity. Freud's own theory of human nature provides a useful platform here for understanding the psychology of science insofar as it emphasises the fundamental role of motivation, affects, and their relation to enquiry (e.g., Freud, 1900a, 1915c). For Freud (1915c), all mental activity is motivated in the interests of the drives: it is the primary drives that are interested in states of affairs relevant to their gratification (see Maze, 1983) and our interest in events is, then, never dispassionate enquiry but always motivated in relationship to "needs", even when it appears selfless or self-defeating. Thus, in contradistinction to the "rational ego" that is sometimes found in Freud's thinking (e.g., Freud, 1923b, p. 25), we can instead see the ego as an extension of the drives, and so any appearance of passionless rationality is at best a reflection of repression itself (see Maze, 1983).

While all behaviour and cognition is motivated, unpleasure occupies a primary role in instigating defensive activities. Freud writes that "psychical activity draws back from any event which might arouse unpleasure" (Freud, 1911b, p. 219) and repression is comparable to a "flight-reflex in the presence of painful stimuli" (Freud, 1901b, p. 147). Such responses are premised upon Freud's postulated, general motivating principle forming the basis of both normal and pathological behaviour: "The nervous system has the most decided inclination to a *flight from pain*" (Freud, 1950, p. 307, his italics), and in a draft sent to his early confidant Wilhelm Fliess (Draft K), Freud writes: "there is a normal trend towards defence—that is, an aversion to directing psychic energy in such a way that unpleasure results" (Freud in Masson, 1985, p. 163). This general trend forms the prototype of repression in terms of the psychical equivalent of withdrawing from painful stimuli: "This effortless and regular avoidance by the psychical process of the memory of anything that had once been distressing affords us the prototype and first example of *psychical repression*" (Freud, 1900a, p. 600, his italics). The prototype of repression here operates in a similar fashion to motor movements away from painful stimuli (Boag, 2007a).

Grünbaum (this volume), however, questions whether "motives of unpleasure" can serve as the basis of repression, claiming that the finding that people do remember painful experiences contradicts Freud's theory of unpleasure instigating repression. The issue that Grünbaum is trying to draw attention to here is that Freud's claim of the primacy of unpleasure as a motivating principle for repression

must be incorrect, because if this were the case then *all* (or at least nearly all) unpleasurable mental states would succumb to repression. Grünbaum writes:

> Apparently, Freud assumes *axiomatically* that distressing mental states, such as forbidden wishes, trauma, disgust, anxiety, anger, shame, hate, guilt, and sadness—all of which are *unpleasurable*—almost always actuate, and then fuel, *forgetting* to the point of repression. Thus, repression regulates pleasure and unpleasure by defending our consciousness against various sorts of *negative affect*. (Grünbaum, this volume, page 13, his italics)

While acknowledging that Freud does discuss cases of painful events being remembered, Grünbaum believes that Freud never provides a satisfying account of this and concludes that Freud's lack of attention to the issue of unpleasurable memories being remembered is a major failing of his theory of repression:

> … Freud's attempt … to uphold his thesis of motivated forgetting is evasive and unavailing. Since some painful mental states are vividly remembered while others are forgotten or even repressed, I claim that *factors different from their painfulness determine whether they are remembered or forgotten*. For example, personality dispositions or situational variables may in fact be casually relevant. To the great detriment of his theory, Freud never came to grips with the *unfavourable* bearing of this key fact about the mnemic effects of painfulness on the tenability of the following pillar of his theory of repression: When painful or forbidden experiences are forgotten, the forgetting is tantamount to their repression *due to their negative affect*, and thereby produces neurotic symptoms or other compromise-formations. (Grünbaum, this volume, page 14, his italics)

It should be noted here that Grünbaum is not attacking the possibility that unpleasure may motivate repression, but, rather, he takes issue with the causal efficacy of unpleasure as a sufficient condition for repression. However, Freud never stated that *all* painful experiences instigate "repression" per se, even if the primitive organism is inclined to avoid any form of unpleasure. Here Grünbaum ignores the relatively obvious *developmental* factor proposed by Freud, whereby all later (strong

cases of) repressions require a primal repression during a critical period (e.g., Freud, 1911c, 1915d; cf. Boag, 2012, pp. 30–33):

> All repressions take place in early childhood; they are primitive defensive measures taken by the immature, feeble ego. In later years no fresh repressions are carried out; but the old ones persist, and their services continue to be made use of by the ego for mastering the instincts. New conflicts are disposed of by what we call 'after-repression.' (Freud, 1937c, p. 227)

That is, for Freud, one would not expect amnesia for any traumatic event unless there is a link to a primally repressed content. Freud further proposes a central role for psychological conflict (entailing fear and anxiety at the prospect of acting upon one's own desires) as an essential condition for repression (see Freud, 1919g, p. 209), and, as noted earlier, Freud makes a distinction between repression and "normal fending off" of unpleasurable stimuli (Freud, 1936a, pp. 245–246).[3]

Additionally, as Grünbaum alludes to, Freud was fully aware of the apparent contradiction of behaviours that aimed to increase rather than avoid unpleasure (the so-called "economic" problem of masochism—Freud, 1924c). That some individuals nevertheless perform certain activities that appear "masochistic" may appear to present a theoretical challenge, but there is no logical problem with the proposal that pain itself might come to be rewarding, or that we may learn to forgo immediate gratification and endure states of unpleasure in the *belief* that we may possibly acquire other sources of gratification or avoid even greater suffering. For instance, an athlete might believe in the motto "no pain, no gain", whereby pain signals a desirable event, or a triathlete's "masochistic" painful endurance of running, swimming and cycling may be comprehensible in the context of a perceived greater goal. Accordingly, the psychoanalytic thinker will suspect in any such cases where a person performs apparently masochistic activities that there is precisely such "rewards", which may or may not be consciously known to the individual involved. Consequently, Grünbaum's criticisms of the motives of unpleasure and repression come to nothing.

However, Grünbaum's criticism does indicate that the pleasure and reality principles require clarification. For Freud, the infant's helplessness and inability to tolerate frustration in the face of an unsatisfied need creates a primary process hallucinatory reinvestment of previously learnt experiences of satisfaction (Freud, 1900a, p. 598). While

this temporarily silences the need, a *real* object or event is required for any actual gratification. The apparatus subsequently learns to inhibit reinvesting the mnemic idea to the point of hallucinatory perception. Consequently, the infant learns to discern veridical from non-veridical experience through awareness of "indications of reality", which determine whether the wished for object and situation is in fact real or not: "A new principle of mental functioning was thus introduced; what was presented in the mind was no longer what was agreeable but what was real, even if it happened to be disagreeable" (Freud, 1911b, p. 219). This introduces the "reality" principle, a reality-tempered modification of the primitive pleasure principle, where *actual* conditions of satisfaction and frustration are taken into account before initiating action:

> Under the influence of the ego's instincts of self-preservation, the pleasure principle is replaced by the *reality principle*. This latter principle does not abandon the intention of ultimately obtaining pleasure, but it nevertheless demands and carries into effect the postponement of satisfaction, the abandonment of a number of possibilities of gaining satisfaction and the temporary toleration of unpleasure as a step on the long indirect road to pleasure. (Freud, 1920g, p. 10, his italics; cf. 1900a, p. 601; 1915d, p. 120; 1925i, p. 127)

That is, the reality principle is simply an elaboration of the pleasure principle, and represents an enforced detour along the path to gratification. Nevertheless, what this all indicates is that attention to what is the case may easily be subverted, since pleasure and unpleasure remain as primary motivating stimuli.

Pathological science, self-interest, and socially constructed blindness

An example of where motivated ignoring may be subverting claims to objectivity is in the field of psychometrics. Psychometrics is the purported measurement of psychological attributes, such as mental abilities and personality traits. While scientific psychology generally considers measurement to be a sine qua non of science and objectivity, Joel Michell proposes that psychometrics provides an example of what he calls "pathological science" (Michell, 2000, 2008). Michell (2000) defines pathological science in terms of a two-level breakdown in the

processes of critical inquiry, analogous with pathology of individual cognition, whereby a person is not only in error but also prevented from recognising being in error. For example, given the relative scope of our sensory apparatus in relation to the universe, it appears that the sun revolves around the earth, giving rise to the prima facie plausible, yet mistaken, geocentric world view. However, as we know, such a view is erroneous, and through demonstration and argument the view can generally be corrected. In such instances, says Michell, error itself is not pathological, since it can be corrected when the relevant evidence is available. A pathological condition, however, follows if the correction of error is prevented by some relatively permanent condition "that not only interferes with the cognition of the facts of a certain class, but also hinders correction of these errors" (Michell, 2000, p. 640). For instance, politico-religious factors might prevent the relevant evidence from emerging, thus preventing the error from being recognised.

In the case of psychometrics, Michell (2000, 2008) argues that there is an uncritical acceptance of the belief that psychological attributes are quantitative. For any attribute to be quantitative (and hence possibly measureable) requires that it possess both ordinal and additive structure (see Michell, 1999, Chapter Three). Addressing whether any attribute possesses additive structure requires both careful conceptualisation and empirical tests before asserting that such attributes are quantitative. In psychometrics, however, this has clearly not occurred: there is simply the presumption that the psychological attributes of interest are quantifiable. However, while the presumption that attributes are quantitative is not, in itself, pathological (for instance, the belief may form a provisional hypothesis that proponents nevertheless recognise requires greater scrutiny), there are reasons to believe that the detection of error is being prevented: the vast majority of psychological researchers disregard the possibility that attributes are not in fact measureable and the possibility of error "is ignored or even disguised" (Michell, 2008, p. 12; cf. Michell, 2004b, p. 121).

In earlier writings Michell (1997) described this problem as one of psychometricians possessing "methodological thought disorder": psychometricians displayed a sustained failure to observe the significance of the issue of quantity in relation to measurement. However, Michell believes that social factors are primarily responsible for this situation, guiding a certain course of enquiry and inhibiting others, and giving rise to what he describes as a "socially constructed blindness".[4]

Michell (1997, 2008) cites social interests and systemic support for this pathology via a variety of factors, including textbooks endorsing an operational definition of measurement (a definition that ignores the issue of quantity), current teaching of psychometrics, which uncritically assumes that attributes are measureable, as well as a lack of career paths for anyone straying from the dominant research paradigm. Furthermore, various pressures—including limited funding resources and an apparent desire to be seen as a "real science" for legitimisation—make "measurement" a clearly desired goal. Consequently, these factors contribute to a discipline-wide ideological support structure, a "shared system of beliefs" (Michell, 1997, p. 374), which maintains the methodological status quo. The institution of psychology is, then, set up in such a way as to prevent questioning of the issue of measurement:

> In the case of measurement in psychology this ideological support structure works to prevent psychologists from recognizing otherwise accessible methodological facts relevant to their research. This is not then a pathology of any individual psychologist. The pathology is in the social movement itself, i.e., within modern psychology. (Michell, 1997, p. 374)

However, while not denying that social factors are involved, individual psychometricians are themselves in an ongoing state of blindness about the facts of measurement. Here Michell introduces the possibility that, at the individual level, such blindness involves motivated ignoring and even self-deception. Insofar as the situation is maintained by individual researchers, Michell writes that "mainstream psychometricians have neglected to test the hypothesis that psychological attributes are quantitative, and … *they attempt to disguise this fact*" (Michell, 2004b, p. 121, italics added; cf. Michell, 2000, p. 639; 2008, p. 10).

Pathological science and sustaining blindness

How might repression and motivated ignoring help explain this state of affairs? To begin with, and before rushing to any psychodynamic explanation, it would probably be safe to say that the vast majority of psychological researchers would be ignorant of the issue of quantity and measurement simply because it does not receive any serious attention in the psychology curriculum, and so there is no need to posit motivation ignoring of self-deception in such cases. For example, Michell's

own surveys demonstrate that psychology texts for the most part omit critical discussion of the issue (see Michell, 1997) and one can turn to any textbook of research methods and statistics to see that this is the case. For example, Gravetter and Forzano's (2012) psychology research methods textbook devotes an entire chapter to "measurement" but never actually discusses either the issue of quantity or defines measurement itself. Nevertheless, whether "accidental" or incidental ignorance is sufficient for explaining this state of affairs is another matter. While it is most likely the case that many in psychology are simply ignorant about what measurement entails, there are nevertheless individuals within psychology purportedly trained to think critically about scientific matters, any of which should be capable of cognising the "otherwise accessible methodological facts relevant to their research" (Michell, 1997, p. 374).

At first glance the case of pathological science and psychometrics could be understood simply as an instance of where the reality principle (observing the world as it is) is being subverted by wishful thinking ("*We believe what eases our minds*, whether it is true or false"—Anderson, 1934, p. 72, his italics). Psychometricians desire their data to be quantitative and simply uncritically believe that this obtains. This account of the wish-fulfilling character of the mind has been taken up (albeit implicitly) by Mele (2001) in his account of self-deception, and a discussion of Mele's account is instructive, since it both illuminates the key features of the problem as well as highlights limitations of Mele's cognitive explanation that Freud's account fills. For Mele (2001), standard "garden variety" cases of self-deception involve situations where a person falsely believes some state of affairs obtains when there is evidence to the contrary. For example, a person may believe that his or her partner is not having an affair when the evidence indicates otherwise. For Mele, the belief is held simply because this is what the person wishes to believe, and while this believing is motivated, there is no necessary implication that the person believes that not $-p$ ($\sim p$) and then comes to deceive him- or herself that p obtains.

Mele provides four examples of how desiring p can lead to the belief in p through misinterpreting relevant evidence either negatively or positively. For instance, a person desiring p may dismiss evidence for $\sim p$ (e.g., an academic might use ad hominem attacks to discount a critic's argument) or even interpret evidence of $\sim p$ as supporting p (e.g., the more that someone says "no", the more this is taken for

evidence of "yes"). Alternatively, a person may focus on confirmatory evidence via selective evidence-gathering while ignoring disconfirming evidence (confirmation bias). With respect to applying this within the context of psychometrics and pathological science, the conditions for self-deception appear to be even stronger because, as Michell observes, the institutional factors are such to take the issue of quantity and measurement as a given. Accordingly, the pathology of science within psychometrics could be explained simply in terms of wishful thinking, unobstructed by an intrusive reality.

However, some account is still required for how *disconfirmatory* evidence is ignored, especially in those cases where the scientist genuinely wishes to be objective towards the matter of enquiry. For example, a variety of responses to Michell's work have, in fact, managed to ignore the issue of quantity, even when directly engaging with Michell's work (e.g., Borsboom & Mellenbergh, 2004; see Michell, 2004b in reply). While wishful thinking may explain how confirmatory evidence is selected (which may be fairly simple: I desire that *p* and thus am sensitive to instances of *p* in the environment), ignoring otherwise available, disconfirmatory evidence requires some further explanation, because some other factor (or factors) is then required to explain how a reflective, critical mental act is prevented from occurring. Mele refers to this as *blindness* when he writes: "Selective evidence-gathering may be analysed as a combination of hypersensitivity to evidence (and sources of evidence) for the desired state of affairs and blindness—of which there are, of course, degrees—to contrary evidence (and sources thereof)" (p. 27). Such "blindness" is the issue at hand, then, that requires explanation. A person may believe that *p* because *p* is desired but then the question becomes one of how the person remains *blind* to evidence to the contrary.

One approach here could be to simply propose that we turn our attention from beliefs and evidence that cause us unpleasure. This could be a simple, defensive response, premised upon Freud's proposal that "psychical activity draws back from any event which might arouse unpleasure" (Freud, 1911b, p. 219). Mele similarly writes: "Because favourable hypotheses are more pleasant to contemplate than unfavourable ones and tend to come more readily to mind, desiring that *p* increases the probability that one's hypothesis testing will be focused on *p* rather than ~*p*" (p. 30). Here Mele offers three possible "sources" of biased belief (vividness of information, the availability heuristic,

and confirmation bias). However, the adequacy of these sources as explanations is questionable, simply because they appear to merely re-describe what is in need of explanation. For instance, "confirmation bias" is where "[p]eople testing a hypothesis tend to search … more often for confirming than for disconfirming instances and to recognize the former more readily …" (p. 29). Since this "tending to search for confirmation rather than disconfirmation" is what we are trying to explain, to then say that people attend to X rather than Y due to "confirmation bias" is simply a circular explanation.[5] Instead, what requires consideration is explaining *selective inattention*, whereby ignoring unpleasant facts cannot preclude the awareness of those same facts.

Varieties of selective inattention

This is where recognising a series of repressive activities may contribute to understanding a variety of responses resulting in varieties of motivated ignoring. In some cases it may simply be that a subset of psychometricians recognise the veracity of Michell's argument and simply "turn a blind eye" to it, because taking Michell's argument seriously would lead to uncertainty and unpleasure associated with added difficulties. The use of Likert data in psychological research appears to provide an example of this. Likert data (numerical rating responses on a scale e.g., strongly disagree = 1 to strongly agree = 4) are generally recognised as providing ordinal rankings rather than interval or ratio scales required for measurement per se (e.g., Jamieson, 2004; Kuzon, Urbanchek, & McCabe, 1996). Nonetheless, Likert data are extensively used with parametric tests that assume at least interval scales of measurement, and this situation appears to be recognised. For instance, McKenzie (2013) recently writes: "Many health-science researchers and clinicians use t-tests and other parametric tests on Likert scale data. This practice is not technically correct, but seems to be undertaken on the basis of tradition, especially in psychology" (p. 100). The likely situation here is that a sub-set of psychological researchers are aware that Likert data are not appropriate for the typically favoured parametric tests and are willing to turn a blind eye to the problem, because parametric tests yield greater chance of statistical significance compared to non-parametric tests (i.e., provide a more satisfying and less unpleasurable outcome).

While turning a blind eye might explain some cases, there is also the possibility of stronger cases of motivated ignoring. In such cases, an

individual might desire to be genuinely scientific about their research, but also feel pressured to perform in ways that subvert that scientific aim. As Michell notes, science occurs in a social setting where "conflicting social interests motivate science, and there is potential for pathologies to arise" (Michell, 2008, p. 8). Stronger cases of selective inattention could result from the individual's conflicting aims (perhaps survival *vs.* superego demands), promoting ignorance of one outcome as compared to another. For instance, considering the social context of the science of psychology—including the pressure to publish or perish, and the publication-career relationship whereby career progression is related to publication outputs, research grants, etc.—it may not be surprising that any factor that provides a threat to research careers (i.e., survival) may be strongly defended against. In fact, if Michell is correct then the implications for current psychology include recognising that much of the discipline is built upon error; that much of current practice is at best play-acting science; that both teaching and research practice require revision; and that the apparent ease of current psychometric methods and statistical packages need to be replaced with a much more rigorous approach to the subject matter. In other words, what Michell is proposing is nothing less than a "paradigm shift" with its concomitant identity crisis for psychology.

It is thus hardly surprising that Michell's thesis could be so threatening and anxiety-provoking that it makes psychometricians incapable of recognising what is in reality a simple fact regarding quantity and psychological attributes. Elad-Strenger (2013) has recently discussed existential crises associated with paradigm shifts within science invoked by the threat of change. She proposes that a researcher's research worldview is associated both with security and self-esteem (and veritable immortality). Threats here to survival and narcissism are likely to invoke defensive responses. While Elad-Strenger's analysis might not extend to all researchers (since one would expect differing degrees of investment in the discipline), the change that Michell is promoting fits the pattern that Elad-Strenger describes, since his conclusions threaten both the livelihoods and narcissistic investment for a variety of individuals. Threats to careers and threats to narcissism, together, mean that it could thus literally be the case, then, that a researcher wishes to be objective and rigorous about their enquiry—and even recognises the central importance and significance of Michell's claims—but simply cannot come to acknowledge it (i.e., she/he suffers from the

blindness of the seeing eye). This is, in essence, a manifestation of the type of pathology that Freud describes in his paper on the resistances to advances in science.

Repression and the resistances to scientific progress

While it may appear reasonable at this junction to produce procedures for identifying resistances in science, as Freud describes, the take-home message from this chapter is much simpler. Whatever your view of human nature, motivated ignoring occurs, and the history of human thinking demonstrates how wishful thinking, selective inattention, and distortion of evidence easily occur, and that objectivity is a hard-won achievement. One need only to look at the creationism/evolution debate to see how the motivated defence of beliefs contribute to maintaining ignorance (e.g., Satan planted fossils to mislead the faithful—see Gardner, 1957). In a similar fashion, then, to the possibility of cultural biases in research, any researcher concerned with objectivity would wish to keep in mind his or her own potential for motivated ignorance. Recognising the possibility of error and paying special attention to the means for both recognising and preventing error is what makes science superior as a method of enquiry (Michell, 2000, 2008; Petocz & Newbery, 2010). However, despite error-detection mechanisms, it is also clear that motivational factors might conspire to nevertheless create blind spots, preventing sensible enquiry in a variety of ways. For instance, a highly respected researcher might knowingly ignore evidence and arguments against his position for fear of loss of status, or an older academic may feel threatened by the arrival of a younger and more productive researcher (a threat to her social standing and narcissistic investment), prompting her to poison his name and keep others ignorant of his research, like the jealous queen in *Snow White*.

Accordingly, despite any question marks surrounding the possibility of repression, Freud's proposal of the resistances to scientific progress affords one of his greatest contributions to science, simply because it highlights the role of affects and the possibility that scientists may have motivated blind spots where attention is being prevented from critically evaluating certain situations. Acknowledging this possibility of bias has important pedagogical implications, since awareness of motivated biases could form part of research curricula aimed at fostering critical thinking (e.g., Michell, 2001; Petocz & Newbery, 2010). In

fact, scientists could be encouraged to become masochistic insofar as unpleasure and discontent with one's own theory could come to signal error, which allows for the potential for correction and revision. Rather, then, than seeing this proposal as an attack on scientific psychology, this discussion affords an opportunity for critical self-reflection, a practice that when at the heart of scientific enquiry provides the greatest possibility of progress.

However, it was also noted earlier that there is the matter of determining whether appeals to "resistance" are simply ad hominem responses to critics, aimed at subverting genuine critical claims. While this matter does not abrogate the possibility of emotive reactions that interfere with scientific enquiry, the issue of judging theories on their objective merit and avoiding ad hominem charges remains. Here we can simply restate the well-known principle that the motivation of the researcher is irrelevant to the truth or falsity of any claims made, and that, instead, the truth or falsity of claims is a matter of evidence and argument. Additionally, it is not being asserted that any negative reaction to a theory is an example of "resistance", as there may be sound reasons for such reactions (Hospers, 1959) and so, again, assessing any criticism of a theory is a matter of evidence and argument alone.

The foregoing should also not be taken to suggest that objectivity is not possible or that science is necessarily irrational as Kuhn (1962) proposes when he writes that in paradigm choice "there is no standard higher than the assent of the relevant community" (p. 94). While so-called "normal science" in Kuhn's view involves scientists solving "puzzles" but accepting the assumptions of the paradigm uncritically (as might describe current psychometric practice), there is no reason why this must necessarily be so, even if it describes observed trends in science. As Michell (2000) observes, "[t]here is nothing intrinsic to the concept of a paradigm that prevents critical enquiry into the truth of paradigms, no matter how difficult in practice" (p. 647). This being so, then, it is perfectly intelligible to ask what factors might be preventing critical engagement and here the resistances to science may be one of possibly many contributing factors.

Conclusion

While the scientific merits of psychoanalysis still receive critical attention and the reality of repression is questioned, Freud's theory nevertheless contributes an essential consideration with respect to the potential

for motivated ignoring (resistance) in science. As the discussion of turning a blind eye and blindness of the seeing eye indicate, there are varieties of motivated ignoring, and while stronger cases of repression may be disputed, the possibility of motivated ignoring is nevertheless an essential consideration for any researcher. Scientists are not rational, computing machines but instead flesh and blood, desiring and affective creatures, who more or less attend to reality as a function of need. While we can become more attentive to the possibility of error, there is a pressing need for greater critical reflection upon motives and responses that may sustain ignorance. Here Freud's theory of repression, and his claim to resistance to scientific progress, constitutes one of Freud's greatest contributions to science, simply because scientists may have motivated blind spots where attention may be deflected from critically evaluating matters relevant to enquiry. While critics have attempted to undermine the possibility of repression, the importance of motivated ignoring in the psychology of science has itself been ignored. In fact, the significance of repression and motivated blindness extends beyond the realm of science and into the world of politics, business, and even debates concerning climate change (see Heffernan, 2011). Accordingly, the relevance of Freudian theory for science and greater society has never been clearer.

NOTES

Chapter Three

1. I agree with Grünbaum that "extension" is a vague term.
2. The words in quotes are those used by Grünbaum (p. 11, this volume) to describe aspects of the clinical theory itself.
3. This is according to Daniel Moerman, a renowned placebo researcher, in a personal communication.
4. Note the similarity here to Davidson's (1963, 1970, 1973, 1974a, 1974b, 1975, 1981) notion of charity, in which there is a default (and charitable) assumption of rationality. There is of course a major difference. Psychological continuity and determination in no way limits one to the strictly rational. (For more on Davidson and charity see Brakel 2009, and Chapter Six of this volume, Part Two.)
5. I say "wrongly" based, because often the similarity categorisation in question is unconscious and predicated upon primary process matchings of inessential features.

Chapter Six

1. Interestingly, these three functions of psychoanalysis have been widely embraced. For instance, in a site as generic as www.answers.com/

topic/psychoanalysis, and as recently as August 8, 2012, one is referred to the *McGraw-Hill Encyclopedia of Science and Technology*, which states: "Psychoanalysis may be defined as 1) a psychological theory; 2) a form of *psychotherapy* …; and 3) a method for investigating psychological phenomena."

2. These three words will be used interchangeably as synonyms in the chapters authored by me. "Representational" is the usage preferred by psychologists, "contentful" by philosophers, and "meaningful" by analysts, although the psychoanalytic usage is less concise.
3. Indeed, understanding a patient's subjective first-person agency is perhaps the central tenet of psychoanalytic treatment; but the importance of the individual agent is also built in to the theory and method, as will be demonstrated.
4. These most basic assumptions are at the level of philosophy of science. There are also technical assumptions, less fundamental but of no less import. These concern repeated objective observations and measurements in order to achieve reliability; requirements for consistency with respect to observations and measurements to assure validity; standards for recording data; rules for designing experimental tests and re-tests; principles for theory development. There are other philosophy of science principles relevant to the topic at hand, specifically concerning standards of evidence. These will be addressed later within Part One of this chapter.
5. This derivability *should* be equally true for any of the several "psychoanalytic schools"—including that of the "classical" ego psychology/development/conflict and compromise; the Kleinian school; the Lacanians; the British object relations school; self-psychology psychoanalysts; and the relational school of psychoanalysis, with its sub schools of intersubjectivists, and social constructivists. I maintain that this derivability *should* hold for all these schools in so far as each should be different merely in accentuating different aspects of the common and shared clinical theory—common because it is derived from the five basic fundamental assumptions of the general psychoanalytic theory of mind, which I will discuss at length below. But as I pointed out earlier (Brakel, 2009, p. 4): "… my view—that all branches of psychoanalysis share the general theory—is not a view universally held. Wallerstein (1988, 1990), for example, who is widely known in the psychoanalytic world for his position that there is a 'common ground' among all the psychoanalytic schools, asserts that the essential commonality owes to psychoanalytic *clinical theory*, not the *general theory*. But Wallerstein (2005, p. 624) recognises that his notion of the common ground is far from generally accepted; he alludes to 'a chorus of critics' who claim

that different metapsychologies necessarily lead to different clinical theories and techniques. Here I side with the critics, but with an important addition—if the different metapsychologies do not include the five basic fundaments of psychoanalytic general theory [to be presented in the text that follows] they are not psychoanalytic theories, whether or not the clinical theories and techniques give the impression of psychoanalytic common ground."

6. "My view" as described earlier (Brakel, 2009, p. 5) "… owes much to, but alters and elaborates that of Freud (1900, 1915e), Rapaport (1944), Shevrin (1984), Brakel (1991), and Shevrin et.al. (1996). Each of these authors has picked out concepts as foundational so as to bring order to the unruly entity called 'psychoanalytic theory'. I continue in this tradition, claiming … that my selection, pertaining just to the general theory, is simpler. For example, Rapaport, although acknowledging that clinical psychoanalytic theory is distinct from the general theory, nonetheless considers 'carry[ing] the method of interpersonal relationship to its last consequences' (p. 202) as one of the basic assumptions. For me, while this is indeed a central element for any account of psychoanalytic [therapeutic] technique, it is [therefore] part of the clinical, not the general theory. Parallel to [but also opposite from] my attempt here to derive the general theory from basic foundational concepts, Rubenstein (1976) proposes the fundaments for a strictly clinical psychoanalytic theory."
7. The type of regularity and lawfulness is not tightly constrained. But some regularity/lawfulness, however minimal, is needed to even allow any scientific investigation to get underway—for example, to be capable of picking out particular phenomena as the phenomena to study.
8. For more on negative hallucinations in clinical settings, see Brakel (1989a, 1989b).
9. As is true for the assumptions of psychoanalysis, gaining [independent, not merely within-the-theory] evidence for the corollary—the existence of primary and secondary process mentation—also requires data and methods independent of psychoanalytic presuppositions. (For more on independent evidence see Shevrin et al., 1996). For examples of such independent evidence for the existence of primary- and secondary-process mentation see Brakel, Kleinsorge, Snodgrass, and Shevrin (2000); Brakel, Shevrin, and Villa (2002); Brakel (2004); and Brakel and Shevrin (2005).
10. I am using the term "a-rational" quite deliberately. "A-rationality" is much broader in its application than is "irrationality"—the term more typically employed. Whereas irrationality implies rationality that has gone astray, entailing a dispositional capacity for rationality, a-rationality implies no such thing. Thinking, for example, can be a-rational if it is

"not yet rational" as in very young humans and "never to be rational/ but good enough for survival" as in certain birds and mammals (see Brakel, 2002; Brakel & Shevrin, 2003). That for Freud the primary processes were earlier, developmentally, suggests that even on a standard psychoanalytic account, a-rationality more properly describes the primary process mode of thinking. Note that the a-rational primary processes are very frequently appropriated to irrational ends as is evidenced in most psychological symptoms. (Indeed this might be the source of the mischaracterisation of the primary processes as irrational instead of a-rational.)

11. These are far too numerous to list. However, to give a small (and admittedly self-serving) sample I will provide just the several studies with which I have had an actual role. For empirical work gaining evidence for a dynamic unconscious and unconscious conflict, see Shevrin et al. (1992) and Shevrin et al. (1996). Regarding primary and secondary processes (or dual processes) see the empirical studies of Brakel and colleagues alluded to in note 9, above.
12. See Garlick, Gant, Brakel, Blaisdell (2011) for an empirical examination of this matter in pigeons.
13. See Brakel (2010, p. 62, pp. 142–144) for an account of the central role of primary processes in conditioning.
14. As surprising as the fourth feature might seem, it can be seen to follow from any one of the other three primary process characteristics mentioned (and of course any combination thereof). If one lacks the capacity to grasp one's own experiences as having continuity, for example, there is no unitary agent holding both X and a contradiction of X to be true. Likewise, and even more basically, before attempts to regulate representations for considerations of truth, any proposition that is considered merely "is". While an external view would regard this as a default consideration-as-true, from the internal viewpoint there is no attempt or even capacity to get the truth conditions right. This being the case, take some X that "is" (i.e., is considered-as-true); but if a contradiction of this X is also considered in this manner, prior to considerations of truth and falsity, this ~X "is" (i.e., is considered-as-true) no less. Finally, tenselessness, too, can yield tolerance for contradiction. If every moment is an unexamined timeless present, a "now" with no history and no future, X held at moment t will not be negated by ~X being held at $t + 1$, nor will the ~X of moment $t + 1$ be negated when at $t + 2$ X is held. I thank Jennifer Church (personal communication) for this final point.
15. I will use a-rational rather than irrational. Irrational implies the rational gone wrong. A-rational refers instead to states not-yet rational and not-yet irrational.

16. The views of Davidson to be presented in this chapter are representative of what is basic to attributionism. Other attributionists change certain features of the attributionist programme without really changing what is at the core. Cherniak (1981), for example, relaxes the criteria for what counts as rational. Dennett (1987, p. 98) agrees with Cherniak. Stich (1983), a projectivist, suggests that projecting one's own beliefs and mental contents will provide less stringent rationality requirements than assuming rational contents in others. And yet these relaxed-criteria versions of rationality all amount to rationality-attributing nonetheless. Hence they pose the same difficulties for the *a-rational* primary process.
17. Also, as long as intra-structural causal relations between states (belief/desire or belief/belief) are most often rational, Davidson does allow that non-rational, causal connections occasionally take place intra-structurally between states. However, as Church (1987) points out, this strategy cannot help Davidson account for the systematic, consistent, even predictable, nature of much irrationality.
18. Davidson's principle of charity argument appears in many places in addition to those cited. See, for example, "Thought and talk" (particularly p. 159) in *Truth and Interpretation* (1984), "Mental events" (particularly pp. 221–223) and "The material mind" (particularly pp. 257–259), both in *Essays on Action and Events* (1980).
19. Dennett's phrase is from Dennett (1978, p. 19).
20. The "divorce" between representations and rationality may not look like a divorce at all. But although Normal-condition, properly functioning beliefs are rational, that they are rational is purely contingent in the Millikan programme. Rational true representations just happen to be the sort that have contributed to the selective fitness of their holders. For representations that are not true (e.g., the lead pellets represented as bugs by toads), the Millikanian position allows that rationality need not be even contingently related.
21. Millikan's biological success criterion for beliefs being true "… not on the average, but just enough …" is a far less stringent requirement than the attributionist's interpretation success criterion, which demands that most beliefs be true.
22. Stich (1990, see especially pp. 55–70) offers arguments against the assumption that true-belief believing enhances selective fitness.
23. This definition of play activity is from Fagen (1981) in *Animal Play Behavior*.
24. This has been demonstrated convincingly for Rhesus monkeys in Symon (1978) in *Play and Aggression: A Study of Rhesus Monkeys*.
25. Note that this analysis, analogous to that regarding beliefs, secures content for those primary process phantasies that are *not* properly

functioning and/or *not* operating under Normal conditions, much as Normal proper functioning beliefs secure the determinate content of faulty and false beliefs.

Chapter Seven

1. Controversies over the meaning of "scientific" are not new. But the earlier rejection of psychoanalysis during the post-Second World War methodological consensus was linked to specific socioeconomic conditions; academic psychologists, concerned to present a "scientific" image in the era of Big Science, used Popper, logical positivism and operationism to marginalise not only psychoanalysis but any non-experimental, non-quantitative psychology in general (Solovey, 2004, 2013).
2. For more on this see section Category Six.
3. One of the problems with neuropsychoanalysis is that, while it acknowledges that "it is, of course, terribly important in this field to be clear about one's conceptualisation of the relationship between the mind and brain" (Solms & Turnbull, 2011, p. 4), that conceptualisation is anything *but* clear, vacillating between Kantianism: "The actual ontological nature of the mind is something epistemologically unknowable" (p. 4) and direct realism: "the mind in itself exists on the same plane as the rest of nature; it is just one of the things that we perceive" (p. 4). Its formulation of dual-aspect monism in terms of two "perceptual surfaces", and its view that "neuroscience offers a second perspective on the unknowable 'thing' that we call the mental apparatus" (p. 5) is conceptually problematic. As Talvitie & Ihanus (2011) remark: "As far as dual-aspect monism is the 'theoretical base' of neuropsychoanalysis, the theoretical base is incoherent" (p. 1597), and, further, "differing metaphysical presuppositions create a substantial obstacle to the integration of neuroscience and psychoanalysis" (p. 1598). However, this cannot be the reason for its being ignored; contemporary psychology and cognitive neuroscience are no less metaphysically confused (see Bennett & Hacker, 2003, for the widespread crypto-Cartesianism in the field). The real stumbling block is the glaring suffix "-psychoanalysis".
4. I am aware that my use of this term is somewhat loose, and departs from its conventional application to quantitative statistical analysis. However, I have selected it deliberately to echo that usage while underscoring parallel possibilities in conceptual and non-quantitative research. Moreover, as Mark Milic (personal communication) has noted, the standard statistical meta-analysis relies on an initial qualitative evaluation of the studies to be included in the analysis, thus

highlighting the sense in which *all* meta-analyses are, in the first instance, qualitative.

5. The themes in this chapter are currently being developed more extensively in two monograph manuscripts in preparation: (1) "The meaning of meaning and its scientific investigation"; (2) "Science and meaning in the Freudian enterprise".
6. Admittedly these two critics are relatively lightweight, especially when compared to Grünbaum, who, according to Mills (2007), is set apart from other critics like Crews in that "he has more substance, breadth and sophistication to his critiques" (p. 539). This is true (though, with comparison targets such as Crews, Eysenck and Popper the competition is not particularly stiff). Be that as it may, most psychologists, unfortunately, have not heard of Grünbaum, while they are all too ready to invoke Eysenck and Popper on the failings of psychoanalysis.
7. That is: "a meaning is not the product of causes but the creation of a subject" (Home, in Wallerstein, 2006, p. 307).
8. Admittedly, many have "tried to grapple with the issue of how to reconcile the search for meanings and reasons through the individual exploration of a unique human life with the effort also to fit the findings derived from that search into the explanatory constructs of a general theory of mind as elaborated within a natural science causal framework" (Wallerstein, 2006, p. 307). For further discussion on how they might be included and reconciled, see later (Category Five); also Petocz, 2011a; Petocz, 2011b.
9. Although there were originally several moderate behaviourists who did attempt to tackle meaning (e.g., Osgood, 1956; Osgood, Suci, & Tannenbaum, 1957), it was the radical behaviourism of Watson and Skinner that came to dominate mainstream psychology as a *distinctive* movement, until challenged by Chomsky and the cognitive revolution. Behaviourism's neglect of meaning has been addressed recently by DeGrandpre (2000) in his paper "A science of meaning—Can behaviorism bring meaning to psychological science?", where he attempts to reconcile behaviourism and meaning via "redefining the process of reinforcement as meaning making" (p. 735).
10. Needless to say, the propositions of the theory must first be properly *understood*. More below.
11. Grünbaum has consistently and convincingly demonstrated that "Popper's treatment of psychoanalysis as the avowed anvil of his theory of scientific rationality was an intellectual fiasco throughout" (2008, p. 587; see also Grünbaum, 1993b).
12. This challenge it shares with mainstream psychology. As Wallerstein (2006) observes, it is a moot point whether, in their aim "to strip

psychoanalysis of the putative theoretical mischief that is … created by its many metaphors and reifications" (p. 306) those who attempt to replace Freud's energic model with the cybernetic model of information processing are offering something that is "any less metaphoric".

13. The (sometimes very considerable) *practical* difficulties of observation are not the issue here.
14. According to Freud (1916–1917), the patient's conflicts "will only be successfully solved and his resistances overcome if the anticipatory ideas he is given tally with what is real in him" (p. 452).
15. Not, however, all of it. I have long felt that the studies conducted by Shevrin and colleagues (Shevrin et al., 1992, 1996) are particularly impressive in their design, appropriate use of mixed methods, and attention to ecological validity.
16. See later (Category Six) for problems in the standard statistical and data-analytic practices in experimental psychology that *all* research (whether or not it pertains to psychoanalysis) must address.
17. This inadequacy extends to a number of themes targeted by Grünbaum. For example, he asks (this volume): "if apples are held to symbolise sex unconsciously for Cézanne or anyone else, why doesn't *anything else* that resembles apples in *some* respect (e.g., being quasi-spherical) do likewise?" (p. 10) The answer lies in overdetermination and the need for meeting necessary *and sufficient* conditions: apples are a (indeed, *the* archetypal) fruit (*juicy*, available to pluck and consume directly); they change from the greenness of immaturity to the red blush of ripeness; potatoes are "apples of the earth"; etc. (cf. the apple with which Eve tempted Adam, the apple offered to sleeping beauty, the golden apples of the Hesperides, and numerous linguistic examples testifying to the female sexual symbolism of apples ("plucking an apple" = having sex; "taking a bite of a green apple" = enjoying a virgin, etc.)) (see Williams, 1994). Of course, this doesn't *prove* their unconscious significance for Cézanne or anyone else; one would want also to follow Freud's advice and consider the person's *associations*—something Grünbaum would not set much store by either.
18. "Augmented" in the sense that it is an extension of "common sense psychology" (see below).
19. These are: (i) the existence of unconscious thoughts, feelings, and motives; (ii) motivational plurality involving ambivalence, conflict, and compromise; (iii) the developmental importance of childhood experiences; (iv) the role of early representations of self and others in later relationships and pathology; and (v) personality development as involving not just regulation of sexuality and aggression but also moving from immature dependence to mature interdependence. Notably (and regrettably) absent is the thesis of a motivational theory of instinctual drives.

20. When behaviourists claim to observe, for example, that *the rat pressed the bar* (as opposed to *the rat's paw moved in the direction of the bar*, etc.), they are not just implicitly assuming the cognition that they explicitly reject, but are actually observing directly the guiding of the rat's movements by its cognition (cf. Dretske, 1988).
21. I am by no means claiming that there are no scientific problems with psychoanalysis in general or with Freud's theory in particular. To begin with, given the proliferation of different versions of the theory (Jungian, Lacanian, Kleinian, Kohutian, etc.), prompting the legitimate question *which psychoanalysis* are we talking about, I have myself (Petocz, 1999) argued that only a scientific version (i.e., one that adheres consistently to realism, determinism, and naturalism) is defensible. Accordingly, I have been quite critical of a number of themes in Freud's work, including the concept of a "system unconscious" and its special "primary process" characteristics, the standard view of symbolism, the confused assimilation of non-conventional meaning to language, the inconsistencies with respect to commitments to realism, determinism, and naturalism, and so on. However, I have also there argued that, with respect to a large number of Freud's central tenets (the role of unconscious processes and the CRS (Conflict-Repression-Substitution) formula for compromise formations, the connections between motivation, emotion, and cognition, the vicissitudes and elaborations of the consummatory activities of the instinctual drives, the importance of early infantile attachment experiences, the role of sexuality and aggression, symbolism, and so on), there is so much of value for a general unified psychology as to encourage any serious scientific psychologist to engage in the task of extraction from and exegesis of Freud's material towards developing a coherent and explanatory version of psychoanalytic theory. Such a version, it may be argued, becomes a coherent and explanatory *general* psychology.
22. As I intimated earlier, the challenge here (which psychodynamic neuroscience will share with all the others) is to work out a coherent conception of the mind-brain relation such that we are clear exactly what the "neuro-" can, and what it cannot, tell us. There is still much confusion here (cf. Bennett & Hacker, 2003), and much more "conceptual housekeeping" (Fotopoulou, 2012b, p. 5) to be done.

Chapter Eight

1. Modern science is like modern liberal states. Internally its practices are often democratic: in the seminar the best argument counts (at least sometimes), no matter who it comes from. In the lab the result counts, no matter the gender or ethnicity of the one who conducts the

experiment. But in its relations to other knowledge traditions modern science is in actual practice all too often like the liberal state: imperialistic. It declares: "If you don't play by our rules discussion ends and the bombs start falling." Of course, there are many scientists who are respectful of other forms of knowledge and other cultural practices than science, and many liberals who oppose imperialistic wars. Nevertheless, I would maintain that the historical record shows us that no reckoning of what modern science and liberalism have offered to the world is complete unless some account is given of what I call here their imperialistic tendency. Paradigmatic in this regard are the complete works of John Stuart Mill. See also, for example, Apffel-Marglin and Marglin (Eds.) (1990).

My comparison between the role of democracy in the political system dominated by the liberal state and in the knowledge system dominated by modern science may seem provocative. That is not how it is meant. My sense of the conditions for debate in contemporary philosophy of mind is that anyone who sees my comparison as a provocation more than as an honest invitation to discussion will not be available for a rational consideration of what Freud and Wittgenstein may be able to offer to the debate.

2. Scientism is sometimes defined as belief in the importance of the scientific method. But in our "post-positivistic time" it seems to me that there is agreement that we do not have agreement about what the scientific method (or methods) is (are). Nevertheless, the ideas I mention have a large, even if often only silent, following.
3. This is not the only way in which scientism can be alive in a culture. I think there is a huge difference between the scientism of today and the scientism, or "positivism" as it was then often called, of, for example, Auguste Comte in the nineteenth century or, to take my favourite example, the Vienna Circle around 1930. For the logical positivists in the early days of the Vienna Circle, such as Otto Neurath and Rudolf Carnap, the vision of one unified, comprehensive and progressive scientific edifice that will provide us access to an ever more complete scientific world view was not a background assumption. It was a programme that the Vienna Circle was committed to explicate philosophically and take further with a missionary zeal. In 1929 the Vienna Circle even published what they called a manifesto for a *Wissenschaftliche Weltauffassung* ("a scientific world view"). Freud was of course not a member of the Vienna Circle, but he shared with it the scientistic mood of his place and time. It was natural for Freud to think that for his discoveries to be legitimate as a form of knowledge they needed to have a legitimate status within science. The discursive constellation in which

Freud's self-understanding had been shaped changed soon after his death. The proud positivistic optimism of the early years of the Vienna Circle gradually imploded through the intrinsic criticism provided by Popper, Quine, Sellars, Kuhn, and others. We also know that in the 1960s and 1970s "hermeneutic anti-positivism" was in vogue thanks to the seminal contributions by Gadamer, Winch, Ricoeur, von Wright, Taylor, Karl-Otto Apel and others. According to them, the methods of empirical science are not adequate if we wish to get access to the phenomena studied in the social sciences and in the humanities.

One aspect of my discussion in this essay is, then, my perception that scientism of today is mostly implicit and cynical in the sense defined here as is also the metaphysical commitment to naturalism that is the (usually hidden and unacknowledged) counterpart to the epistemological commitments of scientism. It follows that the moral self-understanding that is one dimension of scientism is today quite different from what it was when scientism was proudly claimed by Carnap and Quine and other leading philosophers. The collection of essays by Quine (Quine, 1969) is a paradigmatic, and as I agree with Rorty, failed attempt to make scientism and naturalism credible after the demise of the positivistic optimism of the Vienna Circle. For Rorty's position, see Rorty (1980, 1989), and his many interesting replies to his critics in Brandom (Ed.) (2000). For my critique of Quine and of Rorty, see Wallgren (2006).

I think of this—that scientism of yesterday was typically explicit and proudly so and that today it is typically uncritically presumed—as a social fact. I think also that this social fact is a surface symptom of a deep change in the enlightenment heritage towards cynicism with respect to its own ideals. We now live in a time when practices are often in accord with a scientistic perspective even though scientism is not very honourable on philosophical terms and only few people are much concerned to think about this curious constellation. I think of the rise of cynical post-positivistic scientism as one effect of an underlying neo-pragmatic, post-foundationalist turn in analytic philosophy, and a post-modernist turn in continental philosophy that has given to us, since the 1980s, a world in which the "project of modernity" (Habermas, 1981) or of Enlightenment is continued, even though few people now believe in the possibility of providing a philosophical legitimation of it in the sense of Locke, the French encyclopedists, Kant or Hegel. For discussion of these themes, especially of the collapse of the foundational ambitions in analytical philosophy and in critical theory and of Wittgenstein's supreme relevance with respect to this collapse, and for further references, see Wallgren (2006). One interesting comment

on the question of how the spirit of the Enlightenment has changed from the early twentieth to the early twenty-first century is the 2013 edition of the Art Biennale in Venice that was curated under the general title "The Encyclopedic Palace." (The explicit reference of the title is the 1950s vision of the Italian architect Maurzio Auriti, but I take the title of Mariti's work and of the Biennale to be metaphoric for the vision of a world enlightened through the progress of modern science.)

4. In fact, I think it is a bit more than a guess. Something like my (iv) plays a role, I think, when Cohen et al. (2010, p. 107) write: "… enhanced endorphin surges in the context of synchronised performance [of e.g., dancing and walking] might *explain* the positive effect commonly associated with these activities … and … the greater willingness to behave altruistically towards those with whom one performs such activities." (Italics added.)
5. That is an everyday thing to say. But what do I say when I say it? Of what kind is, for instance, the statement: "Few things in life are more wonderful than finding a person with whom you can in one long day ski one hundred kilometres or more in perfect rhythm through the pristine forests of Finnish Lapland." If I say this, is it a confession or a scientific hypothesis? Answer: It can be both.
6. I am of course aware that one line of response to the sceptical questions that I raise is to say: "OK. It is not about the *brain*. Meaning is *embedded* or *enacted* and there need not be any direct correspondence between neurophysiological evidence and mental, psychological or social phenomena." I will not go into this. Let me just say that as long as we remain within a discursive regime defined by a prior commitment to naturalism all reactions of this kind (including varieties of *emergentism*) will serve to blur the issues rather than to clarify them.
7. See Barnosky et al. (2012, pp. 52–58).
8. The Human Brain Project Wins Top European Science Funding. See https://www.humanbrainproject.eu/-/the-human-brain-project-wins-top-european-science-funding [last accessed 30/5/2014].
9. For a fine close-up analysis, from the perspective of the sociology of knowledge, of how the agenda of research about logic, philosophy and human reason may be shaped by other forces than the force of the better argument, see Kusch (1995).
10. My translation.
11. My translation and italics.
12. The early contributions by Horkheimer, Adorno, Marcuse, Fromm and others from the 1930s have later been followed by other ways of relating Freud to the "left-Hegelian" heritage as in Habermas's early book *Erkenntnis und Interesse* (1968). For some remarks that integrate this

kind of interest in Freud with an interest in Wittgenstein, see Honneth "Dezentrierte Autonomie" (2000).

13. Apel's (1973) early, insightful reception of Wittgenstein ended on a deeply critical note and has, as I have argued, stood in the way for a more constructive engagement with Wittgenstein in the tradition of critical theory. For some discussion of Wittgenstein also in relation to Habermas, Foucault (and more peripherally to Derrida) see my *Transformative Philosophy* (2006). For Wittgenstein's remarks on his time, see L. Wittgenstein, *Culture and Value* (1998).
14. In Wittgensteinian scholarship Cavell has perhaps done more than anyone else to claim the relevance of Wittgenstein's philosophy of logic, language and meaning for our thinking about modernity, democracy, and politics. See especially Cavell's magnum opus *The Claim of Reason* (1979). See also, for example, Chapter Eight in Sluga's recent book *Wittgenstein* (2011).
15. See, however, how Janik and Toulmin interpret Wittgenstein's early vision of the unity of logic and ethics as an expression of a typical Viennese mode of discontent with modernity in their *Wittgenstein's Vienna* (1973).
16. For the last point see Wittgenstein, *Culture and Value* (1998, p. 50). For other sources of my remarks on Freud's relevance for Wittgenstein see McGuinness's biography (2005), Monk's biography (1990), McGuiness (2002) and Backström (2013). For discussion of Wittgenstein's Jewishness, see McGuinness (2001), Stern (2001), and Klagge (Ed.) (2001). For further philosophical discussion of the Freud-Wittgenstein relation see also Bouveresse (1995) and Lear (1998).
17. *Philosophical Investigations*, part 1, boxed passage at the end of remark 133. I here use the 4th revised edition by Hacker and Schulte (2009). See also the Bergen edition of Wittgenstein's *Nachlass* (2000) as well as the publications by Waismann (1978, 1984) stemming from the period of Wittgenstein's intense cooperation with him.
18. Pichler (2004), Stern (2004), Klagge (2011), Wallgren (2006). The terms polyphony and heteroglossy as applied to the description of Wittgenstein's style are drawn from Mikhail Bakhtin's work on Dostoevsky (1929, 1934). Because of the associations of the term polyphony with structure in musical composition—associations that are not relevant for present concerns—I prefer heteroglossy as descriptive of Wittgenstein's style. See Wallgren (2013a). For the unity of style and method in the case of Wittgenstein, see also Conant (2002).
19. See, for example, Vlastos (1991), Janik and Toulmin (1973), Gaiser (1968). See also Wallgren (2013b).

20. In addition to earlier cited works see also Baker's essays in his *Wittgenstein's Method: Neglected Aspects* (2004) and Backström (2013) for detailed discussion of some of the topics mentioned here. See also Kuusela (2008).
21. Motto to *Philosophical Investigations*, and *Culture and Value* (1998, p. 6). See also von Wright (1982).
22. See, for example, Freud (1915e).
23. I will speak, for brevity, of brain states and not of events in the brain nor of changes between states. With brain states or states in the brain I mean only the reported facts about evoked response potential brain-waves (ERPs).
24. Cf. Wittgenstein, *Philosophical Investigations* (1981, part 1, remark 352). Wittgenstein's discussion of the example of the series 7777 may suggest that the question about it has no answer at all because the question does not make sense. Our case is different. One idea that the discussion that follows seeks to elucidate is that Wittgenstein is not (dogmatically perhaps) committed to the requirement that when we have problems with concepts the only way to get them right is to break some illusion or overcome speaking nonsense that disguises this as sense. This is a big point in the recent debates in Wittgensteinian scholarship that have been triggered by the groundbreaking studies by Diamond (see especially *The Realistic Spirit* (1991)), Conant (see especially "The method of the tractatus", 2002, op. cit.) and Baker (*Neglected Aspects*, 2004, op. cit.).
25. See again my *Transformative Philosophy*, passim (2006).
26. Here as before I will not distinguish between mental (or brain) states, events, or contents. It will add complexity to our argument if we introduce the distinctions—and if we also introduce attention to the various approaches within the naturalist framework of how the mental and the brain are related—but none of this would be of consequence for our main theme. (See endnote Six above.)
27. For Dennett's and Hacker's views, see especially their debate in Bennett, Maxwell, Dennett, Hacker, and Searle: *Neuroscience & Philosophy: Brain, Mind, and Language*, (2007).
28. The debate between Dennett and Hacker in *Neuroscience & Philosophy: Brain, Mind, and Language* (2007) illustrates well the kind of quagmire one often comes across in contemporary philosophy of mind. The contributions by both discussants are of course extremely interesting. But one wonders how on earth they could even begin to understand each other unless the terms of debate change.
29. In Heidegger's criticism of modernity naturalism—renamed and ingeniously redescribed by Heidegger as presentism—is the target of criticism. Wittgenstein's criticism seems to me to go deeper.

30. Wittgenstein's *Tractatus Logico-Philosophicus* contains an early articulation of the basic tenets of truth-theoretical semantics. Remark 4.024 is the classical reference. Truth-theoretical semantics has been a big hit in analytical philosophy. Important contributions have come also from the Vienna Circle, (esp. Carnap) and from Quine, Sellars, Davidson, Hilary Putnam, Dummett, and many more. In the discussion that follows I will make the, undoubtedly controversial, assumption that in the philosophy of language after the breakthrough achievements by Frege, Russell, and Wittgenstein, and even after the performative and pragmatist turns, the notion that explanation of how language can reach reality is a central part of any successful theory of language has continued to play a key role.
31. See Wallgren 2006 and 2013b.

Chapter Nine

1. Freud in the German original (1923a, p. 321) wrote "seelisch", adjective of soul. Possible translations are mental or psychic.
2. In a methodological context the distinction *empirical/empiristic* has the following meaning: *empirical* describes a set of methods, which make use of systematic observations and experiments. *Empiristic* describes—on a methodological level—a decision for an exclusive use of empirical methods. This decision can or cannot be adequate for the research subject. Misunderstandings arise when someone only accepts empirical methods as *scientific* methods; this evaluation essentially depends on what someone understands by science and research.
3. As Figueiredo (2013) rightly noted, some analytic concepts are used more like passwords, which do not serve communication, but rather indicate membership of an in-group.
4. I agree with Ogden that besides *what the patients think* it is also relevant *how the patients think*. But instead of a shift I would rather speak of a complementary view. The understanding of symbolic meaning remains unchanged and important along with the understanding of the processes of thinking.
5. Such adaptations of concepts put us firmly in another field of research where the use and the constant clarification and differentiation of our concepts are a central concern (see Dreher 2000, 2005 on conceptual research).
6. The studies on mirror neurons give us some hints concerning the bridge between "hardware" and the phenomena of psychic entanglements (e.g., Rizzolatti & Craighero, 2004).

7. These *clinical* processes remind one of the concepts of *epistemological obstacles*, which Bachelard (1938) discusses when writing on the thorny processes of gaining knowledge in *science*: these obstacles are overcome by sudden epistemological breaks or ruptures, and such ruptures are often generated in an interim area between the world of knowledge and the world of imagination. What Bachelard says concerning science may be transferred to psychoanalysis. His conceptualisation highlights how intertwined, in the scientist and in the analyst, the processes of gaining knowledge are with the feeling processes and the processes of imagination.
8. One must certainly add that the missing possibility of falsification also applies to statistical hypotheses, which cannot be falsified; at the most they can be rejected. Even after a statistical decision for or against a research hypothesis, there remains a small probability for the contrary hypothesis. "False", however, is a logical value and would correspond to a probability of 0, which would be impossible for statistical data. The verification of statistical hypotheses is, of course, also not possible.
9. The question of *causal closure* of the world, discussed in this context, is not an empirical but a metaphysical one, which is why a decision can be made *pro* or *contra* the assumption of causal closure.
10. The complexity and the opaqueness of the psyche was not invented by psychoanalysts; it is an evolutionary product. The human psyche as we know is resistant to self-knowledge and to becoming known by others. This is already contained in the Socratic demand "know thyself", which, after all, describes a never ending life-long task.
11. A metaphoric term that is introduced as a didactic means by Plato; in the Socratic dialogues this method is used to assist someone—through skilful questioning—to find their own answer, a kind of intellectual midwifery.
12. An approach of this kind with a naturalistic design is undoubtedly the multi-perspective catamnestic study of the German Psychoanalytic Association (Leuzinger-Bohleber et al., 2003b).
13. Only one argument from many others: "The applicability of the (evidence-based) guidelines may be questioned, or even suspect, when individual patients within the heterogeneous population to which the guidelines are applied in clinical practice differ in certain critical characteristics from those of the trial population on which the guideline recommendation is based. That is, the generalisability of trial results to clinical practice may be compromised by a number of factors involved in execution of the trial, such as where patients were recruited" (Goldberger & Buxton, 2013, pp. 1–2).

14. The results of clinical research are especially of interest for practitioners. It is therefore surprising that studies often only report reliabilities. For practitioners, indeed, validities would be more important, so that they could judge whether that what is measured by an instrument is applicable for their purposes.
15. A pedestrian asks a man, who is evidently looking for something, what he is looking for. "My keys", is the answer. Had he lost his keys here? "No" answers the man, "but here under the street light I can at least see something". Sometimes the selection of methods runs like this story.
16. To be exact, already the drawing of samples from a population should be done at random; even in RCT studies this is not often realised.

Chapter Ten

1. See, however, Boag (2007a, 2007b, 2012) for a critical discussion of Macmillan's assessment of repression.
2. Furthermore, once the repression is lifted, a person can quite accurately say, as did Miss Lucy R., that she or he had known the repressed all along, since it was known (in Breuer & Freud, 1895, p. 117)—what was prevented during repression was *acknowledging* that it was known.
3. Freud's general view on the significance of pain as a motivating stimulus appears incontrovertible (Craig, 2003; Leknes & Tracey, 2008) including pain anticipation as a motivating stimulus (e.g., Rainville, 2002).
4. Personal communication.
5. Explanations that rely on "tendencies" and "dispositions" generally fall into explanatory difficulties (see Boag, 2011).

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