

Evaluating the best current evidence is important, but can we do more? Are we now in a position to give a “final accounting” of Freud’s work? Before answering, I should say what this means, or rather what *I* mean.

If we mean a verdict that is certain, in the sense that it could not possibly be overturned by new evidence, then no assessment of Freud’s work can be final in this sense. A more modest goal would be to give a final accounting in the same sense that we have given one for, say, the hypothesis that penicillin is effective in treating syphilis, or that Laetrile is not effective in treating cancer, or, to take a case more analogous to Freud’s, Darwin’s theory. We have enough evidence in such cases to warrant belief in the respective propositions, even if new evidence could conceivably make a difference. In this sense, we can render a “final” verdict, one not likely to be nullified by new discoveries. Can we do something analogous for Freud’s hypotheses?

One practical problem is that the body of existing evidence is quite large. There are well over 1500 experimental studies of the theory, and a large mass of clinical data, as well as evidence from everyday life. I try to deal with this difficulty by focussing on the best evidence that we now possess. Another problem is the myriad of Freudian hypotheses—too many to deal with in a medium-sized book. I propose to handle this problem by concentrating primarily on Freud’s central hypotheses, especially those that are now claimed to have some empirical support. Freudian hypotheses that are clearly speculative, such as his conjecture about the origin of theism, are rarely claimed to be supported by evidence, and will not be discussed here.

Concerning the remainder of Freud’s hypotheses, I believe that once we fix the proper standards of evaluation, we are in a position to say of *some* of them: There is now enough evidence to judge that this one is true and that one is false. In most cases, alas, the evidence is not so strong in either direction. Consequently, our judgments must be much more tentative. The most we can reasonably say is: This hypothesis has very little empirical support (or no support at all, or is even disconfirmed to some degree); or the hypothesis has more than a little support, but not enough to warrant our believing it.

For most parts of Freudian theory, then, I do not believe that the evidence warrants either acceptance or rejection. Yet, in another sense, I think that a final accounting is possible. If one uses the right standards of evaluation, and gives a correct reading of the current overall evidence, then, except in a few areas where new research is likely to change the picture, a final verdict can be rendered, not because the evidence is definitive, but because it is not likely to get any better. Whether this is so or not depends on several factors, including the kind of evidence that is required for confirmation. The issue of a final accounting is taken up in the last chapter.

This book is intended for philosophers, psychiatrists, psychologists, and others who are interested in either the fate of Freudian theory or therapy, or in philosophical issues about criteria of theory evaluation. The philosophical issues most relevant to assessing the Freudian evidence are the epistemological and philosophy of science issues talked about in part I, but some conceptual, philosophy of mind, and evaluative issues are also discussed in part II.

Part I

Chapter 1

Non-Natural Science Standards

Before turning to evidential issues, it will be useful to say something about the hypotheses to be evaluated.

It is extremely difficult to lay out all of Freudian theory, and it is doubtful that anyone has done this in a perspicuous way (see, e.g., Holt 1989, 327, on the difficulty of separating the clinical theory from the metapsychology). Here, I will be content to specify important parts of Freud's theory, relying heavily on the formulations of writers sympathetic to Freud, such as Brenner (1973), Fisher and Greenberg (1977, 1985), and Kline (1981).

As several commentators have pointed out, it can be misleading to talk as if there were a single item that is "Freudian theory." What often goes by that name is a collection of minitheories that are in varying degrees independent of one another. The epistemological significance of this fact is that they are not all likely to be felled by the same blow. Some may be true; others may be false.

The Hypotheses

The Mental Apparatus

Perhaps the most widely known part of Freudian theory is the division of the human mind into consciousness, the preconscious, and the unconscious. In addition, there is a threefold division of a different sort: an id, ego, and superego. The id is present at birth, is unconscious, and is the source of all instinctual energy. The ego

develops out of the id, is partly conscious, and protects against the id's unacceptable impulses by the operation of defense mechanisms, especially repression. As the child develops, he or she internalizes certain characteristics and demands of the parents; thus, the super-ego or conscience is formed.

The Theory of Dreams

In *The Interpretation of Dreams* (1900, *S.E.*, 4, 5), Freud held that all dreams are wish fulfillments. Furthermore, the operative wishes are repressed wishes originating from the infantile period. They affect dream content in the following way. Dreams serve a sleep-preserving function, but that function is interfered with by wishes emanating from the unconscious that are unacceptable to the ego. Because a person's usual defenses are weakened during sleep, the ego is forced to compromise: sleep continues, and the repressed wishes are allowed into consciousness as the person dreams, but only in a disguised form. So there is something akin to censorship: the dreamer is allowed to become aware of something that reflects a repressed infantile wish, but not in a recognizable form.

Freud thus distinguishes between the manifest and latent content of a dream, that is, the collection of dream images experienced by the dreamer and their underlying meaning. Many of the items that appear as part of the manifest content are symbols, most of which are sexual in nature. In addition to interpreting the dream symbols, an important technique for deciphering the dream is to have the dreamer free-associate about its manifest content. At least in theory, an analyst can thus figure out the latent content and ultimately learn about the dream's unconscious determinants.

The foregoing, simplified account needs to be modified in more than one way. In his later writings (1925, *S.E.*, 20), Freud concedes that not every dream represents a wish fulfillment. Sometimes, as in anxiety dreams, the attempt to represent a repressed wish fails. A better formulation, then, is that every dream constitutes an *attempt* at wish fulfillment. Furthermore, Freud later agrees, in his *An Outline of Psychoanalysis* (1940, *S.E.*, 23), that a residue of preconscious activity in adult waking life can also affect the content of a dream.

Personality Types and Stages of Sexual Development

According to orthodox Freudian theory, each of us goes through four stages of sexual development. In the first year of life, the child passes through the oral stage, during which the mouth is its primary source of pleasure. During the next 3 years, the anal stage, the child's interest shifts to the anus. How it responds to such things as toilet training, defecating, and, in general, the use of its bowels, can have an important effect on personality development. Roughly, from age 3 to 5 years, the child passes through the phallic period, and its genitals become of major concern. There is then a latency period until puberty when an interest in sexual things re-emerges.

How the child reacts during the various stages of sexual development may play a crucial role in the development of the adult personality. A child may become fixated at one stage, or because of later problems may regress to it. In his "Character and Anal Erotism" (1908, *S.E.*, 9), Freud describes the constellation of traits causally linked to the anal stage. The anal character consists essentially of three traits: obstinacy, parsimony, and orderliness. Other Freudians, most notably Abraham ([1924] 1965), have also delineated an oral character. Which traits are likely to be present, however, will vary depending on whether their etiology is linked to the early oral (sucking) or later oral (biting) stage. Kline (1981, 10), following Abraham, connects such items as optimism, impatience, hostility, and cruelty to the sucking stage, and envy, hostility, and jealousy to the biting stage.

The Oedipal Phase and the Castration Complex

Approximately, between the ages of 3 to 5 years, the male child develops a desire to possess his mother sexually, and sees his father as his chief rival. Soon, however, partly because of threats he receives in reaction to his masturbation, the boy comes to fear that his father will cut off his penis. If he can recall seeing genitals of females, he will infer that these females have been castrated and this will enhance his fear that he will be the next victim. Thus, the child develops castration anxiety. As a consequence, the oedipal period comes

to an end. The boy ceases his sexual advances to his mother, and begins to identify with his father.

When the female child discovers that men have a penis and she does not, she concludes that she has been castrated. She turns against her mother, whom she blames for this state of affairs, and develops a desire for a penis, or “penis envy.” She then shifts her affection toward the father, whom she fantasizes as impregnating her. The oedipal phase comes to an end for the female, not because of fear of castration, but because of fear of loss of love.

There has long been controversy about the centrality of the oedipal theory to Freudian theory, but it clearly was of great importance to Freud. Kline (1981, 131) quotes Freud as saying “. . . if psychoanalysis could boast of no other achievement than the discovery of the repressed Oedipus complex, that alone would give it a claim to be counted among the precious new acquisitions of mankind.”

The Defense Mechanisms

In his early writings, Freud sometimes uses “repression” as a synonym for “defense.” In later works, repression counts as but one kind of defense: the keeping of something out of consciousness. (Whether this is all there is to the Freudian idea of repression is controversial; see the discussion of repression in chapter 6, pp. 220–224.) Still, repression is of far more importance in Freudian theory than any of the other defense mechanisms. It plays a crucial role in Freudian explanations of the etiology of the psychoneuroses, dreams, and parapraxes. Indeed, as Grünbaum points out (1984, 3), Freud saw the idea of repression as the “cornerstone” of the whole structure of psychoanalysis (1914, *S.E.* 14:16).

Other Freudian defense mechanisms include denial, reaction formation, projection, and displacement. In denial, the ego wards off something from the external world which it feels as painful; it does this by denying some perception that brings knowledge of such a demand on the part of reality. When engaging in reaction formation, the subject develops an attitude or behavior that is the opposite of the one being defended against. In projection, one attributes to someone else characteristics of oneself that one unconsciously re-

jects. Displacement involves the ego's protecting against instinctual demands of the id by redirecting aggression originally aimed at someone or some thing, and redirecting it toward someone else or some other thing.

The Etiology of Psychoneuroses and Slips

Freud distinguishes between "actual neuroses," such as anxiety neurosis and neurasthenia, and "psychoneuroses," such as obsessional neurosis, hysteria, and depression. The former are caused by events in adult life and are not explainable by Freudian theory. The psychoneuroses, in contrast, arise from the repression of erotic wishes; their symptoms are compromise formations. In fact, neurotic symptoms represent the most economical expression of unconscious conflicts. If they are eliminated without changing the underlying psychic conflict, new and less advantageous symptoms are likely to arise.

Slips of the tongue and other parapraxes, insofar as Freudian theory purports to explain them, are analogous to neuroses. They are caused by repressed wishes, and also constitute compromise formations.

Paranoia

In the Freudian account, the delusions of the paranoid represent a defense against repressed, unacceptable homosexual urges. Through reaction formation, the proposition "I love him" is transformed in his psyche into "I hate him." That proposition, in turn, is transformed, through projection, into "He hates me." It is thus "a remarkable fact," Freud writes, "that the familiar principal forms of paranoia can all be represented as contradictions of the single proposition 'I (a man) *love him* (a man)'" (Freud's emphasis, 1911, *S.E.*, 12:63).

The above highly schematic and incomplete account is intended to identify some of the major sections of Freudian theory to be evaluated; it clearly is not intended to be a substitute for a detailed presentation of Freud's views. One thing that should be clear,

however, is that the subject is Freudian theory and therapy (see chapter 6), not later psychoanalytic theories such as ego psychology, object relations theory, or self-psychology. Many of the epistemological arguments of part 1 are relevant to these theories as well, but they are not my topic (for an excellent discussion of the evidence for these theories, see Eagle 1993).

The topic is also not Freud himself. There has been much discussion recently of arguments that Freud was guilty of self-deception or even calculated fraud in his handling of some of his evidence (Esteron 1993; Crews 1993; Masson 1984). These arguments are clearly relevant to the history of psychoanalysis and to the assessment of Freud's scientific integrity, but they bear on only some of Freud's arguments, and they are clearly insufficient to undermine all of the evidence that others have tried to amass in support of Freudian theory and therapy. I turn next to issues about evidential standards.

Evidential Standards

What are the proper standards for evaluating Freudian hypotheses? One tradition holds that we should use non-natural science criteria, a second that we use the same standards that are employed in the natural sciences. I take up the first tradition in this chapter and the other in the next.

Intuitive Credibility

Some who argue for the use of special evidential standards in Freudian psychology employ such concepts as *self-evidence*, *insight*, or *intuitive credibility*. The philosopher and psychiatrist Karl Jaspers was one of the first to defend such a standard, although in one crucial respect his view was different from contemporary Freudians. In his *General Psychopathology* (1963), Jaspers refers to the 1922 edition of the same work, where he argues for a psychology of "meaningful connections." He claims that the proper standard for determining meaningful connections is self-evidence, but he does not apply this standard directly to Freud's theory, which he takes to be causal in nature. Instead, he recommends its use in judging the theory that is

to replace Freudian theory. In fact, he rejects Freud's theory precisely because of its causal character: "The falseness of the Freudian claim lies in the mistaking of meaningful connections for causal connections" (Jaspers, 1963, 539).

Some contemporary Freudians in the Jaspers tradition liken psychoanalytic interpretations to judgments about works of art. Just as an art critic might have the insight that the elements in a painting have a harmonious relationship to one another, an experienced analyst, at some point in the analysis, can intuit that a certain interpretation of the patient's problem is correct.

The philosopher Charles Taylor has developed a view of this sort. He points out (Taylor 1985) that there are two rival epistemologies that divide American psychologists: the classical and the hermeneutical. The "classical" view, he claims, is dominant among experimentalists. It includes at least two principles. The first requires that hypotheses be "intersubjectively univocal," by which Taylor means that they be based on what he calls "brute data" (117, 121). The second requires that the auxiliary assumptions that link data to a hypothesis be interpretation-free (118).

The first requirement of the classical model, Taylor claims, rules out the use of certain data that we encounter in everyday life, such as the judgment that a painting reflects a powerful harmony between certain elements, or judgments about peoples' characters and motives (118). Taylor's other examples include psychoanalytic interpretations of verbal slips, and the application of the concepts of resistance and repression. These psychoanalytic examples show, Taylor argues, that psychoanalysis cannot meet the demands of the classical epistemological model, nor need they. Psychoanalysis, he claims, is an example—"the most obvious case" (122)—of a hermeneutical science.

What sort of evidential standards does the hermeneutical view presuppose? In his 1985 paper, Taylor does not say, but in an earlier influential paper (1979, 66), he claims that the sciences of man, insofar as they are hermeneutical, have to rely on intuition. In this respect, they differ from the physical sciences, whose theories can be judged by their predictive capacities. Theories in the hermeneutical sciences, in contrast, "are founded on intuitions" (Taylor 1979, 71).

Let us see how the appeal to intuition might work in an example where a Freudian interpretation is offered. Consider an actual case of a 54 year old schizophrenic woman who dragged a broom around a hospital ward for approximately 1 year. Allyn, Haughton, and Hughes (1965) asked two psychiatrists to observe her through a one-way window and to interpret the meaning of her bizarre “symptom.” The first said that the broom represented to the patient some essential perceptual element in her field of consciousness and that her behavior was analogous to that of a small child who refuses to be parted from some favorite toy, or piece of rag, etc. The second psychiatrist gave a more Freudian-like interpretation. Her pacing and broom dragging, he pointed out, could be seen as a ritualistic procedure, a magical action. Her broom would then be, he said: “(1) a child that gives her love and she gives in return her devotion, (2) a phallic symbol, (3) the scepter of an omnipotent queen . . .” (43).

An immediate problem is that we have competing intuitions as to why the woman was dragging the broom around. How do we tell which intuition is correct? Taylor is hardly unaware of this problem. He comments that, in at least some cases where our intuition is challenged, a valid response is to tell the other person to change himself or herself:

Thus, in the sciences of man insofar as they are hermeneutical there can be a valid response to “I don’t understand” which takes the form, not only “develop your intuitions,” but more radically “change yourself.” This puts an end to any aspiration to a value-free or “ideology-free” science of man (Taylor, 1979, p.68)

I fail to see how a Taylor-type response helps. In the case of the broom-dragger, each psychiatrist can tell the other to change, but who is right: the Freudian or the non-Freudian? Intuition alone does not tell us.

The truth is that neither psychiatrist was right. The woman dragged the broom around because she was reinforced for doing this by the hospital attendants. Allyn, et al. (1965) instructed the attendants to give both a cigarette and a broom to the woman, and to reward her with cigarettes every 15 minutes if she continued to hold the broom. By gradually shaping her behavior in this manner,

they induced her to drag the broom around the hospital ward approximately 40% of her waking time. She continued to do this for about a year, at the end of which the investigators deliberately extinguished the so-called symptomatic behavior. Why did the investigators instigate the broom dragging behavior? They apparently wished to expose the baselessness of many psychiatric interpretations.

The case of the schizophrenic woman is only one case, but conflicting intuitions are likely to be a recurring problem in interpreting psychoanalytic data. In cases where a Freudian interpretation might seem appropriate, non-Freudian, psychoanalytic theorists, such as those who embrace ego psychology, self-psychology, or object relations theory, are likely to have intuitions that conflict with those of the Freudian. Cognitivists, operant conditioning theorists, and proponents of either commonsense or physiological hypotheses will also have intuitions in competition with the Freudian's.

Consider Taylor's example of a verbal slip allegedly revealing repression and displacement (1985, 123). If it can be established that this is what the slip reveals, then the revelation might be used as evidence to support some other Freudian hypothesis. One can agree to this without accepting a hermeneutical epistemology. If, however, there are rival and equally plausible interpretations of the slip, then, in the absence of further evidence, we are not warranted in taking the Freudian interpretation to be a datum. Intuition alone is not sufficient for choosing between equally plausible rival interpretations. Suppose, however, that the Freudian interpretation of a patient's behavior is the only one accepted by the analyst? Does such an endorsement enhance the plausibility of the Freudian interpretation, as Taylor appears to suggest? (123) That depends on our empirical evidence. Do we have independent empirical evidence that analysts are usually right in making certain sorts of interpretations of peoples' slips or of other events? If we do not, then the analyst's intuition in favor of a particular interpretation will fail to confirm its correctness.

Taylor's intuitionist criterion, moreover, fares poorly even where we can think of no other interpretation of a neurotic symptom, dream, or slip but a Freudian one. I say that my client cannot remember his friend's name not merely because he forgot it, but

because he is harboring some repressed wish. You agree. You have the same intuition. Even if we cannot think of any other explanation, how would our shared intuitions provide any evidence at all for our hypothesis? If I say that my intuitions tell me that repression caused my client's forgetting, how is that different from saying that I *think* that this is what occurred? If there is no difference, then it can be asked: "Yes, this is what you think, but what is the evidence that your belief is correct?"

Am I saying that intuition can never count as reason for belief? No. Perhaps my intuition that one proposition logically entails another can be a good reason for believing that the entailment holds, but this is a different sort of case. We are talking in the Freudian case about a causal hypothesis (see below), not an obvious necessary truth.

One could reply that there is no need in such a case to appeal to intuition. If the Freudian explanation is the only one available, then we can rely on what philosophers call "an inference to the best explanation." Assume that the repressed wish hypothesis, if true, would satisfactorily explain the client's forgetting, and that it is consistent with what we know about him or her. We might then reason as follows: The repression hypothesis provides the best available explanation of the client's forgetfulness; so, we have some rational grounds for believing it to be correct.

If the above form of inference were valid, it would be valid when applied in the natural sciences. It does not qualify, then, as a non-natural science standard, which is what Taylor (1979, 66) is trying to provide. I discuss the issue of its validity in chapter 2.

I conclude that Taylor's version of a "hermeneutical epistemology," insofar as it relies on intuitive support, yields an inadequate standard for evaluating Freudian hypotheses. I do not intend, however, to rely on his alternative, the "classical model." That model demands that we have "brute data" for confirmation. As Taylor defines this notion (1985, 121), this requires that confirmatory data be *beyond dispute* arising from personal interpretation or discernment. I agree with Taylor that this would impose too high an evidential standard on Freud's hypotheses. If a case can be made for

requiring experimental evidence for confirmation, the argument has to be more subtle than saying simply that we need “brute data,” and that this, in turn, requires experiments.

There are other hermeneutical standards besides Taylor’s, but before turning to them, I want to look at a more powerful attempt to provide an evidential role for intuition.

In two recent brief papers, Thomas Nagel (1994a, 1994b) argues for a nonexperimental approach to the justification of Freudian hypotheses. He argues, first, that most psychoanalytic hypotheses cannot be tested by experiment or statistical analysis, and, second, that such testing is unnecessary for confirmation (1994a, 35). Although both claims are important, the first is of marginal interest to the present topic: the question of the adequacy of non-natural science standards of confirmation. Even if Freudian theory could not be tested experimentally, experimental evidence might still be required to confirm its claims. To say that meeting a certain standard is necessary for confirmation is not to imply the feasibility of doing what is needed to meet the standard. I will be brief, then, in discussing Nagel’s first claim.

Nagel argues that testing Freudian hypotheses experimentally is not logically impossible but is generally impractical. If his argument is cogent, we can save time when examining the evidence from the 1500 plus Freudian experiments that have already been done. We can know in advance that most of the evidence is defective. However, the only reason Nagel gives for thinking Freudian experiments to be impractical is that much of mental life (including, presumably, that part talked about by Freudian theory) consists of multiple causes and background conditions that will never precisely recur (Nagel, 1994a p. 35). The same situation arises, however, when the causes are conscious mental events, but the problem has not prevented cognitive psychologists from doing convincing experimental tests of cognitive hypotheses. Nor has it prevented Freudians from doing numerous experimental studies of repression, to take but one example. What these latter experiments show is controversial (see Holmes, 1990 for a skeptical review), but they cannot be discredited for Nagel’s reason *unless* it can be shown that their capacity to

provide genuine tests presupposes that the phenomena do not consist of multiple causes and background conditions that will not precisely recur. I see no reason to think that this can be shown.

Nagel's point would have more force if it were restricted to singular causal judgments, as in his example of Freud's explanation of why a certain young man forgot the word *aliquis* in a Latin quotation. However, he does not do that; he says (1994a, 35) that the "same problem" (about nonrecurring conditions) also arises for more general Freudian principles.

Even in the case of singular causal judgments, experimental evidence might be indirectly supportive. Suppose that experiments confirmed both that repression does occur and that its occurrence tends to cause the forgetting of certain types of words. That evidence might provide indirect support for Freud's conjecture concerning the young man's forgetting of the Latin word.

Nagel bases his second claim, that experimentation is generally unnecessary for the justification of Freudian hypotheses, on a view about the justification of commonsense psychological explanations *and* about Freudian theory being an extension of commonsense psychology.

In speaking of an extension of commonsense psychology, one might mean that Freudian theory gains credibility from the empirical evidence supporting commonsense claims. An obvious problem for this view is that many Freudian hypotheses, about the oedipal phase of sexual development, the latent meaning of dreams, the sexual etiology of psychoneuroses, etc., go far beyond common sense; in many cases, common sense and Freudian psychology are at war with each other. Nagel, however, is not defending what might be called "an empirical extension" of common sense. He points out (1994a, 34) that Freud took the familiar idea that people are often unaware of their true motives, but carried it so far that he could not defend it just by appealing to common sense.

What Nagel has in mind is a methodological extension of common sense. Commonsense explanations, he contends, provide a distinctive form of understanding from within (1994a, 34), and they are evaluated by a distinctive standard, one not appropriate for particle physics, cancer research, or the study of reflexes (35). Nagel's fur-

ther point (35) is that Freudian explanations provide the same sort of understanding “from within” and should be judged by the same standard we use in commonsense psychology.

An understanding from within works like this. To understand someone else’s thoughts, feelings, or behavior requires that we make sense of the phenomena, even if only “irrational sense,” from his or her point of view, by using our own point of view as an imaginative resource (1994a, 34). In providing Freudian explanations, we do pretty much the same thing, Nagel claims. We put ourselves, so to speak, “in the shoes” of other people, and make sense of their symptoms and responses by attributing to them beliefs, desires, feelings, and perceptions—with the difference that these are aspects of their point of view of which they are not consciously aware (p.34.).

Suppose that Nagel is right about what we typically do when we offer Freudian explanations. What is the distinctive standard of commonsense psychology that we use to evaluate them? First, we need observational data about the subject, something to which we apply our understanding from within. After we figure out how to make sense of someone’s behavior, however, how do we tell if our explanation is correct? Suppose, for example, that we know a good deal about a friend’s personal problems and we try to understand it in Freudian terms, but his or her analyst has a conflicting interpretation. How do we tell which one is correct? The analyst may have more extensive data, but suppose that another analyst looks at the same data, and reaches a different conclusion. By what criterion do we judge who is right?

Nagel’s answer to my question initially appears to be that we judge Freudian explanations by their intuitive credibility. Thus, he says (1940,. 35), that in evaluating Freud’s conjecture about the forgetting of part of the Latin quotation, we simply have to decide whether it is an “intuitively credible” extension of a general structure of explanation that we find well supported elsewhere, and whether it is more plausible than its alternatives. He also says (36), although here he is making a different point, that to many of us, it certainly *feels* (his italics) as if, much of the time, consciousness reveals only the surface of our minds. For many, he notes, this feeling is confirmed by their dreams.

Nagel's special evidential standard, then, seems, at first, to be exactly the same as Taylor's: judge Freudian explanatory hypotheses by appealing to intuition. In that case, his proposal would be open to the same objections as Taylor's.

Grünbaum (1994) rightly asks of this proposal, Whose intuition is to decide which of the rival explanations "makes sense" of the phenomena correctly? He adds that Nagel's recipe degenerates into subjectivity. Nagel's response (1994b) is to explain "intuitive credibility" in a way that distinguishes his evidential standard from Taylor's:

The fundamental causal principle of commonsense psychology is that in most cases, you can discover causally relevant conditions (conditions that make a *difference* in precisely Grünbaum's sense) for a human action or thought or emotion by fitting it into a rationally coherent interpretation of the whole of the person as an intentional subject of this type—by seeing how from the person's point of view it is in some way *justified*. Interpretation reveals causation, because that's the kind of system a human being is. (Nagel's italics, 56)

Referring to the above principle, Nagel says "That's what I mean by intuitive plausibility, and it necessarily applies in the first instance to specific explanations, rather than to general principles" (56).

What reason do we have for thinking that Nagel's principle is correct? An a priori justification is not likely to suffice. It is not an obvious necessary truth that a reason that justifies an action from an agent's point of view was also a cause of it. Nagel's grounds, however, are not a priori but empirical. The principle is well supported, he claims (56), in endless simple cases where it can be independently corroborated by prediction and control. The support from these simple cases, he contends, warrants our application of the same principle in identifying psychological causes in unique and unrepeatable cases: by trying to make intentional and purposive sense of them.

As an example of a simple case, Nagel refers to a man who puts on a sweater because he feels cold. The man, let us assume, desires to be warmer and believes that putting on extra clothing will help him to achieve that goal. He thus has a reason to act, one that justifies his behavior. We reasonably infer that this justificatory reason made a difference in the way he acted, and, therefore, was a

cause of his behavior. If we had reason to doubt that this was the man's reason (perhaps someone paid him to model the sweater), there would be ways to check the accuracy of our explanation. Nagel's principle appears to work in this simple case, but what is the warrant for extending it to non-simple cases?

Nagel's answer (1994a, 35) is that we simply have to decide whether an application is an "intuitively credible" extension of a general structure of explanation that we find well supported elsewhere. I disagree. The justification requires empirical support. The reason is that Nagel's principle can be justifiably applied in simple cases precisely because certain empirical presuppositions hold. We need empirical evidence to know that these conditions are met in a new case; without such evidence, Nagel's principle fails to support a causal inference.

One presupposition of applying Nagel's principle in the case of the man putting on a sweater is that he does this intentionally. Suppose that I trip at a political luncheon, and spill a glass of wine on a rival. I might well want to embarrass him, but without evidence that the act was deliberate, it would be rash to infer that this reason, because it would make sense of the act, was in fact the reason I acted.

A second empirical presupposition is that the cited reason, the desire to become warmer, typically does make a difference in the sort of circumstances under discussion. As Grünbaum puts it in his (1994) reply to Nagel, we need evidence that in a reference class, *C*, the incidence of *Y*'s (say, putting on one's coat) in the class of *X*'s (say, desiring to be warm) is different from its incidence in the class of non-*X*'s. We have such background evidence to draw on in the case of the man putting on his coat, but not, for example, in the case of an academic who feels fully justified in doing something to hasten the death of a hated colleague. If I am such a person and I ignore the colleague's threat of suicide, it would be premature to conclude that my desire to see him out of the picture was what caused my inaction, unless one had evidence that such a desire would typically move me or someone like me to do something so extreme.

A third presupposition is that there be no competing cause, be it a reason or something else, that is just as likely to have been the cause of the action. If it is equally likely that I gave a homeless man \$10 for either of two reasons (but not both), either to help him or

to impress my girlfriend, then Nagel's principle does not support one explanation rather than another.

There is, finally, a fourth presupposition in Nagel's simple case. We are taking for granted that the man had a desire to get warmer. If we had no reason to believe that, we would not be warranted in moving from "This reason would justify the action" to "This reason caused, or was a partial cause, of the action." In general, we are obviously not entitled to infer that *X* caused *Y* without having some good reason for thinking that *X* occurred or was present.

If even one of the above empirical presuppositions is missing, the use of Nagel's principle to warrant a causal inference is problematic. Yet, in typical cases in which Freudian theory is potentially applicable, one or more of these presuppositions does not hold. When people have a particular sort of dream, or commit a slip of the tongue, or develop neurotic symptoms, do we have evidence that they are *intentionally* doing these things? We might if we had prior evidence for Freud's theory of repression, but Nagel is not assuming that we do; if he were, he would not need to appeal to his principle to support Freudian theory. A second crucial presupposition also generally fails in the Freudian cases. Even if we had evidence that slips, dreams, etc. are generally intentional (something that I deny), is there good evidence that they are typically preceded by repressed wishes? The history of attempts to demonstrate the existence of repression (see Holmes 1990) strongly suggests otherwise. Without such evidence, a third presupposition of commonsense psychology is not met: We have no good reason, in applying Freudian theory to a particular case, for believing that repressed wishes generally make a difference to the occurrence of dreams, slips, or neuroses. Finally, quite often, when we try to explain something in Freudian terms, there are competing explanations of equal or greater plausibility.

I conclude, then, that Nagel's principle is generally insufficient to justify singular Freudian causal inferences. The reason is that one or more of the required empirical presuppositions are generally unmet. Of course, we will often encounter similar problems if we try to warrant commonsense explanations by using Nagel's principle. Nagel cites Gorbachev's dismantling of the Soviet empire as a case where experimental evidence is unlikely to be of much help. But is

this not a case where Nagel's principle also fails to provide a justification for a causal inference? It fails precisely because certain empirical presuppositions do not hold. Most of us do not know enough about Gorbachev to say what reasons he had, let alone which ones made the primary difference. Consequently, it is not enough to say: Reason *X*, given his overall views, would have justified his doing what he did. We would still need evidence that this reason was one of *his* reasons *and*, if it were, that it played a part in his decision. Similar problems arise in "endless" cases, to use Nagel's term, where people unjustifiably draw a causal conclusion when all they are warranted in saying is that their explanation makes sense of the agent's behavior.

Is it surprising that Nagel's principle works in simple cases but not in more complex cases? There should be no surprise at all once we realize that the justificatory work it appears to be doing in the simple cases is really being done by the empirical presuppositions that permit its application. Suppose that I have the following empirical evidence concerning the man with the sweater: (*a*) he intended to put it on; (*b*) he had a certain reason to put it on; (*c*) people often put on their sweater for that very reason; and (*d*) we know of no credible alternative explanation. To justify my causal inference, I can appeal directly to this empirical evidence. There is no need to add: his desire to get warm justified his behavior from his point of view. Nagel might agree with this; he does not say that his principle is a basic epistemological principle that by itself supplies warrant. Rather, we trust the principle because of its successful application in simple case.

If Nagel agrees that even in simple cases the warrant for a causal inference is provided by certain empirical presuppositions, then our disagreement lies elsewhere. He holds that intuitive credibility can decide if it is legitimate to extend his principle to Freudian cases. I have argued, in opposition, that this is not enough: We need empirical evidence to decide if the required empirical presuppositions are met, and that evidence is lacking.

Nagel's view of Freudian psychology as an extension of common-sense psychology is shared by other philosophers (e.g., Cavell 1993). I discuss this position further in chapter 3 (pp. 106–124).

Noncausal Readings of Freudian Theory

Although some advocates of a hermeneutical epistemology (e.g., Taylor 1985, 123) agree that Freudian theory makes causal claims, others (e.g., Ricoeur, at least in his early writings, e.g., 1970; Klein 1976, 26) are skeptical about this reading of Freud. They see his theory as being essentially about meanings rather than causes. This position does not by itself entail the appropriateness of special evidential standards. However, its proponents also argue that if a theory is about meanings rather than causes, then natural science standards do not apply. The position must face two powerful objections.

The first is that many of the central hypotheses of Freudian theory, including many defended by such Freudians as Kline (1981) and Fisher and Greenberg (1985), are patently causal in nature. For example, when Freudians claim that repressed wishes engender slips of the tongue, or bring about neuroses, or instigate certain types of dreams, they are obviously making causal claims. Even Ricoeur apparently now concedes this fact (see Grünbaum 1984, 48).

The second problem is that even when Freudian theory does make claims about the meaning of such items as neurotic symptoms, dreams, and slips of the tongue and pen, and these claims are not obviously causal, they typically presuppose causal hypotheses, or their confirmation requires confirmation of related causal claims. To illustrate, suppose that an analyst says that the latent meaning of a dream about watching a girl riding horseback on a desolate beach is as follows: the dreamer has a repressed wish to sleep with his sister who is represented in the dream by the horseback rider. This interpretation presupposes a causal hypothesis: that the repressed wish made a difference to the manifest content of the dream. The dreamer dreamt what he did at least partly because of that wish. In this example, talk about the meaning of the dream presupposes a causal claim. If there were no repressed wish, or none that made a difference to what was dreamt, the meaning hypothesis would be false. In other cases, Freudian meaning hypotheses may not logically imply or presuppose any causal hypotheses, but *proving* them is likely to require confirmation of one or more causal claims.

For example, Freudian theory holds that objects that commonly appear in dreams, such as trees, poles, and other elongated items, symbolize the penis and curved, rounded items represent female sexual organs. Some Freudians have tried to establish the hypothesis in the following way. First, they reason that if such items represent sexual organs outside of dreams, then they probably do so in dreams as well. They then argue as follows: males tend to prefer the sexual organs of females, and females, the sexual organs of males. So we can find indirect evidence for Freudian symbolism in dreams by doing experiments and seeing if male subjects prefer rounded shapes and females, elongated shapes (Jahoda 1956). This argument clearly requires answers to causal questions. In particular, if male subjects express a preference for rounded shapes, is that *because* they associate, perhaps unconsciously, such shapes with female sex organs? Perhaps some other argument could be given for the Freudian view about the meaning of dream symbols, but it is hard to see how such an argument could work without relying on any causal hypotheses at all.

Either of the two points made above about dreams and dream symbols applies to typical (not necessarily all) Freudian hypotheses about the meanings of neurotic symptoms, dreams, and slips of various sorts. Either the hypothesis or its proof will presuppose some causal hypothesis.

Rather than argue that Freudian theory is essentially about meanings, one could argue that the unconscious motives that it postulates are not said to be causally sufficient so even if the theory is true, these motives do not causally necessitate behavior (Radnitzky 1985, 201). The point is correct, but it fails to show that Freudian theory is not a causal theory. The theory does postulate factors that are causally relevant, that is, they make a difference as to what human beings do. If they do, then they are causes.

Radnitzky could reply that unconscious motives, reasons, intentions, and the like never even make a difference to how people act, but then his position would become incoherent. He believes that psychoanalytic theory can be used to explain an individual's conduct, experience, or emotions (206), but it could not do that if

unconscious motives and reasons never made any difference. I may have had an unconscious reason for marrying my wife, but citing it does not explain what I did if I acted solely because of a different reason. In general, it is inconsistent to say that I did *X* because of reason or motive *Y*, and yet *Y* made no difference at all to what I did.

Instead of arguing that Freudian theory is entirely or *essentially* noncausal, one might also argue that it is partly causal and partly not. Charles Nussbaum (1991) defends a mixed account of this sort. He argues (193) that confirmation and disconfirmation of the repression hypothesis require an explanatory model that makes use of reasons or intentions that are not causes, but he also holds that causal explanations cannot be dispensed with altogether. Nussbaum (94–95) acknowledges the problem just mentioned—of saying that reasons are noncausal and yet explanatory—but makes no direct attempt to deal with it. Perhaps his discussion of Freud’s “redescription” of his patient’s behavior (200, 204) was intended to resolve the problem. However, as I will argue shortly, it does not.

The connection between intention and action cannot be causal, according to Nussbaum (196), because in order to explain an action, the connection *must* be described by an analytic statement. It must be so described, Nussbaum argues (195–196), because to explain an event, we must first identify it, and that requires that we construct a practical syllogism of the following form:

P intends to bring about b.

P believes (considers, thinks) he cannot bring about b unless he does a.

P does a.

The above “syllogism,” Nussbaum contends (195), is best understood as a hypothetical statement, which, he claims, is analytically true.

To illustrate, Nussbaum considers the behavior of a man looking around in a field: “When we explain (by reasons) that the man in the field is looking for his lost watch, we construct a practical syllogism which states that he intends to find his watch, that he believes that he will not find it unless he looks about the field, and that he therefore looks about the field” (195). However, the corresponding hypothesis—if P intends to find his watch, and believes he cannot do so unless he looks about the field, then he looks about the

field—is analytic, according to Nussbaum (195–196). Consequently, the connection between intention and action is not causal.

Nussbaum’s argument has at least two serious problems. The trouble begins with one of the first steps: Why believe that to identify an action we need to construct a practical syllogism? If I see a man apparently looking about a field, to identify his action, it is enough to know that he intends to be looking about a field. To know what he is doing, I need not construct a syllogism specifying a belief and further intention (say, to find his watch). Nussbaum concedes (198), in fact, for certain types of behavior, such as that of neurotics, it is not even possible to identify them by constructing a practical syllogism. Yet, in his example (200) of Freud’s analysis of an obsessional woman, the patient performed various actions (such as ringing a bell for her housemaid) and Freud was able to ascertain what they were. He had to do that in order to accomplish what he next attempted: to explain the woman’s behavior in terms of her preconscious desire to mitigate her husband’s embarrassment at being impotent on their wedding night. Nussbaum says of this case (201) that Freud’s explanation consists of offering a redescription: What the patient was “really doing,” according to Freud, was restoring her husband’s honor, or protecting his pride. I shall return to Nussbaum’s contention shortly, but even if it is accepted without objection, my point remains: Freud was trying to explain the woman’s obsessional actions, but to do that, he first had to identify them, and he did that without the help of a practical syllogism.

Without support for this crucial premise—that identification of an action always requires construction of a practical syllogism—Nussbaum’s entire argument collapses, for his conclusion that intentions are not causes depends on the assumption (196) that the connection between intention and action *must* be described by an analytic statement, which in turn is dependent on his initial assumption about the need to invoke a practical syllogism to identify an action.

Even if the initial assumption were granted, however, Nussbaum would need support for a second controversial claim: that the hypothetical proposition that expresses the practical syllogism is analytic. To see the problematic nature of this claim, consider an example given by G.H. von Wright (1971, 116): “We have the premises of a

practical argument: an agent intends to bring about something and considers the doing of something else necessary for this end. It is time for him to act. He thinks so himself. Perhaps he has resolved to shoot the tyrant. He stands in front of the beast, aiming at him with his loaded revolver. But nothing happens.” Nussbaum allows (1991, 195) that an intended action may not occur because of interfering conditions so he might respond that there must be such conditions in von Wright’s case that prevent the man from acting. Why, however, *must* there be such conditions? It might turn out that determinism is not true at the level of human action, and that the causation of action is irreducibly stochastic. Even if determinism is true, at least at the macro level, it is not analytically true: we have no logical or conceptual guarantees that when an action fails to follow, when someone has an appropriate intention and belief, that there will always be interfering conditions or a competing intention.

Nussbaum (195) cites von Wright as a supporter of the logical connection argument, but von Wright, at least in his later writings, does not argue that it is logically or conceptually impossible for the premises of a practical syllogism to be true and the conclusion false. He argues the opposite. Thus, in discussing the man intending to shoot the tyrant, he denies that there is a logical compulsion to say that either he gave up his intention or there were interfering conditions (such as his being paralyzed). On the contrary, he says that we can just as well say, If the case can be imagined, it shows that the conclusion of a practical inference does not follow with logical necessity from the premises. To insist otherwise, he contends, would constitute dogmatism. He adds: “Thus, despite the truth of the Logical Connection Argument, the premises of a practical inference do *not* with logical necessity entail behavior” (von Wright, 1971, 117).

Nussbaum (195) concedes that the issue of the logic of the practical syllogism is difficult, so he is somewhat hesitant in expressing his view. However, for his argument about intentions not being causes to work, he needs to defend his hardly self-evident claim that the hypothetical proposition corresponding to the practical syllogism is analytic. He does not do that, nor does he answer opposing arguments.

I conclude that Nussbaum fails to show that intentions (and reasons) are not causes. The failure of his argument prevents him from reaching his stated goal (194–195) that of showing, contrary to Grünbaum’s view, that some Freudian explanations make use of reasons that are not causes.

Nussbaum also contends (200, 204) that psychoanalytic explanation involves a *redescription* of a patient’s behavior. In support of this general claim, he cites but one case, the one mentioned earlier in which Freud attempts to explain a woman’s obsessional behavior. It would be risky to rest much on this single case given that, as Nussbaum realizes, Freud did not invoke his repression theory to explain the woman’s behavior; rather, he appealed to a noninfantile wish in the woman’s preconscious. Furthermore, the woman herself discovered the wish; it was not uncovered through the use of psychoanalysis. Putting aside doubts about how representative the case is, does Freud attempt to explain by offering a redescription? Does he say, as Nussbaum puts it (201), that what the woman was “really doing” was restoring her husband’s honor or pride? He does not; the “really doing” locution is Nussbaum’s. Instead, Freud explains the obsessional behavior by referring to the woman’s preconscious wish to mitigate her husband’s embarrassment. In doing so, Freud is giving a causal explanation. He is not saying merely that the wish was present when the action was performed; he is also implying that it made a difference to what the woman did. Furthermore, if it had made no difference, then what would be the point of saying that what she was “really doing” was trying to restore her husband’s pride? There are many ways to describe her actions. Why deem Nussbaum’s description privileged unless the woman’s desire to restore her husband’s pride made a difference to what she did?

I am not objecting to the use of the phrase, “what the woman was really doing.” My point is that redescribing the woman’s action by means of Nussbaum’s locution is compatible with also giving a causal explanation of her behavior, which is what Freud was attempting. If he were not attempting to do that, it is unclear how he would be explaining her actions even if he were right about her motive. I conclude that Nussbaum’s attempt to show (202) that some psychoanalytic explanations are noncausal does not succeed.

Thematic Affinity

Instead of interpreting Freud noncausally, some philosophers make a case for special evidential standards by arguing that meaning connections (or so-called thematic affinities) are evidential signs of causal connections. Thus, Donald Levy (1988, 212) claims that to speak of an affinity of any kind (including a thematic affinity) between things signifies generally a causal relation between them. Stated in this bold fashion, Levy's thesis has obvious counter examples. If I speak of an affinity of meaning between two words, I need not signify a causal connection between them; an affinity of content between two wishes in two different people is not evidence, I think even Levy would grant, that one wish caused the other. Last night's dream may have a manifest content very similar to that of a dream I had last year, but that is not reason to think that one dream caused the other. What Levy means, I think, is that generally a thematic affinity between certain *kinds* of items, such as one person's motive and action, or wish and verbal slip, or desire and dream image, is some evidence of a causal connection between the two. However, even this less general thesis seem wrong.

Consider some examples that Grünbaum (1988, 1990) discusses. In the first case, I have a dream that includes the image of a house. There is a thematic affinity between my seeing a house the previous day and my now dreaming about a house, but there may be no warrant for thinking that the first event caused the second. I see at least one house almost everyday (e.g., my own) regardless of whether I then dream about a house. That is true of many other people as well. So, in the absence of additional evidence, I am not warranted in believing that seeing a house the previous day made any *difference* to what I dreamt—despite the presence of a thematic affinity.

In contrast, consider a woman who sees for the first time Frank Lloyd Wright's house Falling Water and then dreams about a house containing the same details. Here we have a strong thematic affinity and a warrant for believing in a causal connection, but what supplies that warrant is specific background information rather than thematic affinity. For example, Grünbaum stipulates that the woman had, until her visit, never heard of Falling Water or seen a picture

or description of it. Another crucial piece of information is that the woman's dream occurred the night of the day she visited Wright's house. Without such background information, the causal inference is unwarranted. Finally, consider a case where a student writes a term paper by copying from an old encyclopedia article. Here we have an extremely high degree of meaning affinity—the two texts are exactly the same—and yet our background evidence about, for example, the very low probability of two papers written by different people being identical if there is no copying supports the charge of plagiarism. These and other cases (Grünbaum, 1990) support the following contentions.

First, thematic affinity *by itself* is not generally evidence of a causal connection. Second, it is not generally evidence of such a connection even when the degree of meaning kinship is very high. If we accept these two points, however, a problem arises, as several philosophers have pointed out (Levy 1988, 212–213; Sachs 1989, 374–377; Hopkins 1988). How do we account for our knowledge of a causal connection in cases of commonsense psychology where there is a meaning affinity between *A* and *B* but no *experimental* evidence that the first caused the second? Consider, for example, Freud's inference that a repressed wish caused a certain young man to forget the Latin word *aliquis* when quoting a line from Virgil's *Aeneid*. As Levy (213) points out, Grünbaum (1984, 258–259) grants, for the sake of the argument, that the man later remembered the word as a result of free-associating, but challenges Freud's conclusion that a repressed wish caused the original forgetting. If thematic affinities are not counted as indicators of a causal connection, Levy asks (213), how do we warrant the claim that free-associating restored the forgotten word to the young man's consciousness? He suggests (213) that to insist on re-creating the situation Freud encountered in controlled experiments seems "far-fetched." If we grant, however, that thematic affinity warrants the causal inference here, then why cannot it not also warrant Freud's inference about the cause of the forgetting?

Sachs (1989, 444) makes the same point when he discusses a certain slip of the tongue. A man turns from the view of a woman's exposed bosom and mutters "Excuse me, I have got to get a *breast* of

flesh air. Sachs assumes that the slip was prompted by a wish to caress, although he concedes that the motive is not obvious. His point is that if we do not allow that the meaning affinity between the wish and the slip is evidence that the first caused the second, then we are unable to account for our knowledge of the causal connection. If we do allow it, then the way is open for Freud to appeal to meaning connections to warrant his causal inferences about repressed wishes in what Sachs calls (374) cases of “opaque” parapraxes.

Both Sachs and Levy assume that we have but two choices in the cases they discuss: either insist on experimental evidence to support our belief in a causal connection *or* concede that thematic affinities (or perhaps, thematic affinities of high strength) are generally evidence of causal connections. There is, however, a third choice (assuming that a causal inference is warranted): we support the inference by appeal to empirical evidence of a nonexperimental kind. Putting aside skeptical doubts about all causal inferences or about knowledge of other minds, there are noncontroversial cases where we reasonably infer that a certain mental event made a difference. Hopkin’s case (1988, 38) of watching someone move a glass toward a tap and inferring a desire to get a drink clearly illustrates the phenomenon. In this sort of case we do not need experimental evidence, but that is because there are an abundance of empirical details available, gleaned from what people tell us about their intention, observations of their subsequent behavior, etc., that support the inference. There is no need, then, to look to thematic affinities or to require experimental confirmation.

In the cases cited by Sachs and Levy, it is not clear that the available empirical details are supportive. But it is also not clear that the causal inferences are warranted. The case of the man who witnessed the exposed breast is not an actual case; it was made up by Grünbaum (1984, 200) to illustrate a point about causal fallacies. Sachs provides no grounds whatsoever for concluding that if such a case were to occur, the slip of the tongue would likely be caused by a wish to caress. If such a slip were to occur, there might well be other equally plausible alternatives (see Erwin, 1993, 440–441). Questions have also been raised about whether Freud’s *aliquis* case was not also made up. If it is an actual case, did Freud have evidence that the free

associating restored the memory of the forgotten word? If he did, he does not tell us what it was. The young man did remember the word after free associating, but he, presumably, was also trying to remember the word. Perhaps it was the striving and not the free associating that caused him to remember.

I am not suggesting that we cannot find evidence to make it plausible that the free associating helped restore the memory. My point is that *if* there is no such evidence, it does not follow that we must concede the legitimacy of appealing to thematic affinities. Another obvious alternative is to say that the causal inference, in the absence of any sort of empirical evidence, is unjustified.

Levy (1988, 214) asks for an argument for denying all evidentiary weight to thematic affinity. I would reply that the burden of proof lies with those who appeal to such affinities to support causal hypotheses. Suppose I were to say, *B* followed *A*; so there is evidence that *A* caused *B*. One could reply as follows. It is not necessarily true that temporal succession is evidence of a causal connection. The proof of this is that there are known cases where *B* follows *A* but there is no evidence that *A* caused it. So, it is unlikely that we can know *a priori* that *B*'s following *A* is generally evidence that *A* caused *B*. Still, we might have empirical evidence that it is generally (but not universally) true that the perception of one event following another is evidence of a causal connection. I say we might have, but in fact we do not. So the burden of proof is on those who would appeal to temporal sequence as a reliable indicator of causation.

An exactly parallel reply can be made to those who say, as James Hopkins does, "So quite generally, connection in psychological content is a mark of causal, and so potentially of explanatory, connection" (1988, 39), Hopkins makes it clear that by "connection in psychological content" he means a meaning connection. As the cases cited earlier show, it is not a necessary truth that where there is a meaning similarity, even one of very high degree, the items being compared are causally connected. If it is not necessarily true, then how can *a priori* considerations show, as claimed by Gardner (1993, 243), that it is *generally true* that psychological proximity, and effectively charged connections of content, signify causal influence? If there is some *a priori* argument to show this connection, then what

is it? The only reason that Gardner gives (1993, 243) is that the alternative is to view the mind as an “atomized jumble of ideas,” which would contradict its identification *as* a mind. But why is this the only alternative? It might be that the mind is not an atomized jumble of ideas and yet sometimes where there is a connection of content, a causal connection is present, but often it is missing.

It might be missing, for example, in Freud’s example (1901, *S.E.*, 6:9) of the young man who tried unsuccessfully to quote a line from the *Aeneid*. A chain of associations, according to Freud, later led from a reflection on the missing word *aliquis* to the miracle of St. Januarias’ clotted blood and eventually to the man’s expressing anxiety about his girlfriend missing her period and possibly being pregnant. Yet what caused the man to forget the word *aliquis* might have had nothing to do with his anxiety about his girlfriend. Rather, his preoccupation with her problem might have caused him to free-associate to the thought of blood, and might have done so even if he had begun the chain of associations by reflecting on a different word, even one he had not forgotten.

As Hopkins points out (1991, 87), there is also a connection in content between a certain experience of Breuer’s patient Anna O and some of her symptoms. Yet, for all we know, the cause of her symptoms may have been physiological. In the case discussed by Sachs (1989, 444), where a man slips and says “breast of flesh air,” even if free-associating revealed a desire to caress, that desire might not have caused the slip. In these cases, where a meaning connection is not correlated with a causal connection, are the minds of the people involved necessarily an “atomized jumble of ideas?” That is hardly obvious. Gardner’s argument, then, lacks cogency: It depends on the dubious premise that if it were not generally true that thematic affinities signified causal connections, the mind would be an atomized jumble of ideas. Perhaps all that Gardner means is that where there is a meaning similarity between two items, there is likely to be a causal explanation of this fact, not that the similarity is evidence of those two items being causally connected to each other. If that is all Gardner is saying, then his point is too weak to be of help to those who take the meaning similarity between *X* and *Y* to be evidence that *X* caused *Y*, or that *Y* caused *X*.

Instead of searching for an a priori argument, we might try to develop an empirical argument for the thematic affinity thesis, but it is up to those who place evidentiary weight on such connections to provide the argument. Perhaps Levy did try to do this by arguing that in cases of commonsense psychology, we are warranted in drawing a causal inference, but do not have, and perhaps could not have, experimental confirmation. However, I have already answered that argument. Neither Levy nor Sachs (1988) provides any other argument. Sachs simply asserts (1988, 374) that certain parapraxes *show* that a wish or effect that preceded them also caused them. That seems very close to saying that the causal connection is self-evident in such cases. As I argued in the exposed breast and the *aliquis* cases, however, if there is a causal connection, it is hardly self-evident.

Hopkins, in contrast, to Sachs and Levy, does try to show, in an interesting series of papers, but especially (1991), that under certain conditions, meaning affinities are signs of causal connections. What Hopkins argues is pertinent not only to Freudian psychology but also to the philosophy of mind and the philosophy of psychology generally (e.g., it is relevant to the traditional problem of other minds and to the defense of folk psychology against criticisms of eliminative materialists).

As Hopkins (123, *n.5*) uses the term “motive,” it applies to a variety of types of psychological causes including beliefs, wishes, or desires and affects such as love, hatred, greed, and lust. A close connection holds between language and motives, Hopkins contends, in the sense that motives characteristically have, or can be given, what he calls a “linguistic articulation” (88). For example, if we say that John believes (hopes, fears, or whatever) that Freud worked in Vienna, we can articulate John’s motive by using the sentence “Freud worked in Vienna.” The motive, then, can be said to have a *content*, which is given by the sentence used to articulate it (89). Hopkins claims (incorrectly, I believe) that such a sentence states what he alternatively calls a “truth-condition” or a “satisfaction condition”: “As I shall be using these terms, the truth condition of ‘Snow is white’ is that snow is white, of ‘Grass is green’ that grass is green and so on, ad infinitum. The notion is used for motives by the way of the sentences that articulate them. Thus the sentence that articulates the

motive of belief in ‘John believes that snow is white’ is ‘Snow is white.’ The truth-condition of this sentence, and hence of the belief itself, is that snow is white” (124, *n.* 9).

As further examples, Hopkins claims that the satisfaction condition of the *hope* that snow is white is that snow is white, and of the *desire* that snow be white is that snow is white (or be white). Finally, Hopkins claims that a logical or conceptual connection lies between a motive and its satisfaction condition: “The condition of satisfaction, realization, or whatever, of a given motive stands in a relation to that motive that is logical or conceptual. It is a norm or rule, given in language, that having a drink of water satisfies a desire to have a drink of water, or that a belief that grass is green is true if grass is green” (124, *n.* 9).

Given this truth-condition relationship, Hopkins claims that the linguistic articulation of a desire serves to describe it as a cause, and in grasping that articulation, we already know a central feature of its causal role, that is, what it is supposed to do (p. 92). Furthermore, the way we interpret one another, according to Hopkins, is by assigning sentences to motives (sentences specifying their content). In doing this we also understand one another in terms of causes: “The finding of sense or meaning, the articulation of object- and satisfaction-directedness, and the establishing of commonsense causal orders, are one and the same” (95). And so our natural criteria for sound interpretation, which are based on content, are at the same time, Hopkins contends, criteria for good causal explanation. Thus, the better a desire and action match in content, the better we take the former to explain the latter. For example, singing the national anthem and a desire to sing the national anthem overlap in content (there is what Grünbaum calls “a thematic affinity”) and this makes the desire particularly well suited to explain the action (95–96).

I will not explore here the prospects for using Hopkins’s analysis, *if* it were correct, to support the more contentious of Freud’s dream interpretations or in warranting his hypotheses about the origin of neurotic symptoms (97, 116). Instead, I want to challenge the analysis itself.

To begin with, I deny Hopkins’s (124, *n.* 9) claim that there is the stated logical or conceptual connection of being a truth condition

between the assertion of the existence of a motive and its condition of satisfaction. That snow is white is, of course, a truth condition for “snow is white,” but it is not a truth condition for “John believes that snow is white.” Clearly, John may *not* believe that snow is white, although it actually is. Similarly, it is not a truth condition for “John desires to have a drink of water” that John actually has a drink of water. Hopkins does realize that John may desire to drink and yet not drink, but the point is that John may drink water without having desired to do so. What might have misled him is the logical or conceptual connection between the following: 1. “John has a desire to have a drink of water,” and 2. “Having a drink of water satisfies that desire.” Given what Hopkins means by “satisfies,” (1) implies (2), but that lends no support to his claim that the motive and its satisfaction condition (desiring the drink and having a drink) are logically or conceptually connected by way of the action being a “truth condition” for the assertion of the existence of the motive. They are not so connected.

Hopkins claims that a connection between a motive and its satisfaction condition enables the articulation of the content of the motive to establish its causal role. Thus, in his view, when we specify the satisfaction condition of a desire (i.e., articulate its content), we thereby “describe it as a cause” (92). But suppose I articulate John’s desire to act always in a purely selfless manner. The satisfaction condition is that John always acts in that manner. I state this condition, but it does not follow that I have described John’s desire as a cause. I would not contradict myself if I said, for example, that John has such a desire, and that the aforementioned condition would satisfy it, and yet the desire has no effect on his actual behavior: whenever he thinks he acts selflessly, he deceives himself. Nor would understanding the content of John’s desire guarantee that I know whether the desire plays any causal role in explaining his behavior. Hopkins speaks of knowing what a desire is “supposed to do” (92). This means what a desire *should* do if acted on intentionally. For example, he claims that a desire to get a drink, if someone acts on it intentionally, *should* produce an action of getting a drink. The basis for this claim, or even what it means is unclear. However, even if Hopkins is right about what desires should do, knowing what a

desire is “supposed to do” is not sufficient for knowing its actual causal role. Do people ever act on the desire to get a drink? Does such a desire ever make any difference at all to the way people behave? These are empirical questions, even if they have obvious answers: they cannot be answered merely by articulating the content of the desire. The overlap in content between the desire to get a drink and the action of getting a drink may be grounds, then, for believing that the desire is, in Hopkins’s words, “supposed to” bring about that action, but it is not grounds for believing that it actually does so.

Even if Hopkins’s claim were only about what a desire is supposed to do, it does not follow that “hermeneutic understanding, and a grasp of the causes of behavior, form a unity” (92). At most, what follows is a unity between the understanding of the content of a desire and knowing what it is *supposed to do*. Knowing the actual causes of behavior requires empirical information, not merely hermeneutical understanding.

It also does not follow from Hopkins’s argument that the finding of motivational “sense” or “meaning” and the establishment of commonsense causal order are one and the same. It is one thing to make sense of what people do by postulating motives; it is another thing *to establish* that a motive featuring content overlap with an action really did cause it. The desire to sing the national anthem may be “particularly well suited” to explain someone’s singing the national anthem, if this means that if the desire is actually present, then singing the anthem would satisfy it, but noticing this overlap in content between the desire and the action is no evidence that the desire did cause the action. Perhaps the action was performed to curry favor with someone who is very patriotic or because failure to sing would incur penalties. Or perhaps the singer did act voluntarily, but had no desire to sing the anthem sincerely, and sang it only to mock the audience. In brief, unless I have seriously misunderstood him, Hopkins’s claims about the articulation of the content of motives fail to support his thesis that an overlap in content between a motive and action or motive and alleged manifest dream symbol is conceptual evidence of a causal link between them.

Besides appealing to linguistic articulation, Hopkins presents examples in which overlap in content purportedly provides grounds for a causal hypothesis. However, in all of his cases, either there is no evidence of causality or if there is, the warrant is provided by other background evidence and not thematic affinity.

Hopkins's first example is that of Josef Breuer's patient, Anna O, who suffered from an aversion to drinking. Under hypnosis, she traced this symptom to an episode in which she watched a dog drink water from a glass. Hopkins claims that the causal link between episode and symptom seems marked in the content of the symptom itself since both were concerned with such topics as drinking water, disgust, anger, and refusal (87). Indeed we have an overlap in content, but why is that grounds for thinking that the episode caused the symptoms to appear? As Grünbaum (1990, 572–573; see also 562–663) notes, Breuer's treatment of Anna O's aversion to drinking water was a failure; that is reason to doubt that he had identified and removed the cause of her phobic reaction.

Another example Hopkins uses concerns Freud's Irma dream. In his preamble to his dream analysis, Freud tells us that his treatment of his patient Irma was only partly successful: some of her somatic symptoms remained. On the day preceding the dream, a junior colleague, Otto, made a remark about her condition, one that Freud interpreted as a reproof. The next morning Freud had a dream about Irma in which the cause of her problems is attributed to an injection given by Otto, who probably, it is suggested in the dream, used an unclean syringe. Later, when reflecting on his dream, he writes, "It occurred to me, in fact, that I was actually *wishing* that there had been a wrong diagnosis; for if so, the blame for my lack of success would have been got rid of" (1900, *S.E.* 4: 109). This hypothesis may have occurred to Freud, but he nowhere gives any evidence that it is true. He was not aware prior to the onset of the dream that he ever had such a wish. Even if he had, he gives no reason to think it had any effect on the content of his dream.

Hopkins says that the hypothesis "fits with" (1991, 101) material in the rest of the dream, in particular with Freud's dreaming that the illness was caused by Otto's injection. If "fits with" means "co-

heres with,” that is true of indefinitely many rival hypotheses and is not evidence for Freud’s particular hypothesis. Hopkins, however, presumably means that Freud’s conjecture *explains* the dream segment involving Otto. But how satisfactory is the explanation? Freud would not have been responsible for Irma’s pain if it were due to Otto’s injection, but he would then have been responsible for a misdiagnosis and the ensuing consequences; for his treatment plan was based on his contention that her pains had a psychological origin. So one has to assume not only that Freud did not want to be blamed for Irma’s continued problems but also, and implausibly, that he did not mind being blamed for a gross misdiagnosis. A more plausible explanation of the Otto dream segment is that Freud had no wish for a misdiagnosis, but merely wished to shift the blame for Irma’s problems to the thoughtless Otto. (He does report remembering that Otto was thoughtless, or something like that, the previous afternoon (*S.E.*, 4: 117.) In short, without some supporting evidence or argument, the hypothesis that Freud puts forth (rather tentatively) concerning the causal relevance of his alleged wish for a misdiagnosis is unsupported. Other things that Freud says about his Irma dream are more plausible, but as Grünbaum (1984, 229) notes, they are supported by grounds from commonsense psychology, as opposed to thematic affinities.

In contrast to the two cases just discussed, it is plausible that Freud’s dreaming of drinking cool water (another case Hopkins cites) was caused by his being thirsty. At the very least, there is reason to believe that he was thirsty while dreaming. He reports having eaten anchovies before going to bed and waking up thirsty. A clear alternative, however, to mere thematic affinity can be given as to what makes the hypothesis plausible. We have background evidence of a Millian kind (see Grünbaum 1993, 154), that events or conditions such as being thirsty or having a desire to urinate often make a difference as to what people dream.

To take another of Hopkins’s examples, when we see someone moving toward a tap with glass in hand, we are often in a position to infer that he or she wants a drink. However, that is not always true. I often perform the same action in order to wash a glass and there is no reason to think that I desire a drink. Where the inference is

warranted, it is so because we have background evidence that the act is not intended to serve some other purpose, that it is voluntary (I am, e.g., not hypnotized), that the agent is thirsty, that people often drink water to satisfy their thirst, and so on. This background information, not mere thematic affinity, provides the warrant. That is also true of someone singing the national anthem. If we did not have grounds for thinking that a particular rendition of the anthem was voluntary, or that people generally sing the anthem because they want to, we might very well have no reason to believe that the desire to sing caused the action.

Hopkins does have some additional examples, but they, too, fail to help his case. In each example, either it is not clear that the inference to a causal hypothesis is warranted *or* it is very questionable that thematic affinity rather than background evidence supplies the warrant.

A proponent of thematic affinities' evidential value could try another tack. Instead of trying to find convincing examples, we could try to formulate a cogent general rule that allows us to separate cases where thematic affinities are signs of causation from those where they are not. It is clearly insufficient, however, to say merely: Thematic affinities are evidence of causation *given* the proper background evidence. We can also say: If *B* followed *A*, that is evidence that *A* caused *B given the proper background evidence*. Such "rules" are too trivial to be of any use to the Freudian cause.

Hopkins does try to state more informative general conditions than the above (for thematic affinities being evidence of causation). He writes: "These conditions include, at the outset, the accurate ascription of base motives, and also a degree of connection between motive and dream that is significant enough effectively to rule out coincidence" (1991, 106). By "degree of connection," Hopkins means degree of *meaning* as opposed to *causal* connection. At least two problems arise with his conditions. The first, and minor, one is that, on at least one natural reading of "coincidence," we may rule out a coincidental relationship between events *A* and *B*, but be unable to discount the operation of some third factor causing *B*, or causing both *A* and *B*. We can avoid this problem by stipulating that ruling out a "coincidental" relationship between a motive and dream