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Paul E. Meehl

Subjectivity in Psychoanalytic Inference: The Nagging Persistence of Wilhelm Fliess's Achensee Question

An alternative subtitle to this essay, which my non-Freudian Minnesota colleagues urged upon me, would have been, "Whose mind does the mind-reader read?" To motivate discussion of a topic not deemed important by some today, consider the story of the last "Congress" between Freud and Fliess, the rupture of their relationship at Achensee in the summer of 1900—the last time the two men ever met, although an attenuated correspondence continued for a couple of years more. Setting aside the doubtless complex psychodynamics, and the prior indications (from both content and density of correspondence) that the relationship was deteriorating, I focus on the intellectual content of the final collision. Fliess had attacked Freud by saying that Freud was a "thought reader" who read his own thoughts into the minds of his patients. Freud correctly perceived that this choice of content for the attack was deadly, that it went for the jugular. Freud's letter to Fliess after the meeting (Freud 1954) indicates that Fliess had written, apparently to soften the blow of the criticism, something about "magic," which Freud again refused to accept and referred to as "superfluous plaster to lay to your doubts about thought reading." (p. 330) A year later Freud is still focusing on the thought-reading accusation, and writes, "In this you came to the limit of your penetration, you take sides against me and tell me that 'the thought-reader merely reads his own thoughts into other people,' *which deprives my work of all its value* [italics added]. If I am such a one, throw my everyday-life [the parapraxis book] unread into the wastepaper basket." (p. 334) In a subsequent letter Freud quotes himself as having exclaimed at Achensee, "But you're

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undermining the whole value of my work." (p. 336) He says that an interpretation of Fliess's behavior made the latter uncomfortable, so that he was "ready to conclude that the 'thought-reader' perceives nothing in others but merely projects his own thoughts into them. . . *and you must regard the whole technique as just as worthless as the others do.*" (p. 337) (Italics added)

Not to belabor the point, it seems that when Fliess wanted to hurt, he knew precisely what was the tender spot, and so did Freud. So that in addressing myself to this vexed topic of the subjectivity of psychoanalytic inference, I am at least in good company in thinking it important. Surely it is strange that four-fifths of a century after the publication of the *Interpretation of Dreams* it is possible for intelligent and clinically experienced psychologists to reiterate Fliess's Achensee question, and it is not easy to answer it.

One has the impression that the epistemology of psychoanalytic inference is less emphasized today than it was in Freud's writings, or in the discussions as recorded in the minutes of the Vienna Psychoanalytic Society. Despite the scarcity of psychoanalytic tapes and protocols (relative to, say, Rogersian and rational-emotive modes), and the lack of any verbatim recordings from the early days, it seems safe to say that the kinds of inferences to unconscious *content* and *life history episodes* that so fascinated Freud, and played the dominant role in his technique, are much less emphasized today. We cannot ignore the fact that Freud considered the dream book his best book. Why is there less emphasis upon discerning the hidden meaning, whether in the restrictive sense of "interpretation" or the more complicated sense of a "construction," than there used to be? I suppose one reason is the tendency among analysts to say, "Well, we don't worry as much about it, because we know the answer." The trouble with that is that there are *two* groups in American psychology who think we now "know the answer," and their answers are very different, consisting of the Freudian answers and the non-Freudian answers. Nor are the non-Freudian answers found only among experimentalists or behaviorists or dust-bowl psychometrists. They are found widely among practitioners and psychotherapy teachers.

One source of the lessened attention to psychoanalytic evidence is the long-term shift—especially complicated because of Freud's never having written the promised treatise on technique—from the original Breuer-Freud abreaction-cartharsis under hypnosis, to the pressure technique focusing upon specific symptoms, to the more passive free association (but still emphasizing the content of the impulse defended against or the memory repressed), to resistance interpretation and, finally, the heavy focus on interpretation of the transference resistance. So that today a large part of analytic intervention is directed at handling the momentary transference, aiming to verbalize the patient's current transference phenomenology with interpretations that are hardly distinguishable from a Rogersian reflection during Rogers's "classical nondirective" period. Such sessions sound and read uninteresting to me. My first analyst was Vienna trained in the late twenties and my second was a product of the Columbia Psychoanalytic Clinic under Rado's aegis, and both spent quite a bit of effort on a variety of interpretations and constructions, the Radovian very actively.

Perhaps the seminal papers of Wilhelm Reich on character analysis—despite Reich's own objection to analysts simply "floating in the patient's productions" and "permitting the development of a chaotic situation" or as Fenichel somewhere puts it, "communicating intermittently and unselectively various thoughts that occur as they listen"—nevertheless had the long-term effect, because they focused on resistance and specifically on the characterological resistances as interferences with obedience to the Fundamental Rule, of narrowing interpretive interventions almost wholly to varying forms of the question, "How are you feeling toward me right now?"

The playing down of the importance of old-fashioned interpreting and constructing I see, perhaps wrongly, as related to an oddity in the views expressed by some well-known institute-trained analysts who, though in good standing with the American Psychoanalytic Society, adopt strange positions. Take Dr. Judd Marmor, whose views are expressed in the preface to the huge tome he edited *Modern Psychoanalysis: New Directions and Perspectives* (Marmor 1968). Before touching gingerly on the topic of nonmedical analysts and considering ambivalently the nature and purpose of the training analysis, he has told us that modern psychoanalysis builds upon the great work of that genius Freud, whose followers we are, and who discovered for the first time a powerful and truly scientific way of investigating the human mind. But we are also told that of course today the classical psychoanalytic technique is not used much because it doesn't work, and that the constructs in Freud's psychoanalytic theory need not be taken very seriously. It is clear that Dr. Marmor is jealous of the designation "psychoanalyst" and "psychoanalysis," but I find it hard to see why. An imaginary analogy: Suppose I tell you that I am a microscopist, that I stand in the succession of that great genius, the founder of true scientific microscopy, Jan van Leeuwenhoek, upon whose discoveries, made by means of the microscope, we contemporary microscopists build our work. Nobody practices microscopy, or is entitled to label himself "a microscopist," who has not attended one of our van Leeuwenhoekian night schools. Of course we no longer use the microscope, since it doesn't work as an instrument; and the little animals that van Leeuwenhoek reported seeing by the use of this device do not exist. What would we think of such a position? It seems to me incoherent. One can argue that if our practice consists almost entirely of handling the moment-to-moment transference phenomenology that occurs during interviews, then the study of Freud's writings is largely a waste of time in preparation for practice of psychoanalytic therapy, and should be classed along with requirements that one study brain physiology or correlational statistics, as tribal educational hurdles for the coveted Ph.D. or M.D. degree that must be met, pointlessly, by would-be psychotherapists! One recalls Carl Rogers's famous view, pushed by him toward the end of World War II, that it takes only a few weeks of intensive training in client-centered therapy to become skillful at it and that most of what therapists study in medical school or graduate school is a waste of time.

I have taken somewhat too long on these preliminary remarks, but I wished to sketch the historical and current sociological context in which Fliess's question is, I think wrongly, often set aside. I must also mention four matters I am *not* considering here, although all of them have great interest and importance. First, I am not concerned to discuss the therapeutic efficacy of classical analysis or psychoanalytically oriented therapy, on which my views are complicated, especially since in recent years I have been doing quite a bit of modified RET (Rational Emotive Therapy), and only recently returned to mixing RET with a modified psychoanalytic approach. Rational emotive therapy and behavior modification are probably the treatments of choice for 80 or 90 percent of the clientele. This view is not incompatible with a view I hold equally strongly, that if you are interested in learning about your mind, there is no procedure anywhere in the running with psychoanalysis. Second, I am not going to address myself to the validation of metapsychological concepts, even though I think that a view like Marmor's is in need of clarification. Third, I'm not going to talk about the *output* aspect of analytic interpretation, i.e., the timing and wording of interventions, but only abut the *input* (cognitive) side, i.e., the way one construes the material, whatever he decides to do with it, including the usual decision to wait. Finally, I shall set aside entirely the experimental and statistical studies, not because I think them unimportant but because I am not very familiar with them except through summaries such as the recent paper by Lloyd H. Silverman in the *American Psychologist* (1976). I believe a person would not become convinced of the truth or falsity of the first-level theoretical corpus of psychoanalysis solely on the basis of the experimental and statistical studies, and that most psychologists who are convinced that there is a good deal in psychoanalytic theory have become convinced mainly by their own experience with it as patient and therapist.

It goes without saying, for anyone familiar with current philosophy of science, that there is a complicated two-way relationship between facts and theories. On the one hand, we can say that clinical experience with psychoanalytic material provides some sort of prior probability when we come to evaluate the experimental and correlational evidence, both when it's positive and when it's adverse; and, on the other hand, whatever general principles can come from the study of either human beings or animals, even of the somewhat attenuated, distantly related kind reported in Robert R. Sears's (1943) classic survey on objective studies of psychoanalytic concepts, can in turn give support to inferences made during the psychoanalytic session itself. My own view, despite my Minnesota training, is that if you want to find out what there is to psychoanalysis, the psychoanalytic hour is still the best place to look. It would be strange—although not logically contradictory—to say that we have in that hour a set of important discoveries that a certain man and a few of his coworkers hit upon while listening to what patients said about their dreams and symptoms and so on, under special instruction for how to talk (and even a prescribed physical posture while talking); but that setting is not a good one for investigating the matters allegedly brought to light! So : The rest of my remarks will deal wholly with the subjectivity in inferences reached from the verbal and expressive behavior the patient displays during the psychoanalytic hour.

I realize that I have not given a clear statement of the problem, and it's not easy. The patient speaks; I listen with "evenly hovering attention" (and background reliance upon my own unconscious). From time to time I experience a cognitive closure that, its content worded, characterizes an inferred latent psychological state or event in the patient's mind. For example, it occurs to me that the patient is momentarily afraid of offending me, or that the dream he reported at the beginning of the session expresses a homoerotic wish, or that the Tyrolean hat in the manifest content is connected with his uncle who wore one, and the like. The essence of psychoanalytic listening is listening for that which is not manifest (Reik 1948), for an inferred (and theoretical) entity in the other's mind that has then imputed to it a causal status. Fliess's Achensee question is: "What credentials does this kind of alleged knowledge bring? When you listen to a person talk, you can cook up all sorts of plausible explanations for why he says what he says. I accuse you, therefore, of simply putting your own thoughts into the mind of the helpless patient."

The epistemological scandal is that we do not have a clear and compelling answer to this complaint eighty years after Fliess voiced it, and a century after Josef Breuer discovered his "chimney sweeping" hypnocathartic technique on Anna O. We may motivate the topic as one of great theoretical interest, which I confess is my main one, as it was Freud's. From the clinical standpoint, however, we "mind-healers" do have the long-debated question as to whether, and how, processes labeled "insight," "uncovering," and "self-understanding" work therapeutically. Non-Freudian therapists like Joseph Wolpe, Albert Ellis, and Carl Rogers have argued plausibly that psychoanalytic efficacy (marginal as it is) is an incidental byproduct of something other than what the analyst and the patient think they are mainly doing. Whatever the merits of these explanations, it is difficult to answer questions about whether and how a correct interpretation or construction works, if one has no independent handle on the epistemological question, "How do we know that it is correct?" In putting it that way I am of course referring to its content correctness and not to its technical correctness, not to the output aspect of the analyst's interpretation. It would be strange, would it not, if we were able to investigate the technically relevant components of the output side of analytic interpretation without having some independent test of its cognitive validity? That is, how would I research the question whether, for instance, summary interpretations at the end of the session are, on the average, more efficacious than tentative ones dropped along the way, (cf. Glover 1940), if the problem of inexact interpretation or totally erroneous "barking up the wrong tree" were wholly unsettled? I do not advance the silly thesis that one must know *for sure* whether the main content of an interpretation is cognitively valid before investigating these other matters. But without some probabilistic statement as to content correctness, it is hard to imagine an investigation into the comparative therapeutic efficacy of the two interpretative tactics. The usual statement that an interpretation is "psychologically valid" when it results in a detectable dynamic and economic change may be all right as a rule of thumb, but it does not satisfy a Fliessian critic, and I cannot convince myself that it should. Although there occur striking experiences on the couch or behind it in which the quality, quantity, and temporal immediacy of an effect will persuade all but the most anti-Freudian skeptic that something is going on, these are not the mode. Furthermore, "Something important happened here" is hardly the same as "*What* happened here is that a properly timed and phrased interpretation also had substantive validity and hence the impulse-defense equilibrium underwent a marked quantitative change."

There are few phenomena—and I do mean *phenomena*, that is, virtually uninterpreted raw observations of speech and gesture, not even first-level thematic inferences—that are so persuasive to the skeptic when he is himself on the couch, or so convincing (even when related without tape recordings or verbatim protocol) to clinical students, as the *sudden* and *marked* alteration in some clearly manifested mental state or ongoing behavior immediately following an analytic interpretation. For readers without psychoanalytic experience, I present a couple of brief examples.

When I was in analysis, I was walking about a half block from the University Hospitals to keep my analytic appointment and was in a more or less "neutral" mood, neither up nor down and with no particular line of thought occupying me, but rather observing the cars and people as they passed. I perceived approaching me a man and woman in their late thirties, both with distinctly troubled facial expressions and the woman weeping. The man was carrying a brown paper sack and over his arm a large Raggedy Ann doll. It is not, of course, in the least surprising (or requiring any special psychodynamic interpretation) that the thought occurred to me from their behavior, the doll, and the fact that they were leaving the University Hospital, that a child was very ill or possibly had just died. It would not be pathological for a person of ordinary human sympathy, and especially a parent, to feel a twinge of sympathetic grieving at such a sight. That is not what befell me on this occasion, however. I was suddenly flooded with a deep and terrible grieving and began to weep as I walked. I don't mean by that that I was a little teary; I mean that I had difficulty restraining audible sobs as I passed people, and that tears were pouring down my face. I told myself this was absurd, I must be reacting to something else, and so on and so forth, none of which self-talk had the slightest discernible effect. On the elevator to go up to my analyst's office were two of our clinical psychology trainees who looked at me somewhat embarrassedly, saying "Good morning, Dr. Meehl," vainly trying to appear as if they had not noticed the state I was in. Even under those circumstances, in an elevator full of people, I literally could not control the weeping, including muffled sobbing sounds. I did not have to wait more than a minute or two for my analyst to appear. Trying to ignore the puzzled expression of a psychiatric social worker whose hour preceded mine, I went in, lay down, and at that point began to sob so loudly that I was unable to begin speaking. After acquiring enough control to talk, I described briefly the people I had met, whereupon my analyst (who, while he had had analysis with Helene Deutsch and Nathan Ackerman, had been exposed to strong Radovian influences in his training institute) intercepted with the brief question, "Were you harsh with Karen [my five-year-old daughter] this morning?" This question produced an immediate, abrupt, and total cessation of the inner state and its external signs. (I had spoken crossly to Karen at the breakfast table for some minor naughtiness, and remembered leaving the house, feeling bad that I hadn't told her I was sorry before she went off to kindergarten.) I emphasize for the nonclinical reader, what readers who have had some couch time will know, that the important points here are the *immediacy* and the disappearance of any problem of *control*—no need for counterforces, "inhibition" of the state, or its overt expression. That is, the moment the analyst's words were perceived, the affective state immediately vanished. I don't suppose anyone has experienced this kind of phenomenon in his own analysis without finding it one of the most striking direct behavioral and introspective evidences of the concepts of "mental conflict," "opposing psychic forces," and "unconscious influences"—the way in which a properly timed and formulated interpretation (sometimes!) produces an immediate dynamic and economic change, as the jargon has it.

Comparable experiences when one is behind the couch rather than on it, usually carry less punch. The reason is not that analysis is a "religious experience," as my behaviorist friends object when I point it out, but that the analysand is connected with his inner events more closely and in more modalities than the analyst is, which fact confers an evidentiary weight of a different qualitative sort from what is given by the analyst's theoretical knowledge and his relative freedom from the patient's defensive maneuvers. True, it is generally recognized that we see considerably fewer "sudden transformations" today than apparently were found in the early days of the analytic movement. We do not know to what extent this reduced incidence of sudden lifting of repression with immediate effects, especially dramatic and permanent symptomatic relief, is attributable to the cultural influence of psychoanalytic thinking itself (a development Freud predicted in one of his prewar papers). There are doubtless additional cultural reasons for changes in the modal character neurosis. There was perhaps some clinical peculiarity (that still remains to be fathomed) in some of the clientele studied during the early days, such that true "Breuer-Freud repression," the existence of a kind of "cold abcess in the mind" that could be lanced by an analytic interpretation-cum-construction that lifted the repression all at once, was commoner in the 1890s than today. These are deep questions, still poorly understood. But it remains true that from time to time symptomatic phenomena that have been present for months or years, and have shown no appreciable alteration despite the noninterpretative adjuvant and auxiliary influences of the therapeutic process (e.g. reassurance, desensitization, and the mere fact that you are talking to a helper) do occur and help to maintain therapist confidence in the basic Freudian ideas.

I recall a patient who had among her presenting complaints a full-blown physician phobia, which had prevented her from having a physical examination for several years, despite cogent medical reasons why she should have done so. She was a professionally trained person who realized the "silliness" of the phobia and its danger to her physical health, and attributed the phobia—no doubt rightly, but only in part—to the psychic trauma of a hysterectomy. Her efforts to overcome it were unsuccessful. Repeatedly she had, after working herself up to a high state of drive and talking to herself and her husband about the urgency of an examination, started to call one or another physician (one of whom was also a trusted personal friend who knew a lot about her) but found herself literally unable to complete even the dialing of the telephone number. Now, after seventy-five or eighty sessions, during which many kinds of material had been worked through and her overall anxiety level markedly reduced, the doctor phobia itself remained completely untouched. From themes and associations, I had inferred, but not communicated, a specific experience of a physical examination when she was a child in which the physician unearthed the fact of her masturbation, which had unusually strong conflictful elements because of the rigid puritanical religiosity of her childhood home (and of the physician also). During a session in which fragments of visual and auditory memory and a fairly pronounced intense recall of the doctor's examining table and so on came back to her, and in which she had intense anxiety as well as a feeling of nausea (sufficient to lead her to ask me to move a wastebasket over in case she should have to vomit), she recalled, with only minimal assistance on my part, the physician's question and her answer. This occurred about ten minutes before the end of the hour. She spent the last few minutes vacillating between thinking that she had been "docile," that I had implanted this memory, but then saying that she recalled clearly enough, in enough sense modalities, to have a concrete certainty that it was, if imperfectly recalled, essentially accurate. As one would expect in a sophisticated patient of this sort, she saw the experience as the earlier traumatic happening that potentiated the effect of the adult hysterectomy and led to her doctor phobia. She called me up the following morning to report cheerily, although a bit breathlessly, that she had refrained from making a doctor's appointment after the session yesterday, wondering whether her feeling of fear would return. But when, on awakening in the morning, she detected only a faint anxiety, she found it possible without any vacillation to make a phone call, and now reported that she was about to leave for her appointment and was confident that she would be able to keep it. I think most fairminded persons would agree that it takes an unusual skeptical resistance for us to say that this step-function in clinical status was "purely a suggestive effect," or a reassurance effect, or due to some other transference leverage or whatever (75th hour!) rather than that the remote memory was truly repressed and the lifting of repression efficacious.

Some argue simply that "clinical experience will suffice to produce conviction in an open minded listener." We are entitled to say, with Freud, that if one does not conduct the session in such and such a way, then he will very likely not hear the kind of thing that he might find persuasive. But the skeptic then reminds us of a number of persons of high intelligence and vast clinical experience, who surely cannot be thought unfamiliar with the way to conduct a psychoanalytic session, who subsequently came to reject sizable portions of the received theoretical corpus, and in some instances (e.g., Wilhelm Reich, Albert Ellis, Melitta Schmideberg, and Kenneth Mark Colby) abandoned the psychoanalytic enterprise. Nobody familiar with the history of organic medicine can feel comfortable simply repeating to a skeptic, "Well, all I can say is that my clinical experience shows. . . ."

The methodological danger usually labelled generically "suggestion," that of "imposing theoretical preconceptions" by "mind-reading one's own thoughts into the patient's mind," is itself complex. An experimental or psychometric psychologist (I am or have been both) can distinguish four main sources of theory-determined error in the psychoanalytic process. First, *content implantation*, in which memories, thoughts, impulses, and even defenses are explicitly "taught" to the patient via interpretation, construction, and leading questions. Second, *selective intervention*, in which the analyst's moment-to-moment technical decisions to speak or remain silent, to reflect, to ask for clarifications, to call attention to a repetition, similarity, or allusion, to request further associations, to go back to an earlier item, etc., can operate either as *differential reinforcement* of verbal behavior classes (a more subtle, inexplicit form of implantation!) or as a *biased evidence-sifter*. By this latter I mean that even if the patient's subsequent verbalizations were uninfluenced by such interventions, what the analyst has thus collected as *his* data surely has been. Third, on the "input" side, there is the purely perceptual-cognitive aspect of subjectivity in discerning the "red thread," the thematic allusions running through the material. (As my Skinnerian wife says, we want the analyst to *discern* the "red thread," we don't want him to spin it and weave it in!) Fourth, supposing the theme-tracing to be correct, we make a *causal* inference; and what entitles us to infer the continued existence and operation of an unconscious background mental process *guiding* the associations (Murray's *regnancy*)? Such a construction does not follow immediately from correct detection of a theme *in* the associations. I focus the remainder of my remarks almost wholly on the third of these dangers, the subjective (critics would say "arbitrary") construing of what the verbal material *means*, "alludes to," "is about."

I do not trouble myself to answer superbehaviorist attacks, such as those that say that science can deal only with observables, hence an unconscious fantasy is inherently an illicit construct; since these attacks, besides being dogmatic, are intellectually vulgar, historically inaccurate, and philosophically uninformed. *The crunch is epistemological, not ontological*. The problem with first-level psychoanalytic constructs is not that they are not observable as test-item responses or muscle twitches or brain waves, but that their inferential status, the way in which they are allegedly supported by the data base of the patient's words and gestures, is in doubt.

I also reject, in the most high-handed manner I can achieve, the typical American academic psychologists' objection that psychoanalysis is not "empirical," which is based upon a failure to look up the word "empirical" in the dictionary. There is, of course, no justification for identifying the empirical with the quantitative/experimental other than either behaviorist or psychometric prejudice, nor to identify the quantitative/experimental with the scientific, nor to identify any of these with what is, in some defensible sense, "reasonable to believe." These mistaken synonymies involve such elementary errors in thinking about human knowledge generally, and even scientific knowledge in particular, that I refuse to bother my head with them.

My late colleague Grover Maxwell used to ask me why I think there is a special problem here, once we have shed the simplistic American behaviorist identification: reasonable = empirical = quantitative/experimental = scientific. Do we not recognize the intellectual validity of documentary disciplines like law, history, archeology, and so on, despite the fact that they (with interesting exceptions, such as the cliometrists) proceed essentially as we do in psychoanalysis? Or, for that matter, what about all of the decisions, judgments, and beliefs we have in common life, such as that we could probably lend money to our friend Smith, or that our wife is faithful to us, or that one Swedish car is better than another?

An analogy between psychoanalytic inference and decision making or beliefs adopted in "ordinary life" is defective for at least three reasons, and probably more.

Most "ordinary life" beliefs do not involve high-order theorizing, but concern fairly simple connections between specific happenings, easily and reliably identified. Herewith a list of ten circumstances that affect the degree of confidence or skepticism with which nonquantified impressions from clinical experience should be assessed:

1. Generalized observations about a variate that is of a "simple, physical, quickly-decidable" nature are more to be trusted on the basis of common experience, clinical impressions, anecdotal evidence, "literary psychology," or the fireside inductions generally (Meehl 1971) than claims about variates, however familiar to us from common life or sophisticated clinical experience, that are not "observable" in a fairly strong and strict sense of that slippery word. Thus shared clinical experience that persons in a grand mal epileptic seizure fall down is more dependable than the

(equally shared) experience that schizotypes readily act rejected by a clumsy therapist's remark in an interview. The fact that an experimental animal stops breathing is a more trustworthy protocol, absent solid data recording, than the "fact" that an experimental animal shows "anxious exploring behavior." We would think it odd if somebody published an article proving, with scientific instrumentation and significance tests, that if you hold a bag of kittens under water for an hour, they will be dead!

2. Contrasted with the preceding are three main categories of not simply observable variates that are readily inferred by us, both in common life and in clinical practice: (a) Inferred inner states or events ("anxiety," "dependency," "hostile," "seductive," "manipulative," "passive-aggressive," "anhedonic," "guarded," "paranoid"); (b) clusters, composites, behavior summaries—more generally *traits*, a trait being conceived as an empirically correlated family of content-related dispositions; and (c) inferred external events and conditions, either current but not actually under the clinician's observation (e.g., "patient is under work stress") or historical (e.g., "patient is obviously from a lower-class social background").

3. Even simple physical observables, however, can sometimes be distorted by theory, prejudice, or otherwise developed habits of automatic inference. A classic example is Goring's study of the English convict in which estimated heights of foreheads were positively correlated with intelligence as estimated by prison personnel, although the estimated intelligence (like the estimated forehead height) did not correlate with *measured* forehead heights! The interesting methodological point here is that guards and prison officials could agree quite reliably on how bright a man is, intelligence being a socially relevant property and one that we know (from many data in educational, military, and industrial settings) has an interjudge reliability of .50 or better, so that pooled judgments can have a high reliability; but because these persons shared the folklore belief that high forehead goes with brains, they apparently "perceived" a prisoner's forehead as higher when they thought the prisoner was bright.

4. If the event being correlated is something strikingly unusual, such as an occurrence or trait that deviates five standard deviations from the mean in populations with which the observer is accustomed to dealing, it is obviously going to be easier to spot relationships validly.

5. States, events, or properties that fluctuate spontaneously over time are hard to correlate with causative factors such as intervention, compared

to those that do not fluctuate much "spontaneously" (that is, absent intervention) over time.

6. States, properties, or dimensions that normally move monotonically in a certain direction over time (e.g., patients usually get progressively sicker if they have untreated pernicious anemia) are more easily relatable than those that show numerous spontaneous "ups and downs" (e.g., spontaneous remissions and exacerbations in diseases such as schizophrenia or multiple sclerosis). The long list of alleged beneficial treatments for multiple sclerosis that have been pushed enthusiastically by some clinicians and have subsequently been abandoned as enthusiasm dies out or controlled quantitative studies are performed, is due not only to the urgency of trying to help people with this dread illness, but also to the normal occurrence of spontaneous remissions and exacerbations, the considerable variability in interphase times, in their severity, and in the functional scarring following an acute episode of this illness (see Meehl 1954, p. 136).

7. The more causal influences are operative, the harder it will be to unscramble what is operating upon what. In the case of cross-sectional correlation data in the social sciences, the intractability of this methodological problem is so great as to have resulted in a special methodological approach, known as path analysis, disputes about whose conceptual power in unscrambling the causal connections still persist to the extent that some competent scholars doubt that it has any widespread validity.

8. If a causal influence shows a sizable time lag to exert its effect, which is often true in medical and behavioral interventions, it is harder to correlate validly than if its effect, when present, is immediate. The other side of this coin is the tendency of minimal effects in behavior intervention to fade out with time. Differences in the impact of an educational procedure—such as the difference between two methods of teaching fractions to third graders—if it is barely statistically significant but not of appreciable size immediately after learning, the chances are good that the children's ability to do fractions problems two years later (let alone as adults) will not be different under the two teaching methods. Yet in opposition to this admitted tendency of interventions to fade out, we have some tendency—claimed but not well documented, if at all—for long-term influences of successful psychotherapeutic intervention to escape detection in immediate post treatment assessment. Whatever the influence of these opposed

tendencies, the point is that the existence of the first and the probability (at least in some cases) of the second greatly increase the difficulty of ascertaining an effect.

9. If the time lag between an influence and its consequence, whatever its average size, is highly variable among individuals (or over different occasions for the same individual), a valid covariation is harder to discern.

10. If there are important feedback effects between what we are trying to manipulate and the subsequent course of our manipulation, the relationships are harder to untangle, especially because there are likely to be sizable differences in the parameters of the feedback system.

More generally, a complicated and controversial topic deserving more discussion than the present context permits, we still do not have an adequate methodological formulation as to the evidentiary weight that ought rationally to be accorded the "clinical experience" of seasoned practitioners when it is not as yet corroborated by quantitative or experimental investigation that meets the usual "scientific" criteria for having formal "researched status." This problem is troublesome enough when the situation is that of practitioners asserting something on the basis of their clinical experience, which, when pressed, they can document only by what amount essentially to an educated guesser's anecdotes, whereas the "anecdotal method" is repudiated as an unacceptable method in any sophomore general psychology course! The problem is made worse when purportedly scientific research on the clinician's claims has been conducted and seems to be unfavorable to his generalization.

On the one side, it must be admitted that some laboratory or even field survey studies of clinical hypotheses are clumsy, naive, and unrealistic in one or more ways, so that one cannot fault a good clinician for dismissing them as irrelevant to what he intended to say. I think, for instance, of a silly study by some academic psychologist (whose familiarity with psychoanalysis must have been confined to reading one or two tertiary sources) who published a paper in a psychology journal alleging to refute Freud's idea of the Oedipal situation because a simple questionnaire item administered to college undergraduates asking whether they preferred their father to their mother, showed that both boys and girls preferred their mother but the latter more so. One can hardly blame Freud for not spending much time monitoring the journals of academic psychology in the 1920s if this is the kind of production they were coming up with. I think it appropriate, being myself both an experienced practitioner *and* a psychometric and experimental psychologist, to venture an opinion as to the main sources of this "pseudoscientific unrealism" on the part of some academics attempting to study a clinical conjecture. (I think it also fair to say that it happens less frequently today than it did, say, between World Wars I and II, during which time very few academic social scientists had any real firing-line experience with mental patients or with intensive psychotherapy.) First, the nonclinician literally fails to understand the clinician's theoretical conjecture with sufficient precision and depth to know what would constitute a reasonable statistical or experimental test of it. Of course, sometimes this is partly the fault of the clinician for not troubling to expound the theory with even that minimal degree of scientific rigor that the state of the art permits. Second, one can understand the essential features of the theory but make simplistic auxiliary assumptions, the most tempting of which is the reliance upon instruments that the clinician would probably not trust (the above undergraduate questionnaire being a horrible example). Third is the possibility that, although the instruments employed are adequate for the purpose, the particular psychological state of affairs is not qualitatively of the same nature or quantitatively as intense as that which the clinician had in mind; as, for example, paradigm studies of psychotherapy in which, rather than having a full-blown clinical phobia brought in by a suffering patient, one has a small-scale "artificial phobia" generated in the psychology laboratory. Fourth, clinicians are likely to do an inadequate job of characterizing the clientele, so that a selection of individuals from a population may yield an incidence of something whose base rate in that population is so different from that of the clinic that a statistically significant result is hard to achieve with only moderate statistical power. (How the clinician can have detected something here that the statistician can't is such a complicated question that I must forego discussion of it here, but it deserves an article in its own right.)

Against all of these proclinical points must be a simple, clear, indisputable historical fact: In the history of the healing arts, whether organic medicine or psychological helping, there have been numerous diagnostic and therapeutic procedures that fully trained M.D.s or Ph.D.s, who were not quacks and who were honorable and dedicated professionals, have passionately defended, that have subsequently been shown to be inefficacious or even counterproductive. No informed scholar disputes this. I cannot see the following as anything but a form of intellectual dishonesty or carelessness: A person with a doctorate in psychology advocates a certain interpretation of neurosis on the basis of his clinical experience. He is challenged by another seasoned practitioner who has, like himself, interviewed, tested, and treated hundreds or maybe thousands of patients, and who is familiar with the conceptual system of the first clinician, but persists in not believing it, and denies the causal relations that the first clinician alleges. The first clinician persists in repeating "Well, of course, I *know* from my clinical experience that . . . ."

I suspect that this kind of cognitive aberration occurs partly because introductory psychology courses no longer emphasize the classic studies on the psychology of testimony, the psychology of superstitions, and the inaccuracy of personnel ratings from interviews and the like, which used to be a staple part of any decent psychology course when I was an undergraduate in the 1930s. It is absurd to pretend that because I received training in clinical psychology, I have thereby become immunized to the errors of observation, selection, recording, retention, and reporting, that are the universal curse on the human mind as a prescientific instrument. Nobody who knows anything about the history of organic medicine (remember venesection!) should find himself in such a ridiculous epistemological position as this. Fortunately, we do find statements about certain classes of patients agreed on by almost all clinicians (it is perfectionistic to require *all*, meaning "absolutely every single one") provided they have adequate clinical exposure and do not belong to some fanatical sect, *their diagnostic impressions being shared despite marked differences in their views on etiology and treatment*. Although consensus of experienced practitioners is strictly speaking neither a necessary nor sufficient condition for the truth—it would be as silly to say that here as to say it about consensus of dentists, attorneys, engineers or economists—presumably something that practitioners trained at different institutions and holding divergent opinions about, say, a certain mental disorder, agree upon is *on the average* likely to be more trustworthy as a clinical impression than something that only a bare majority agrees on, and still less so something that is held by only a minority. One says this despite realizing from history, statistics, or general epistemology that a group that's currently in a small minority may, in the event, turn out to have been out right after all.

Suppose, for example, that two psychologists have each spent several thousand hours in long-term intensive psychotherapy of schizophrenics, either psychotic or borderline. They may disagree as to the importance of genetic factors or the potentiating impact of the battle-ax mother. But it would be hard to find *any* experienced clinician, of whatever theoretical persuasion, who would dispute the statement that schizophrenics have a tendency to oddities of thought and associated oddities in verbal expression. Even a clinician who has bought in on the labeling-theory nonsense and who doesn't think there is any such mental disease as schizophrenia (if there are some funny-acting people in the mental hospitals, they surely don't have anything wrong with their brains or genes—a view that it takes superhuman faith or ignorance of the research literature to hold at the present time), will hardly dispute that one of the main things that leads *other* wicked practitioners to attach the label "schizophrenia" is a fact that he himself has observed *in the people so labeled;* to wit, that they have funny ways of talking and thinking, and it's a kind of funniness that is different from what we hear in neurotics or people who are severely depressed or psychopathic or mentally deficient.

But there simply isn't any way of getting around the plain fact that individuals in the healing arts are not immune from overgeneralization and are sometimes recalcitrant in the presence of refuting evidence, even when the statistical or experimental study cannot be faulted on any of the clinical grounds given above. Everyone knows that this is true in the history of organic medicine (where, by and large, we expect the clinical phenomena to be relatively more objective and easier to observe than in a field like psychotherapy), so that for a long time physicians practiced venesection or administered medicinal substances that we now know have no pharmaceutical efficacy. Surely this should lead an honest psychotherapist to face the possibility that he might *think* that he is helping people or—the main question before us in the present paper—that he is making more correct than incorrect inferences from the patient's behavior, even though in reality he is not doing so, and is himself a victim of a large-scale institutionalized self-deception. Even a practitioner like myself who finds it impossible to really believe this about say, a well-interpreted dream, ought nevertheless to be willing to say in the metalanguage that it *could* be so. His inability to believe it belongs in the domain of biography or "impure pragmatics" rather than in science or inductive logic.

Several thousand people are today totally blind because they developed the disease called retrolental fibroplasia as a result of being overoxygenated as premature newborns. For twenty years or so, obstetricians and pediatricians debated hotly the merits of this allegedly "prophylactic" procedure. It was only when an adequate statistical analysis of the material was conducted by disinterested parties that the question was finally resolved. It is incredible to me that psychotherapists familiar with this kind of development in organic medicine nevertheless counter objections to psychotherapeutic interpretations by doggedly reiterating, "My clinical experience proves to my satisfaction that. . . ."

It is not easy to convey to the nonclinician reader how a seasoned experienced practitioner who has had plently of diagnostic and therapeutic exposure to a certain clientele could come into collision with another one, without giving examples outside psychoanalysis. Consider, for instance, the widely-held view that the battle-ax mother (often called by the theory-laden term "schizophrenogenic mother," despite rather feeble quantitative support for her causal relevance) has a great deal to do with determining the psychopathology of schizophrenia and, perhaps, even its very occurrence. The point is that the clinicians who are convinced of her etiological importance have not made up the raw data, and this explains why other equally experienced clinicians skeptical of the schizophrenogenic mother hypothesis don't find that their (similar) clinical experience convinces them of the same etiological view. I have not talked to any experienced practitioner, whatever his views of schizophrenia or its optimal treatment, who disputes certain "observations" about the way schizophrenics talk when they get on the subject of their mother or subsequent mother figures. But collecting these chunks of verbal behavior about battle-ax mothers is several steps removed inferentially from the common (American) clinicians' conclusion that this patient is psychotic mainly because of the way his battle-ax mother treated him. There are half a dozen plausible factors tending to generate this kind of verbal behavior, and they are not incompatible, so that when taken jointly, it is easy to construct a statistical-causal model that will explain the widespread extent of this clinical experience by practitioners without assuming even the tiniest causal influence of the battle-ax mother syndrome upon the subsequent development of a schizophrenia (see Meehl 1972, pp. 370-371). For a more general discussion of the relationship of clinical or anecdotal generalizations to criticism and the difficulty of assessing the relative weight to be given to it in relation to more scientifically controlled studies, see Meehl 1971, especially pages 89-95.

Even those ordinary-life conclusions that are not themselves explicitly statistical nevertheless are often *based upon* experimental or statistical findings by somebody else. Sometimes these findings are known to us, sometimes we rely on reports of them because we have previously calibrated the authorities involved. Thus, for instance, in buying life insurance we rely upon actuarial tables constructed by insurance statisticians, and we also know that the law constrains what an insurance company can charge for a given type of policy on the basis of these statistics. The actuarial table's construction and interpretation is a highly technical business, beyond the insured's competence to evaluate. But he does not *need* to understand these technicalities in order (rationally) to buy life insurance.

In most ordinary-life examples, one is forced to make a decision by virtue of the situation, whereas a psychology professor is not forced to decide about psychoanalytic theory. If somebody replies to this by saying that the *practitioner* is forced to decide, that's not quite true, although it has a valid element. The practitioner is not forced to decide to proceed psychoanalytically in the first place; and, pushing the point even farther back, the psychologist was not forced to be a psychotherapist (rather than, say, an industrial psychologist).

For these three reasons, the easy analogy of a psychoanalytic inference about an unconscious theme, or mechanism, or whatever, with those less-than-scientific, action-related inferences or assumptions we require in ordinary life, is weak, although not totally without merit.

Suppose one drops "ordinary life" as the analogy and takes instead some other nonexperimental, nonstatistical but technical scholarly domain, such as law or, usually, history. The evidence in a law court is perhaps the closest analogue to psychoanalytic inference, but inferences in history from fragmentary data and empirical lawlike statements that cannot be experimented on are also a good comparison. The analogy breaks down somewhat in the case of law, however, in that the application of legal concepts is not quite like an empirical theory, although the lawyer's inferences as to the fact situation are epistemologically similar to those of psychoanalytic inference.

A difficulty in relying on Aristotle's dictum about "precision insofar as subject matter permits" is that this rule doesn't tell us whether, or why, we ought to have a high intellectual esteem for a particular subject matter. If the subject matter permits only low-confidence, nonquantitative, impressionistic inferences, operating unavoidably in a framework not subjected to experimental tests in the laboratory, or even file-data statistical analysis, perhaps the proper conclusion is simply that we have a somewhat shoddy and prescientific discipline. Doubtless some physical scientists would say that about disciplines such as history, or the old-fashioned kind of political science, as well as psychoanalysis. In either case one cannot take very much heart from the analogy. It is, I fear, really a rather weak defense of psychoanalytic inference unless we can spell it out more.

However, an interesting point arises here in connection with the flabbiness of statistical significance testing as a research method. I think it can be shown—but I must leave it for another time—that the use of either a Popperian or a Bayesian way of thinking about a criminal case gives stronger probabilities than those yielded by null hypothesis testing. This is an important point: The fact of "explicit quantification," i.e., that we have a procedure for mechanically generating *probability numbers*, won't guarantee strong inference or precision, although it looks as if it does, and most psychologists seem to think it must. I am convinced that they are mistaken. The "precision" of null hypothesis testing is illusionary on several grounds, the following list being probably incomplete:

1. Precision of the tables used hinges upon the mathematical exactness of the formalism in their derivation, and we know that real biological and social measures don't precisely fill the conditions. That a statistical test or estimator is "reasonably robust" is, of course, no answer to this point, when precision is being emphasized.

2. Random-sampling fluctuations are all that these procedures take into account, whereas the most important source of error is systematic error because of the problem of validity generalization. Almost nothing we study in clinical psychology by the use of either significance tests (or estimation procedures with an associated confidence belt) is safely generalizable to even slightly different populations.

3. The biggest point is the logical distance between the statistical hypothesis and the substantive theory. For a discussion of this see Meehl (1978, pp. 823-834; Meehl 1967/1970; Lykken 1968; and Meehl 1954, pp. 11-14, Chapter 6, and passim).

As to the analogy of psychoanalytic inference with highly theoretical interpretive conjectures in history (e.g., hypotheses about the major factors leading to the fall of Rome, of which there are no fewer than seven, including that the elite all got lead poisoning from drinking wine out of those pewter vessels!), I find critics are as skeptical toward these as they are toward psychoanalysis. So that one doesn't get us very far, at most taking us past the first hurdle of a simplistic insistence that nothing can be "reasonable" or "empirical" unless based upon laboratory experiments or statistical correlations. We have to admit to the critic that not all psychoanalytic sessions are understandable, and even a session that is on the whole comprehensible has many individual items that remain mysterious, as Freud pointed out. We must also grant the point that physicians and psychologists who have certainly had the relevant clinical experiences—thousands of hours in some cases—have "fallen away" from the Freudian position, and claim to have rational arguments and evidence *from their clinical experience* for doing so. And finally, as a general epistemological point, we have to confess that human ingenuity is great, so that if you have loose enough criteria you can "explain anything."

It may help to ask, "Just *why* is there a problem here?" Why, in particular, do some of us find ourselves caught in the middle between, on the one hand, people who think there is no problem, that we know how to interpret, and that any seasoned practitioner has his kit of tools for doing so; and on the other, those skeptical people (e.g., Sir Karl Popper) who think our situation is conceptually hopeless because of a grossly uncritical methodology of inference? I can highlight the dilemma by examining Freud's jigsaw-puzzle analogy. The plain fact is that the jigsaw-puzzle analogy is false. It is false in four ways. There are four clearcut tests of whether we have put a jigsaw puzzle together properly. First, there must not be any extra pieces remaining; second, there must not be any holes in the fitted puzzle (Professor Salmon has pointed out to me that although this requirement holds for jigsaw puzzles, *provided all the pieces are present*, it is too strong for such cases as a broken urn reconstructed by an archeologist, since a few "holes" due to unfound pieces do not appreciably reduce our confidence in the reconstruction); third, each piece has to fit cleanly and easily into a relatively complicated contour provided by the adjacent pieces; finally, when a piece is fitted "physically," that which is painted on it also has to fit into a meaningful gestalt as part of a picture. Now the first two of these do not apply to the great majority of psychoanalytic sessions; and the second two apply only with quantitative weakening. Combining this point with the variation among psychoanalytic sessions (ranging from sessions almost wholly murky, in which neither patient or analyst can even be confident about a generic theme lurking behind the material, to a minority in which it seems as though "everything fits together beautifully"), we recognize a statistical problem of *selective bias,* emphasizing theoretically those sessions that are, so to say, "cognitively impressive." A good Skinnerian will remind us that the interpreter of psychoanalytic material is on an intermittent reinforcement schedule and that therefore his verbal behavior and his belief system will be maintained, despite numerous extinction trials that constitute potential refuters. The statistical problem presented here is that when, in any subject matter, a large number of arrangements of many entities to be classified or ordered is available, and some loose (although not empty) criterion of "orderliness" has been imposed, then we can expect that even if the whole thing were in reality a big random mess, *some expected subset of sequences will satisfy the loose ordering criteria*. This is one reason why we worry about significance testing in the inexact sciences, since we know that articles with significant t-tests are more likely to be accepted by editors than articles that achieve a null result, especially with small samples.

If you wonder why this problem of selecting an orderly-looking subset of cases from a larger mass arises especially in psychoanalysis, my answer would be that it does not arise there more than it does in other "documentary" disciplines, in which the interpreter of facts cannot manipulate variables experimentally but must take the productions of individuals or social groups as they come, as "experiments of nature," to use Adolf Meyer's phrase for a mental illness. My conjecture is that this is a far more pervasive and threatening problem than is generally recognized. It appears distressingly frequent, to anyone who has once become alerted to it, in diverse domains other than psychodiagnosis, including the nonsocial life sciences, the earth sciences, and in almost any discipline having an important "historical" dimension. To digress briefly, lest this point be misunderstood for want of nonpsychoanalytical examples, I give two.

Philosophers of science have recognized in recent years that the largely nonhistorical, nonempirical, "armchair" approach of the Vienna Circle can be misleading. Most of us would hold that a proper philosophy of science must (as Lakatos has emphasized) combine critical rational reconstruction with tracing out the historical sequence of the growth of knowledge as it actually occurred. Hence we find an increasing use of evidence for or against various philosophies of science from historical examples. I find it odd that so few agree with me that this problem involves a constant danger of selecting one's examples tendentiously. Thus, for instance, my friend

Paul Feyerabend loves to talk about Galileo and the mountains on the moon in order to make a case against even a sophisticated falsificationism; whereas almost any article by Popper can be predicted to contain a reference to the quick slaying (with Lakatos's "instant rationality") of the Bohr-Kramers-Slater quantum theory by a sledgehammer falsification experiment. I don't think I am merely displaying the usual social scientist's liking for doing chi squares (on tallies of practically anything!) when I say that this seems to me an inherently statistical problem, and that it cannot be settled except by the application of formal statistical methods.

A second example occurs in paleontology. Almost any educated person takes for granted that the horse series, starting out with little old terrier-size, four-toed *Eohippus*, is chosen by paleontologists for effective pedagogy, but that there are countless similar examples of complete, small-step evolutionary series in the fossil record—which in fact there are not. I think the evaluation of the fossil evidence for macroevolution is an inherently statistical problem, and is strictly analogous to such documentary problems in copyright law as whether a few bars of music should be considered plagiarized, or a few words of verbal text lifted. (See Meehl 1983.) In the evolution case, given a very large number (literally hundreds of thousands) of species of animals with hard parts that existed for various periods of time; and given the heavy reliance on index fossils for dating rocks (because in most instances neither the radium/lead clock nor purely chemical and geological criteria suffice); it stands to reason that some subset of kinds of animals should, *even if the historical fact of evolution had never occurred at all*, give an appearance of evolutionary development as found in the famous horse series.

We deal here with what I learned in elementary logic to call "the argument from convergence." In the light of a hypothesis H we can see how certain facts f_1, f_2, f_3 would be expected (ideally, would be deduced—although in the biological and social sciences that strict deducibility is rare). So when we are presented with f_1, f_2, f_3 we say that they "converge upon" the hypothesis H, i.e., they give it empirical support. I believe it is generally held by logicians and historians of science (although the late Rudolf Carnap was a heavyweight exception, and I never managed to convince him in discussions on this point) that the argument from convergence in inductive logic (*pace* Popper!) is inherently weaker than the argument from prediction, given the same fact/theory pattern. That is to say, if the facts that support hypothesis H are f_1, f_2, and f_3, let's contrast the two situations. First we have pure convergence, in which the theorist has concocted hypothesis H in the presence of f_1, f_2, f_3 and now presents us with the pure argument from convergence; second, we have a mixed argument from convergence *and prediction,* in which the theorist has concocted hypothesis H in the presence of facts f_1, f_2 and then predicted the third fact f_3, which was duly found. Every working scientist (in any field!) that I have asked about this says that Carnap was wrong and Popper is right. That is, the second case is a stronger one in favor of the hypothesis, despite the fact that precisely the same data are present in both instances at the time of the assessment, and their "logical" relationship to the theory is the same. If the logicians and philosophers of science cannot provide us with a rational reconstruction of why scientists give greater weight to a mixed argument from convergence and prediction than to a pure argument from convergence (given identical fact/theory content), I think they had beter work at it until they can.

The danger of content-implantation and the subtler, more pervasive danger of differential reinforcement of selective intervention, combine here with the epistemological superiority of prediction over (after-the-fact) convergence to urge, "Wait, don't intervene, keep listening, get more uninfluenced evidence." But our recognition of the factor of resistance often argues the other way, as, e.g., to get a few associations to a seemingly unconnected passing association, especially when the patient seems anxious to get past it. We simply won't *get* certain thoughts if we never intervene selectively, and those never-spoken thoughts may be crucial to our theme-tracing. The technical problem posed by these countervailing considerations is unsolved.

In my own practice, I usually follow a crude rule of thumb to avoid an intervention (whether requesting further associations to an item or voicing a conjecture) until I receive at least two fairly strong corroborator associations. If the corroborators are weak or middling, I wait for three. *Clinical example:* The manifest content of a male patient's dream involves reference to a urinal, so one conjecture, doubtless at higher strength in my associations because of his previous material, is that the ambition-achievement-triumph-shame theme is cooking. (Cf. Freud 1974, p. 397, index item "Urethral erotism.") Half-way through the hour he passingly alludes to someone's headgear and suddenly recalls an unreported element of the dream's manifest content, to wit, that hanging on a wall peg in the urinal was a "green hat." This recalls to my mind, although not (unless he is editing) to his mind, a reference several weeks ago to a green hat. The patient had an uncle of whom he was fond and who used to be an avid mountaineer, given to recounting his mountain-climbing exploits to the boy. Sometimes when the uncle was a bit in his cups, he would don a green Tyrolean hat that he had brought back from Austria. The uncle had several times told the boy the story about how Mallory, when asked why he wanted so much to conquer the Matterhorn, responded, "Because it's there." The uncle would then usually go on to say that this answer showed the true spirit of the dedicated mountain climber, and that it should be the attitude of everybody toward life generally. We may choose to classify the passing allusion to a green hat as belonging to the same thematic cluster as this material. Later in the session, if it doesn't emerge spontaneously by a return to that element in the associations, we may decide (how?) to ask the patient to say more about the hat, ascertaining whether he says it was a Tyrolean hat and, even better, a Tyrolean hat "such as my uncle used to wear." I call this a strong corroborator for the obvious reason that the base rate of green-hat associations for patients in general, and even for this patient, is small. That generalization isn't negated by the fact that he *once before* had this thought. Once in scores or hundreds of hours is still a pretty low base rate. But more important is the fact that the sole previous mention is what enables us to link up a green hat with the achievement motive.

On the other hand, the presence of alternative and competing hypotheses tends to lower the corroborative power of our short-term prediction. How much it is lowered depends on how many competitors there are, how good a job they do of subsuming it, and, especially, on the antecedent or prior probability we attach to them. This prior probability is based upon general experience with persons in our clinical clientele but also, of course, upon the base rate for the particular patient. For example, in the present instance the patient, although not an alcoholic, has reported having a minor drinking problem; he has also revealed—although it has not been interpreted—a linkage between alcohol and the homoerotic theme. The uncle's tendency to tell this story when in his cups, and his further tendency to get a little boy to take a sip of beer, produces an unwelcome combination of competing hypotheses.

We have also the possibility, frequently criticized by antipsychoanalytic skeptics as a form of "fudging," that what appear at one level of analysis to be competing hypotheses are, at another level (or one *could* say, "when properly characterized thematically"), not competitors but aspects or facets of a core theme. In the present instance, at one level one might view the major determiner of manifest content about urinals and a Tyrolean hat as being *either* ambition or homoeroticism, and unfortunately the cluster of memories concerning the uncle can allude to both of these competing thematic hypotheses (and hence be useless for predictive corroboration). But we may without artificiality or double talk point out that achievement, especially that which involves marked features of competition with other males, has a connection with the theme of activity vs. passivity, strength vs. weakness, masculinity vs. femininity, the latent fear in males of being aggressively and/or homosexually overpowered by other males. (One thinks here of the ethologists' observations on our primate relatives, in which a "subordinate" male wards off threatening aggressive behavior from a dominant male by adopting the "sexual presenting" posture of females.) It could easily be that the additional associative material in the session is useful to us primarily as a means of separating these linked themes of homosexuality and achievement but, although we recognize their thematic linkage and overlapping dynamic sources, is primarily useful in differentiating the aspects of that common cluster as to what the "predominant" emphasis is at the moment. We do not need to force an arbitrary and psychologically false *dis*connection between the ambition theme and defense against passive feminine wishes and fears in order to ask and, in probability, answer the question, "Is the regnant [Murray 1938, pp. 45-54] wish-fear aspect of the theme today, and in the creation of the dream, that of homosexual passivity or that of competitive achievement ["male aggressiveness"]?"

The mathematical psychoanalysis of the utopian future would be plugging in values, or at least setting upper and lower bounds on values, of three probability numbers—none of which we know at the present time, but which are in principle knowable. The first probability is the prior probability of a particular theme, whether from patients in general or from the patients in a particular clinic or therapist clientele or, as presumably the most accurate value, this particular patient. The second probability number would be the conditional probability going from each competing theme to the associative or manifest content element taken to corroborate it. (It boggles the mind to reflect on how we would go about ascertaining that one! Yet it has some objective value, and therefore it should be possible to research it.) Third, we want the probability of the associative or manifest content item without reference to a particular dynamic source. The reason we want that is that we need such a number in the denominator of Bayes' Formula, and the only other way to ascertain that number is to know the probability of each of those elements on the whole set of competing dynamic or thematic hypotheses, a quantity that is as hard to get at as the second one, and maybe harder.

But there is worse to come. Even the pure argument from convergence gets its strength when the facts that converge upon the hypothesis are numerical point predictions, i.e., facts having low prior probability, an argument that can be made from either a Popperian standpoint of high risk-taking or from the non-Popperian standpoint of the Bayesians—in this respect, the two positions come to the same thing. We have to admit that the deductive model in which H strictly entails facts f_1, f_2, f_3 is not satisfied by psychoanalytic inference, although we can take some comfort from the fact that it is not satisfied in other documentary disciplines either. Freud points out, after discussing the dream work, that it would be nice if we had rules for actually constructing the manifest content from the latent dream thoughts arrived at by interpretation, but that we cannot do this. Hundreds, or in fact thousands, of alternative manifest contents could be generated from the list of latent dream thoughts, plus knowledge of the precipitating event of the dream day that mobilized the infantile wish, plus the stochastic nomologicals (if I may use such a strange phrase; or see Meehl 1978, pp. 812-814, "stochastologicals") that we designate by the terms condensation, displacement, plastic representation, secondary revision, and symbolism. The situation is rather like that of a prosecutor making a case for the hypothesis that the defendant killed the old lady with the axe, when he tries to show that there was a motive, an opportunity, and so forth. That the defendant decides to kill her does not tell us on which night he will do so, which weapon he will use, whether he will walk or drive his automobile or take a taxi, and the like. Whether a Hempel deductive model can be approximated here by designating a suitably broad *class* of facts as what is entailed, and by relying upon probabilistic implication (a notion regrettably unclear), I shall not discuss.

There is the further difficulty that the mind of the interpreter plays a somewhat different role in the argument, either from prediction or convergence, than it does in the physical and biological sciences. In order to "see how" a dream element or an association "alludes to" such and such a theme, one makes use of his own psyche in a way that most of us think is qualitatively different from the way in which we solve a quadratic equation. This is, of course, a deep and controversial topic, and one thinks of the nineteenth-century German philosophers of history who emphasized the qualitative difference between *Naturwissenschaften* and *Geisteswissenschaften*, or the famous thesis of Vico—which sounds so strange to contemporary behaviorist and objectivist ears—that man can understand history in a way he cannot understand inanimate nature, because of the fact that history, being human actions under human intentions, is of his own making, whereas the physical world is not! Whether or not Brentano was correct in saying that intentionality is the distinctive mark of the mental, I think we can properly say that there is a role played in psychological understanding of words and gestures that involves so much greater reliance upon the interpreter's psychological processes and content—the fact of the similarity between his mind and that of the other person—that it would be a case of "quantity being converted into quality." In recent years, the business of intentionality has led some philosophers, especially of the ordinary language movement, to deny that purposes can be causes and especially that reasons can be causes, a view I consider to be a mistake—as I am certain Freud would. (See my paper against Popper on "Clouds and Clocks," Meehl 1970; cf. also Feigl and Meehl 1974.)

In this connection, it is strange that one common objection to idiographic psychoanalytic inferences is that they seem too clever, an objection Fliess once made to Freud. I call attention to an unfair tactic of the critic, a "heads I win tails you lose" approach—unfair in that he relies on the relative weakness of the pure argument from convergence when the facts are limited in number and especially when the facts are qualitatively similar (like replicating the same experiment in chemistry five times, instead of doing five different *kinds* of experiments, which everybody recognizes as more probative); but when presented with a more complicated network in which this epistemological objection is not available, he then objects on the grounds that the reconstruction *is* too complicated! It's a kind of pincers movement between epistemology and ontology such that the psychoanalytic interpreter can't win. If the thing seems fairly simple, it doesn't converge via multiple strands (and hence not strongly), or it can be readily explained in some "nonmotivational" way, as by ordinary verbal habits and the like; if the structure is complex, so that many strands and cross connections exist, tending to make a better argument from convergence than the simple case, he thinks it unparsimonious or unplausible, "too pat," "too cute," to "too clever" for the unconscious to have done all this work.

My response to this pincers movement is to blunt the second, that is, the ontological pincer. I do not accept the principle of parsimony, and I am in good company there because neither does Sir Karl Popper, no obscurantist or mystic he! I see no reason to adopt the "postulate of impoverished reality" (as an eminent animal psychologist once called it). I see no reason to think that the human mind must be simpler than the uranium atom or the double helix or the world economy. Furthermore, the critic contradicts himself, because when he says that the interpreter is attributing to the subject's mind something too complicated for the mind to concoct, he is of course attributing that complicated a concoction to the interpreter himself! It doesn't make any sense to say, "Oh, nobody's head works like that," when the subject matter has arisen because Freud's head, for one, obviously *did* work like that. With this rebuttal, the second pincer is blunted and can be resharpened only by saying that the conscious mind does it; but obviously the unconscious couldn't be that complicated. I cannot imagine the faintest ground for such a categorical denial about a matter of theoretical substance; and it seems to me obvious that there are rich and numerous counter-examples to refute it.

In the experimental literature there is a vast body of non-Freudian research dealing with the establishment of mental sets, with the superior strength of thematic similarity over sound similarity in verbal conditioning, and so forth. Or, for that matter, take the righting reflex of the domestic cat. It was not until the advent of modern high speed photography, permitting a slow motion analysis of the movement, that this amazing feline talent was understood from the standpoint of physics. The cat's nervous system, wired in accordance with what must be an awesome complication in DNA coding, embodies, so to speak, certain principles of mechanics concerning the moment of inertia of a flywheel. As the cat begins to fall, he extends his rear limbs and adducts his forelimbs so that his front section has a small radius of gyration and hence a small moment of inertia, whereas his rear is large in these respects. The torque produced by appropriate contractions of the midriff musculature consequently produces a greater rotation of the forebody, so that the rear twists only a little and the front a great deal. When he is nearly "head up" from this first half of the maneuver, he then extends the forelimbs and adducts the rear limbs and again uses his muscles to apply opposed torques, but now his front has a greater moment of inertia and therefore twists less than the rear. This is an extraordinarily complicated behavior sequence, and nobody supposes that the cat is familiar with differential equations for the laws of mechanics. I therefore meet the second prong of the criticism with sublime confidence, because I know the critic cannot possibly demonstrate, as a general thesis of mammalian behavior, either (a) that the nervous system is incapable of complex computings or (b) that all of its complex computings are introspectable and verbally reportable.

As I pointed out in a previous paper on this subject (Meehl 1970), however, answering a silly objection is merely answering a silly objection, and does not suffice to make an affirmative case. Recognizing the weakness of the jigsaw analogy, and recognizing that complicated inference in the other documentary disciplines, while it might reassure us to know that we are in the same boat with historians and historical geologists and prosecuting attorneys and journalists is not, upon reflection, terribly reassuring in the face of methodological criticism; what are the possibilities for reducing the subjectivity of psychoanalytic inference? I hope it is clear that in putting this question I am not focusing on the possible development of some cookbook mechanical "objective" procedure to be employed in the interview. (See Meehl 1956, 1957.) I still try to distinguish between Hans Reichenbach's two contexts, i.e., the context of discovery ("how one comes to think of something") and the context of justification ("how one rationally supports whatever it is he has come to think of"). A caveat is imperative here, however, if we are to be intellectually honest: Distinguishing between these two contexts, important as it is epistemologically, should not lead to the mistaken idea that there is a clean qualitative distinction between a high probability (and hence, in some sense, "cookbook" process?) relied on, and a low probability in a more idiographically creative one. Freud himself relies upon both; witness his acceptance of Stekel's view that there are more or less standard dream symbols and scattered remarks that claim universality. For example, when the patient says, "I would not have thought of that," the remark can be taken as a quasi-definitive confirmation of the interpretation; or when he says, "My mind is a blank, I am not thinking of anything now," this is almost invariably a violation of the Fundamental Rule with respect to transference thoughts. With this warning, I focus then upon research procedures that aim to reduce the subjectivity, in the special sense that they ought to be rationally persuasive to a fair-minded skeptic who himself has not experienced or conducted analyses, leaving open to what extent such research might add to the list of high-probability "rules of thumb" that already exist in psychoanalytic lore. I list five approaches, without evaluating their merits and without claiming that they are entirely distinct, which they aren't.

First, I am convinced that sheer laziness, aggravated by excessive faith in the received tradition (plus the fact that most practitioners are not research-oriented, plus the lamentable dearth of psychoanalytic protocols), has prevented the application of simple and straightforward nonparametric statistics to test some basic, "first-level" psychoanalytic concepts (not the metapsychology yet). To take my favorite example because of its simplicity, when in 1943 I was seeing a patient (as a graduate student, before my own analysis) and proceeding in a modified Rogersian fashion, the patient reported a dream about firemen squirting water on a burning building. Almost all of the rest of the interview dealt with ambition as a motive and its correlated affects of triumph or shame, which struck me forcibly because it just happened that I had been reading one of Freud's long footnotes on the puzzling "urethral cluster" of urinary function, pyromania, and ambition. Being in those days extremely skeptical of psychoanalytic thinking, I resolved to take note in the future of exceptions to such a crude rule. Now, some forty years later, I can report that I have as yet never found one single exception *among male patients* to the induction that, if the manifest content of a dream deals with fire and water, the dominant theme of the rest of the session will be in the domain of Murray's *n Recognition* (or its aversive correlate *n Infavoidance*) and the associated affects of triumph and elation on the one side or shame and embarrassment on the other. Now such a finding (which I cannot record in the research literature because, stupidly enough, I haven't been keeping affirmative tallies but only waiting vainly to find the first exception) does not exist as a "statistic" because nobody—myself included—has bothered to analyze it systematically. And what I am claiming about this relationship is that it yields a fourfold table with one empty cell—a thing we almost never find in the behavioral sciences. I believe that simple chi squares applied to such intermediate levels of psychoanalytic inference should be computed, and that no new techniques need to be developed for this purpose.

Second, it is possible that the application of existing statistical techniques of various complexity, such as factor analysis and cluster analysis, might be helpful. (Cf. Luborsky's factor analysis of time-series correlations based on Cattell's P-technique, in Luborsky, 1953.) I am inclined to doubt the value of these approaches because I am not persuaded that the factor-analytic model is the appropriate formalism for this subject matter. I think it fair to say that what few studies have been done along these lines have not been illuminating.

A third possibility is the application of new formalisms, kinds of mathematics with which social scientists do not customarily operate, chosen for their greater structural appropriateness to the problems of the psychoanalytic hour. I am particularly open to this one because my own current research is in taxometrics and has convinced me that a psychologist with even my modest mathematical competence can come up with new search techniques in statistics that are superior to those customarily relied on. The taxometric procedures I have developed over the last decade appear, so far, to be more sensitive and powerful than such familiar methods as factor analysis, cluster analysis, hierarchical clustering, and Stephenson's Q-technique. (I do not believe that interdisciplinary exchange between mathematical statisticians and psychoanalytic psychologists is likely to be fruitful unless each party possesses some real understanding of the other one's subject matter, a rare situation.) If someone were to ask me what novel formalisms I have in mind, I don't know enough mathematics to come up with good examples, although graph theory with its nonmetric theorems about paths, nodes, and strands of networks is urged by a colleague of mine. I do not suggest that a really new branch of mathematics needs to be invented, but I would not exclude even that, since I think the social sciences have had an unfortunate tendency to assume that their kind of mathematics has to look like the mathematics that has been so powerful in sciences like chemistry and physics. There have been kinds of mathematics that had no application to empirical scientific questions for long periods, the standard example in the history of science being Galois's invention of group theory in the 1820s, a branch of mathematics that found no application in any empirical science until somebody realized over a century later that it was useful in quantum theory.

A fourth possibility is along the lines of computer programming for complex content analysis that we find in the book by Stone, Dumphy, et al., *The General Inquirer* (1966). In my 1954 monograph on statistical prediction I tried to make a case against T. R. Sarbin and others who imagined that there could be a "mechanical" method for analyzing psychotherapeutic material, at least at any but the most trivial level. I think today we must be extremely careful in setting limits on the computer's powers. Ten years ago computers could play a pretty good game of checkers, and had been programmed to search for more parsimonious proofs of certain theorems in Russell and Whitehead's *Principia Mathema-*

tica; but no computer, although it could obey the *rules* of chess, could play even a passable chess game. Recently in Minneapolis two computers were entered as competitors in our Minnesota Chess Open Tournament, and they did rather well. In 1979 a computer got a draw with an international grand master. On the other hand, machine translation of foreign languages has turned out to be so intractable a "context" problem (so my colleagues in psycholinguistics inform me) that even the Russians have dropped it. I don't really want to push the computer as psychoanalyst. I merely warn you that it would be rash for people like me who do not possess computer expertise to say, "Well, whatever they program the damn things to do, it's obvious they will never be able to interpret dreams." Maybe they will, and maybe they won't.

No doubt one reason for persistence of the Achensee Question is that objectification of psychoanalytic inference, in any form that would be persuasive to psychometricians, behaviorists, and other social science skeptics who have scientifc doubts about the validity of the psychoanalytic interview as a data source, would presumably rely on well-corroborated *quantitative* lawlike statements from several disciplines that are themselves in their scientific infancy. Since psychoanalysis deals mainly with words, the most obvious example of such a related discipline would be psycholinguistics, whose conceptual and evidentiary problems (I am reliably informed by its practitioners) are not in much better scientific shape than psychoanalysis itself. But cognitive psychology more broadly —the psychology of imagery, the general psychology of motivation, the social psychology of the psychoanalyst as audience, not to mention the recent non-Freudian experimental work on the mental and cerebral machinery of dreams—would all have to be put together in some utopian integrated whole before it would be possible to write the sort of nomological or stochastological (Meehl 1978, pp. 812-814) relations that would generate any sort of plausible numerical probabilities for the inferences. I am not here lamenting lack of exact numerical values; I am referring rather to the difficulty of expressing crude forms of functional dependencies that would yield any numbers at all. Suppose the "objectifier" requires a kind of evidentiary appeal *more explicit* than, "Well, just look at these clusters and sequences of speech and gesture. Don't you agree that they sort of hang together, if you view them as under the control of a certain (nonreported) guiding theme?" I am not accepting here that it is imperative to meet such a standard of explicitness. I am simply reacting to the social fact that most objectifiers would not be satisfied with less. Now since we don't have a good *general* psychology of language, of imagery, of dreams, of short-term fluctuations in state variables like anxiety or anger or erotic impulse, (and obviously we cannot turn to psychoanalysis itself for that when the Achensee Question is before us), then it is almost pointless for anyone in the present state of knowledge to even speculate utopianly about how such a psychology might look. We do not even know whether the kinds of mathematics favored among social scientists would be appropriate. Thus, for instance, it may be that the mathematical formalisms of factor analysis (Harman 1976) or taxometrics (Meehl and Golden 1982) are much less appropriate for tracing themes in the psychoanalytic interview than, say, something like graph theory (Read 1972 pp. 914-958) or finite stochastic processes (Kemeny, Snell, and Thompson 1957). All of this is music of the future, and I shall not discuss it further.

Does that mean that we have to put the whole thing on the shelf for another century, pending satisfactory development and integration of these underpinning disciplines? One hopes not, and let me try to say why that *may* not be required (although it may!). When we think about the relationship of a mathematical psycholinguistics to a mathematical science of short-term motivational state variables, we are fantasizing a rather detailed prediction (or explanation) of the analysand's choice of words and their sequence. Not perhaps the specific word, which is likely to be beyond the power of utopian psycholinguistics, just as the prediction of precisely how a collapsed bridge falls today is beyond our advanced science of physics; but at least a kind of intermediate-level analysis of the verbal material. Suppose we can bypass that by permitting a somewhat more global categorization of the patient's discourse, allowing for a kind of global subsumption under motivational themes by some kind of partly objective, partly subjective procedure of the sort that I discuss in the research proposal below. That is, we aren't (pointlessly?) trying to predict exactly which verb, or even which class of active or passive verbs, will be emitted during a specified small time interval of the session, having been preceded by a narrowly specified number of allusions to the "mother theme," or whatever. What we are saying is that we have a small collection of rather broadly identified remarks (or gestures or parapraxes), and we hope to persuade the skeptic (assuming we have convinced ourselves) that it is more reasonable to construe this small set of happenings as produced by one dynamic state or event—I shall simply refer to *a theme* in what follows—rather than to reject that possibility in favor of multiple separate psychological hypotheses that are unrelated dynamically or thematically, but each of which might easily be capable of explaining the *particular* individual remark, gesture, or parapraxis that is treated as *its* explanandum.

In several places Freud likens the analytic detective work to that of a criminal investigation, and the analogy is a good one on several obvious counts. We do not consider the prosecutor's summation of evidence to the jury, or the judge's standardized instructions on how to assess this evidence, as somehow irrational or mystical or intuitive, merely because it is not possible to express the invoked probabilistic laws *in numerical form*, let alone ascribe a net joint "empirical support" *number* to the total evidence. As I have said elsewhere in this essay, there are other disciplines that we consider intellectually respectable and worth pursuing and, in certain important private and social decision makings, even deserving of our reliance in grave matters, despite the absence of an effective procedure (algorithm) for computing a numerical probability attachable to outcomes, or to alleged explanations of events that have already taken place.

I don't intend to get much mileage out of those nonpsychoanalytic examples. But it is important, before we proceed, to recognize that the domain of the rational and empirical is not identical with the domain of the statistical-numerical, as some overscientized psychologists and sociologists mistakenly believe. What reservations must be put by a rational skeptic on the force of such extrapsychoanalytic analogies I discuss elsewhere herein; and I agree with the skeptics that those reservations are discouragingly strong. But the point is that we can and do talk about evidence converging, of explanations as being parsimonious or needlessly complicated, and so forth, with an idealized inductive logic (if you don't like that phrase, an idealized empirical methodology), including one that accepts a particular *kind of inferential structure*—say, a Bayesian inference—even though, in the concrete domain of application, we cannot plug in any actual *probability numbers*. If we do hazard mention of numerical values—and this is an important point one should never forget in thinking about psychoanalysis and allied subjects—at most we have some upper and lower bounds, acceptable by almost all members of our clinical and scientific community, on the numerical values of expectedness, priors, and conditionals in Bayes's Formula. I think it is generally agreed that we can "think Bayesian" and, thinking Bayesian, can come up with some reasonable numerical bounds on likelihood ratios and posterior probabilities, without claiming to have determined relative frequencies empirically as the initial numbers that get plugged into Bayes's Theorem. (See Carnap's discussion of the two kinds of probability, Carnap 1950/1962, passim.) For someone who is uncomfortable even with that weak a use of a widely accepted formalism (which is, after all, a high-school truth of combinatorics and does not require one to be a "strong Bayesian" in the sense of current statistical controversy), at the very least one can point to a list of separate causal hypotheses to explain half a dozen interview phenomena and then to a single causal hypothesis—the psychoanalytic one—as doing the job that it takes half a dozen others to do as its joint competitors.

Example: A patient drops her wedding ring down the toilet. In speaking of her husband, Henry, she mistakenly refers to him as George, the name of an old flame of hers. An evening dining out in celebration of their wedding anniversary was prevented because the patient came down with a severe headache. Without any kinds of challenge to the contrary, she "spontaneously" makes four statements at different times in the interview about what a fine man her husband is, how fortunate she is that she married him, and so on. Now you don't have to go through any elaborate psycholinguistics or even any of that "within-safe-bounds" application of Bayes's Theorem, to argue that it may be simpler and more plausible to attribute these four phenomena to the unreported guiding influence of a single psychological entity, namely, some ambivalence about her husband, than to deal with the four of them "separately." In the latter case we would be, say, attributing the wedding ring parapraxis to nondynamic clumsiness (the patient happens to be at the low end of the O'Connor Finger Dexterity Test); the anniversary headache to insufficient sleep and oversmoking; the misnaming George-for-Henry to the fact that old George was in town recently and called her up; and the unprovoked overemphasis on her happy marriage to some recent observations on the unhappy wives that are her neighbors on either side. Setting aside the independent testing of those alternatives, it's basically a simple matter. We have four competing hypotheses whose separate prior probabilities are not much higher than that of the marital ambivalence hypothesis, although some of them might be a little higher and others a little lower. We think that the conditional probabilities are also roughly in the same ball park numerically as the conditional probability of each of the four observations upon the ambivalence hypothesis. The argument that one would make if he knew nothing about statistics, Bayesian inference, or inductive logic but did know legal practice, or common sense, or diagnosing what's the matter with somebody's carburetor, would be: "We can easily explain these four facts with one simple hypothesis, so why not prefer doing it that way?"

It is not difficult to tighten this example up a bit and make it semi-formal, to such an extent that it is the skeptic who is put on the spot—provided, of course, that he will accept certain reasonable bounds or tolerances on the estimated numbers. Thus, suppose the average value of the four priors is not greater than the prior on the ambivalence hypothesis; and suppose the average value of the four conditionals required to mediate an explanation of each of the four observations is not greater than the average conditional of the four observations on the ambivalence theory. Since the "expectedness" in the denominator of Bayes's Theorem is some unknown but determinate true value (however we break it up into the explanatory components associated with the possibilities), and since a dispersion of four probabilities yields a product less than the fourth power of their average, then when we compute a likelihood ratio for the ambivalence hypothesis against the conjunction of the separate four (assuming these can be treated as essentially independent with respect to their priors, quite apart from whether they are explanations of the four explananda), things cancel out, and we have a ratio of the prior on the ambivalence hypothesis to the product of the other four priors. If, as assumed above, the dynamic hypothesis is at least as probable antecedently as the other four average priors, a lower bound on this likelihood ratio is the reciprocal of the prior cubed. So that even if the priors were all given as one-half—an unreasonably large value for this kind of material—we still get a likelihood ratio, on the four facts, of around eight to one in favor of the psychodynamic construction.

Before setting out my fifth (and, as I think, most hopeful) approach to psychoanalytic theme-tracing, it will help clarify a proposed method to say a bit more about theory and observation, at the risk of boringly repetitious overkill. I think our characterization of the theory/observation relation is especially important here because (a) both critics and defenders of psychoanalytic inference have tended to misformulate the issue in such a way as to prevent fruitful conversation, and (b) the pervasive influence of the antipositivist line that all observations are theory-infected (Kuhn, Feyerabend, and Popper) lends itself readily to obscurantist abuse in fields like psychopathology. Having mentioned Popper in this connection, I must make clear that I do not impute to him or his followers any such abuse;

and, as is well known, Popper himself is extremely skeptical about the alleged scientific status of psychoanalytic theory.

As a starter, let us be clear that there are two methodological truisms concerning the perceptions and subsumptions (I think here the line between these two need not be nicely drawn) of "experts," in which the expertise is partly "observational" and partly "theoretical." One need not be appreciably pro-Kuhnian, let alone pro-Feyerabendian, as I most certainly am not, to know that technical training, whether in the methods of historiography or electron microscopy or psychodiagnosis or criminal investigation, enables the trained individual to perceive (and I mean literally *perceive,* in a very narrow sense of that term that would have been acceptable even to Vienna positivism) things that the untrained individual does not perceive. Anybody who has taken an undergraduate zoology class knows this; it cannot generate a dispute among informed persons, whatever their views may be on epistemology or history of science. On the other hand, it is equally obvious, whether from scientific examples or everyday life, that people tend to see and hear things they expect to see and hear, and that, given a particular close-to-rock-bottom "perceptual report," the expert's theoretical conceptions will affect under what rubric he subsumes the observation. These truisms are so obvious that my philosophy-trained readers, who comfortably accept both, will be puzzled to learn that the situation is otherwise in psychology; but it is. Psychoanalytic clinicians have become accustomed to relying on analogies of psychoanalytic method to the microscope or telescope or whatever, together with the alleged combination of sensitization to unconscious material and reduced defensive interferences, supposedly producing *some* variant of "objectivity," consequent upon the analyst's personal analysis and the corrective experiences of his control cases. Nonpsychoanalytic clinicians, especially those coming from the behaviorist tradition, fault the analytic theme-detector for failing to provide "objective operational definitions" in terms of the behavior itself, of constantly going behind the data to concoct unparsimonious causal explanations, and the like. The same polarization is true for nonpractitioners, i.e., academic theoreticians with psychoanalytic versus antipsychoanalytic orientations. In these disputes, when the parties are not simply talking past each other (the commonest case), the difficulty is that each thinks that his methodological principle clashes with his opponent's methodological principle, which is almost silly on the face of it. Consider the following two statements:

M_1: "It takes a specially skilled, specially trained, clinically sensitized, and theoretically sophisticated observer to notice certain sorts of behavioral properties and to subsume them under a psychodynamically meaningful rubric."

M_2: "Training in a special kind of observation predicated upon a certain theory of (unobserved) states and events underlying the behavior being observed may sometimes have the result, and presents always the danger, of seeing things that aren't there, or subsuming them in arbitrary ways, or forcing a conceptual meaning upon them that has no correspondence to the actual causal origins of the behavior observed."

These two methodological assertions M_1 and M_2 are perfectly compatible with each other, as is obvious so soon as we state them explicitly. In fact, in large part each of them flows from the *same* imputed characteristics of the "sensitive" and "insensitive" observer! They just aren't in conflict with one another as assertions. Why would anyone have supposed they were, or (better), argued as if he supposed that?

The problem is that they are in a kind of pragmatic conflict, even though they are logically and nomologically compatible, in that there is a tension generated in anyone who accepts both of them, between his aim to discover the nonobvious (an aim pursued by reliance on M_1) and his aim to avoid theoretically generated self-deception or projection (an aim aided by remembering M_2). This epistemic tension between two aims that are both reasonable and legitimate is not, I suggest, any more puzzling than some better-understood cases, such as the fact in statistical inference that one has tension between his desire to avoid Type I errors (falsely inferring parameter difference from the observed statistical trend) and Type II errors (wrongly sticking with the null hypothesis of "no difference" when there is a difference in the state of nature.) There isn't any mathematical contradiction, and under stated and rather general conditions, one uses a single coherent mathematical model to compute the trade off and assign it numerical values. Nor is there any sort of philosophical collision or semantic confusion. It is simply a sad fact about the human epistemic situation, even for a fairly developed and rigorous science like mathematical statistics, that when we wish to apply it to such a simple task as detecting bad batches of shotgun shells, we experience a pragmatic tension, and that tension has, so to speak, a realistic (not a "neurotic") basis.

Similarly, in dealing with material of appreciable complexity such as the stream of speech and gesture produced by an analysand, if the observer (NB: *noticer, attender*), classifier, and interpreter lacks certain kinds of training and expertise, he won't be able to do the job. But if he does have those kinds of training and expertise, he may be seduced to do a job that is too good. So the first thing one has to do in thinking rationally about this problem is to get away from the pseudo-collision of Principles M_1 and M_2, to wholeheartedly and unreservedly accept both of them, recognizing that the two principles are logically consistent and even flow from the same facts about the human observer and interpreter and the effects of his training and experience. They lead to a pragmatic tension generated by our reasonable desire to avoid two kinds of errors which might be described as errors of omission versus commission, errors of "under-discovery" versus "over-belief," or even William James' famous errors of the tenderminded and the toughminded. One thinks of James' comment on William Kingdon Clifford's stringent "ethics of belief," to the effect that Professor Clifford apparently thought that the worst possible fate that could befall a man was to believe something that wasn't so!

Excursus on Observations

Because of some current tendency in psychology to rely on the Kuhn-Feyerabend "theory-laden" doctrine, I shall permit myself here a few general remarks on the controversy about meaning invariance and theory ladenness, because it seems to me that the situation in psychology is importantly different from the favorite examples employed by philosophers and historians of science in discussing this difficult and important issue. First, the theory-ladenness of observational statements and the associated meaning-variance is most clearly present and most important in what Feyerabend calls "cosmological theories," i.e., theories that say something about everything there is (as he once put it to me in discussion). I believe a psychologist can be seduced into attributing undue importance to the Kuhn-Feyerabend point if he takes it as a matter of course that philosophical arguments concerning meaning variance and theory-ladenness applying to the Copernican hypothesis or relativity theory or (less clearly?) quantum mechanics, apply equally strongly *and equally importantly* to rats running mazes or psychoanalytic patients speaking English on the couch. I recall a Ph.D. oral examination in which the candidate, a passionate and somewhat dogmatic Kuhn-Feyerabend disciple, was asked by one of my philosophy colleagues to explain just how the protocol "Rat number 7 turned right in the maze and locomoted to the right-hand goal box" was theory-laden; in what sense the experimenter would experience a perceptual gestalt-switch if the experiment converted him from being a Tolmanite to being a Skinnerian; and which of the words in the protocol sentence would undergo a meaning change as a result of his theoretical conversion? It was a good question, and our surprise was not at the candidate's utter inability to deal with it, but the extent to which he was prepared to go in a hopeless cause. What he said was that the very genidenity of rat number 7 from yesterday's trial run to today's test run, and the very fact that we called today's run a "test run," were theory-laden. (I think this is pathetic, but if there are readers who don't, I won't press the point.) Of course the "theory" that today's rat is genidentical with yesterday's rat, is not a psychological theory in any sensible or interesting use of the word "psychological," and even if it were, it is firmly believed (I should say presupposed) by both Tolman and Skinner as well as all of their followers, including those that are in transition to the opposing paradigm. If one wants to say that the ordinary common-sense world view that macro-objects as described in Carnap's "physical thing language" are spatio-temporally continuous constitutes a kind of theory, I have no strong objection to this, although I don't find it an illuminating way to speak. But the point is that if it is a theory, it is a theory that cuts across psychological theories of animal learning. And, of course, it's for that reason that the candidate was unable to point to any of the words in the sentence that would have undergone a meaning change as a result of the experimental psychologist's conversion from one theory of learning to another.

The question whether theory defines the concept "test day" is another piece of obscurantism, since, although the theory is what *leads us to make a certain test* (not in dispute), this fact is quite incapable of showing meaning variance or theory-ladenness of the behavioral terms describing what the rat *does* on the test. Witness the fact that an undergraduate with correct 20/20 vision and normal hearing, who is nonpsychotic and familiar with the macro-object English terms "right" and "left," could be safely trusted to make the relevant observations even if he knew nothing about the latent learning concept or about differences between the rat's condition (hungry? anxious?) this night and on previous nights. In most latent learning experiments, the question about test (or critical pre-test) occasions "How is this night different from all other nights?" has its answer either in the precedent *operations* of feeding (or fasting), or what, if anything, is present in the goalbox that wasn't there on previous occasions.

In psychological research on behavior, whether animal or human, the theory-dependence of observations and the theory-ladenness of operational meanings involve a cluster of questions that are related but clearly distinguishable, as follows:

(1) Instruments of observation are usually theoretically understood, whether in the inorganic or life sciences. (Not quite always, e.g., the Wasserman test!). Such instruments are used (a) to control all causally influential variables, (b) to extend the human sensorium, (c) to dispense with human perception by substituting a physical record for the human sense-report, and (d) to replace human memory by a more reliable record (storage and retrieval). In physics and astronomy (I do not know about chemistry, but I imagine there also) there is sometimes a theoretical question concerning the extent to which the instrument can be relied upon for a certain experimental purpose, because the laws of physics involved as auxiliaries in the test proposed are themselves "connected," via overlapping theoretical terms, with the conjectured laws (including dispositional, causal, and compostional properties attributed to conjectural entities) that the experiment is designed to test. There are problematic entities whose role in the nomological net would be altered importantly (for the experiment) if certain other adjacent regions of the net were altered in such-and-such respects. This may happen in psychology also, but it is not as common in good reséarch as those who emphasize theory-ladenness seem to assume. Whether the observing instrument is the unaided human eye, the kymographic recording of a lever press, or a photo cell showing which alley the rat passed through, these are all instruments that rely on no currently competing theory of animal learning. For that matter, no reasonable person believes their deliverances are dependent upon a theory of how the rat's brain works in choosing which direction to turn in a T-maze.

(2) Auxiliary theories, and hence auxiliary particularistic hypotheses formulated in terms of these theories, may be problematic but essentially independent of the subject matter under study. An example would be an auxiliary theory about the validity of the Rorschach Ink Blot Test as a detector of subtle cognitive slippage in schizoidia. We might wish to rely on this in testing the dominant schizogene theory of that disorder, but it is an auxiliary theory that is highly problematic and, in fact, as problematic as

the major substantive hypothesis under test. (Meehl 1978, pp. 818-821) A less problematic auxiliary theory used in listening to psychoanalytic session discourse would be the vague (but not empty) "theory" that speech is determined by interactive causal chains between the current stimulating field (is the analyst moving restlessly in his chair?) and the subject's inner states and dispositions, whether those latter are characterized in brain language (not presently available except for very rough statements of certain systems or regions of the brain that are more relevant to verbal behavior than, say, to keeping one's balance when standing up) or in the molar language (whether behavioristic or phenomenological) again. We tacitly presuppose the "theory" that people cannot speak a language they have not learned. Suppose I know that a patient is a classics professor, and that his wife's name is Irene. Then the possibility that a dream about a "peace conference" conjoined with an association during the session about "knocking off a piece" is connected with his wife, is plausible only because of our tacit auxiliary theories about language. If he didn't know any Greek, he could still link in his unconscious the homophones "peace" and "piece," but he would not link that with the name of his wife because he wouldn't know the etymology of the female name "Irene." (NB: If the therapist doesn't know that amount of Greek, he won't be able to create the thematic conjecture either!)

(3) *Did* this particular investigator design the experiment in the light of the theory he was interested in testing (a biographical question of fact)?

(4) *Could* a seasoned, clever researcher have designed this experiment without the disputed learning theory in mind—perhaps without any learning theory in mind except the minimal nondisputed statement (hardly a technical doctrine of animal psychology) that organisms usually have a knack to "learn things?"

(5) Granted that a purely atheoretical, "blind inductive" ethologist *could* have designed a certain experiment, is it *likely* that he would have designed this particular one and chosen to observe the things he chose to observe, absent this theory? Ditto, absent some other theory about learning?

(6) Whatever has led, or plausibly could lead, to the designing of the experiment and the specification of what was to be observed on the test night, *could* a theoretically naive person with normal sensory equipment make and record the observations without the theory?

It is a grave source of confusion to lump all these questions together by the single seductive statement, "Observations are theory-laden." One can make entirely too much of the simple fact that people's visual and auditory perceptions, and their imposition of a spatial or other reference frame upon what comes to them through distance receptors, is always influenced by their implicit beliefs about the world (genidentity of macro-objects, etc.), their previous experiences, their verbal habits, and their culture's dominant interests and values. Nobody today holds—what one doubts even our philosophical ancestors held—that the favorite epistemological prescriptions of Vienna positivism can be founded upon theorems in formal logic, as if we were to pretend we did not know that we are organisms, occupying space-time, having distance receptors, wired somewhat similarly to one another, able to remember, to speak, and so forth. The agreement among scientists (or critical, skeptical, tough-minded contemporary nonscientists) observing a witchcraft trial as to which witnesses were believable, which sense modalities were generally reliable, could not be achieved without experience of the human mind and society. Does anybody dispute this? On the other hand, these kinds of minimal "epistemological basics" are part of our general theory of macro-objects, formulable in Carnap's "physical thing language," and our knowledge of human beings as observers, recorders, rememberers, and reporters.

These shared, common-sense, well-tested notions are *not* "theories" in the interesting and complex sense we have in mind when we talk about constructing a satisfactory psychology of perception, or an adequate psychology of cognition, or a sound descriptive (non-normative) theory of rat decision making. It is not perfectionist epistemology or vulgar positivism—nor, I think, even an antipathy to ghosts, leprechauns, and capricious deities (not to mention fortune tellers and other epistemological disreputables)—that we form the societal habit of employing instruments of observation and recording as often as we do in the sciences whether these sciences deal with organic or inorganic subject matter. I must confess I see nothing complicated, philosophically earthshaking, or especially interesting about the fact that since humans are not always accurate in noticing whether a visual or auditory stimulus occurred first, one has a problem in such cases about relying on human observation For that reason one substitutes nonhuman instruments whose subsequent deliverances to the human perceiver are of a different form, a form chosen by the instrument maker and the scientist who wanted the instrument built so as to be less ambiguous perceptually. (Incidentally, the notion that we cannot do any basic epistemology or formulate any workable methodology of science prior to having respectably developed sciences of perception, cognition, sociology of knowledge, etc., seems very odd to me when I reflect that astronomy, chemistry, physics, and physiology were well advanced sciences, relying on a developed "method" that made them so superior to pre-Galilean thought, long before psychology or sociology were even conceived as scientific disciplines.)

Do we really have to write a big book about this? Maybe for a critical epistemology of astrophysics, or quark theory; but not, I urge, for animal conditioning, or classical psychometrics, *or even psychoanalysis*. In N.R. Campbell's (today underrated) *Physics: The Elements* (1920/1957) he has a nice section (pp. 29-32) about judgments for which universal assent can be obtained. He is of course exaggerating somewhat when he refers to these judgments as being "universal," and one supposes that even if we eliminate known liars, psychotics, persons with aberrant vision and hearing—things which can be independently tested—we still might have a minuscule fraction of observers who would puzzlingly persist in making judgments out of line with the rest of us. Note that even in such extremely rare cases, it is usually possible without vicious ad hockery to give a satisfactory *causal* explanation of why the incurably deviant response is made. But let's admit that it is not always so. Nothing in the method of science requires that we should come to a quick decision in such cases. What happens is that (a) we get a steady accumulation of protocols from the reliable observers (calibrated in the past in other contexts); (b) we are able to give a satisfactory explanation, again without vicious ad hockery, of the discrepant observer's findings; so (c) we decide to "pull a Neurath" (Neurath 1933/1959) by simply refusing to admit the aberrant protocol into the corpus. We prefer to rest decision (c) on a conjunction of (a) and (b), but if (a) is sufficiently extensive and varied, and the theory forbidding the aberrant protocol is doing well enough, we will dispense with (b)—while keeping our hopes up. Thus, e.g., physicists dealt "Neurath-style" with the irksome Dayton C. Miller ether-drift protocol for some 30 years before Shankland and colleagues provided an acceptable (b). But I think it unfortunate that psychologists do not at least read and reflect upon Campbell's discussion or that of an undervalued contributor—C.R. Kordig (1971a, 1971b, 1971c, 1973) before jumping on the popular Kuhn-Feyerabend bandwagon. Campbell sets up three kinds of perceptual judgments on which universal assent could be attained among normal calibrated observers—to wit, judgments of simultaneity, consecutiveness, and betweenness in time; judgments of coincidence and betweenness in space; and judgments of number. Now this is a pretty good list. Everyone knows that the plausibility of Eddington's famous statement "science consists of pointer readings" (wrong, but not a stupid remark at the time) arises from the very large extent to which pointer readings of instruments substituted for the human eye and ear at the first interface with the phenomenon under study have replaced the human eye and ear as Aristotle and Pliny used them. Again, I don't see anything mysterious about this, and it flows directly from Campbell's principles. That a pointer lies *somewhere between* the line marked "7" and the line marked "8" on a dial is an example of spatial betweenness; that this may be an observation made *during* a few seconds interval during which a single tone was being sounded is an example of (local grammar) temporal coincidence. Now whenever we find that most people's theoretical notions unduly affect consensus in perceiving alleged N-rays (or auras, or levitations)—not to mention "anxiety" in the rat or "latent hostility" in the psychiatric patient—we have recourse to some strategem in which we try to achieve an equivalent of the physicist's pointer reading *without throwing away perceptual input that is relevant*.

I cannot insist too strongly that the raw data of the psychoanalytic hour are speech, gesture, and posture *and*—unless the analyst claims to have telepathic powers—*nothing else*. If my psychodynamic theory allows me to say that I "*observed* the patient's hostility" when the existence of the patient's (latent, nonreported, denied) hostility is what is under dispute between me and, say, a certain kind of behaviorist, then my observations are here theory-infected to an undesirable extent, and I am scientifically obligated to move down in the epistemic hierarchy closer to the behavior flux itself (MacCorquodale and Meehl 1954, pp. 220 ff). If I refuse to do this, or at least to concede the epistemic necessity to do it (given your denial and request for my evidence), then I may be a perceptive analyst and a skillful healer, but I have opted out of the game "science." We have now reached little-boy impasse, "My Dad can lick your Dad," "Can not!", "Can so!" "Can not!," etc., irresolubly ad nauseam.

Now the rock-bottom data in the clinical example below I shall confine to the patient's speech and, in fact, to features of the speech that can be fairly adequately represented in the transcript without a tape, although everyone knows "something is lost" thereby. Thus pauses, gross fluctuations in rate of speech, and volume level can be measured objectively from the tape and indicated by a suitable notation in the transcript. We are, however, going to rely almost wholly upon the content, because that's what my proposed theme-tracing procedure deals with. (One cannot deal with everything at once!) In a way, our problem of tradeoff between errors of omission (insensitive, untrained listener not perceiving) and errors of commission (theoretically biased listener projecting) can be stated quite simply in the light of what was said above. Stating it thus leads fairly directly to my theme-tracing proposal. The problem is this: Restricted segments of the verbal output (blocks, hereafter) can be subsumed under a variety of thematic rubrics, and which rubric it is subsumed under by a psychoanalytically sophisticated listener or protocol reader will, of course, be affected by his tentative subsumption of the other blocks, all guided by his acceptance of the minimal Freudian theory. Now this interblock influence is what we want to get away from if possible, because we are sensitive to the skeptic's objection that "human ingenuity can fit almost anything together if you're not too fussy," based upon the several lines of criticism set out above. We want the clinician to use his psychodynamically sophisticated mind to achieve the subsumption of a verbal block. Yet we would like the conclusion of his subsuming activity to have as a work product something that cannot be easily defeated by the skeptic's "Well, so what? It's all very clever, you do make it sound as if it hangs together; but I am not impressed."

It is not that we think that a proper method of protocol analysis should be *coercive* with respect to a sufficiently determined skeptic, which it is foolish to require. We do not require that "good arguments" for the roundess of the earth be capable of convincing the (still surviving!) Flat Earth Society members. No, the problem is not that we foolishly seek an automated truth-finding machine, some kind of algorithm for particularistic inductive inference in this kind of material, which very likely cannot be come by and is not anticipated even for the utopian phase of psycholinguistics or psychodynamics. Rather the problem is that we ourselves, "pro-Freud leaners" with scientific training and epistemological sophistication, are fully aware of the merits of the skeptic's complaint. That being so, our aim is to use the clinically sensitive mind of the psychoanalytically trained listener or reader to discern the red thread running through the discourse, to carry out the theme-tracing that we are reasonably sure could not be done by a competent clerk or a non-Freudian psycholinguist (Meehl 1954, Chapter 6, 7 and passim). But we would like those red thread discernings to be at what might be called an intermediate level of complexity and persuasiveness, so that given the thematic subsumption of blocks of the discourse, the preponderance or strength of allusions to the "segments of red thread" recurring in the various blocks will speak for themselves when we add them up across blocks. That is what the proposed method of theme-tracing tries to do.

So I present this somewhat simple-minded approach, which involves no fancy mathematics but which attempts to do partial justice to Freud's notion of the "red thread of allusions" running through the patient's material. The approach appeals to me because it combines the advantages of the skilled, empathic, thematically perceptive clinical brain with at least partial safeguards against contamination effects. Let's put the question thus: "How can we make use of the classificatory powers of the skilled mind without that mind's classifications being contaminated by theory, hence rendering the whole process especially vulnerable to the Achensee Question?" We want to use the clinician's brain to do something more complicated than associate tables with chairs or plug in *mother figure* for "empress," or, idiographically, to search his memory bank for previous references by this patient to green Tyrolean hats to find that they always relate to the uncle. But if we start using the skilled clinical mind to do something more interesting and complicated than these jobs, then comes the critic with the Achensee objection, telling us that we are brain-washed Freudians and therefore we naturally read our thoughts into the patients and "see" meanings, metaphors, and subtle allusions when they are not there.

To sharpen somewhat the distinction between the first method (doing a simple chi-square on a four-fold table showing the association between an objectively, i.e., clerically scorable dream content on the one hand and a subjective, impressionistic, skilled clinician's uncontaminated discernment of the theme in the subsequent associations on the other), and the fifth method, which I shall christen "Topic Block Theme Tracing," I choose a short clinical example that is in most respects as straightforward as the fire/ambition example, but *not quite*—and the "not quite" introduces terrible complexities for objective scoring, even by such a clever device as the computerized General Inquirer. A patient begins the session by reporting: *I dreamed there was a peculiar water pipe sticking into my kitchen*. My Radovian training suggests a minor intervention here, for clarification only, so I ask, "Peculiar?," to which the patient responds, "Yes, it was a peculiar water pipe because it seemed to have some kind of a cap on it, I couldn't understand how it could work." The standard symbology here [*waterpipe* = penis, *kitchen* = female genitals] is familiar to undergraduates, but knowing it would only permit the trained clerk or the supertrained clerk (e.g., the General Inquirer) to infer a heterosexual wish. What makes it interesting is the "peculiar cap," juxtaposed with the word "work" [= coitus, at least semi-standard]. Here an idiographic low-frequency consideration enters our minds, mediated by the fact that the patient is Jewish and I am gentile. I conjecture that the capped pipe is an uncircumcised (i.e., gentile) penis, and that the dream expresses an erotic positive transference impulse. I further conjecture (more tentatively) that the current manifestation of these transference feelings involves negative feelings towards her husband, unfavorable fantasied comparisons of me with him, and that the focus of these invidious comparisons will be something in the Jewish/gentile domain. Except for the one word, "Peculiar?," I remain silent until the last five minutes of the session. Everything the patient talked about during that period alluded directly, or almost directly, to the conjectured theme. Space does not permit me to present all of the associations, but to give you an example: She recounted a recent episode in which she and her husband visited a drugstore with whose proprietor the husband had formerly done business, and the patient was irritated with her husband because he slapped the counter and put his hand on the druggist's shoulder and asked in a loud voice how his profits were going. The patient noted the presence in the store of a slightly familiar neighbor woman named Stenquist, who the patient mentions is a Norwegian Lutheran. (She knew from the newspapers and other sources that I was a Lutheran and of Norse origins.) She had the conscious thought in the drugstore that her husband was "carrying on in exactly the way anti-Semites have the stereotype of the way Jewish people talk and act in public." She then talked about a non-Jewish boy she had gone with briefly in high school but quit because her parents disappoved, emphasizing that he was "quiet" and "somewhat shy" and had "very nice manners." She went on to say she liked men who were gentle (note further phonetic link between "gentle" and "gentile"), and after a bit of silence said that she realized it was my business to be gentle in my treatment of her but that she imagined I was the same way in real life. Some more hesitation, then a complaint that sometimes her husband was not gentle in bed; and then finally a reluctant expression of the thought that I would no doubt be gentle in bed.

Now this example presents only minor difficulties for an objective classification of the associative material, since I have picked out material that illustrates the theme. But in the "figure-ground" situation they are less clear, and one *could* miss the point without having his switches properly set by the symbolized uncircumcised penis in the manifest content of the dream. And although it is dangerous to impose limits on what the souped-up computer programs of the future will achieve, it will take some doing on the part of the content analyst who writes the General Inquirer's "dictionary" to deal with this case. That one (low-frequency) idiographic element, a manifest dream element that I have never heard before or since in thousands of hours of therapeutic listening, and that no one in any psychotherapist audience I have asked has had—that would mean a minimum of 100,000 hours of our collective experience—makes all the difference. It is for this reason that we need the psychoanalytic retrieval machinery of our own minds, i.e., that we are still the superprogrammed General Inquirers.

From this example it is only one step to cases in which the manifest content contains little of the received symbology, but is itself idiographic to such a degree that its meaning becomes evident only as we listen to the associations in the ensuing session. So we have a situation in which one cannot see a particular "fact" as bearing upon a particular conjecture except in the light of the conjecture itself. Crudely stated, we don't know that a fact is of a certain kind without knowing what it means, and in the work of the mind we don't know what it means without having purposes and intentions available as potential "explainers." In psychoanalytic listening we impose the relevant dimensions of classification on what, for the behaviorist, are just noises (of course he doesn't really proceed that way, but he pretends to, and tries to impose this impossible methodology upon us!) Our "imposing of relevant dimensions"—the important truth in the Kuhn-Feyerabend line as it applies to psychoanalysis—is precisely what makes us vulnerable to the Achensee Question.

It is almost impossible, as Freud points out in his introduction to the study of Dora, to illustrate the more complex (and, alas, commoner!) kinds of theme-tracing without making multiple reference to previous sessions and to the whole structure of the patient's life. I shall present you with a short example that involves a specific postdiction, namely, what the patient was reading the night before, and whose photograph looked like the person dreamed of. I use it as a kind of litmus test on my Minnesota colleagues to diagnose who is totally closed-minded against psychoanalytic thinking. If this sort of instance doesn't at least mobilize a rat psychologist's or psychometrician's intellectual curiosity to look into the matter further, he is a case of what the Roman Church would call "Invincible Ignorance."

Businessman, late thirties, wife had been a patient of mine; he refused to pay for her psychotherapy because "didn't believe in it, nothing to it." (She cashed an old insurance policy for payment.) Wife benefited greatly, to the point he became interested. Bright, verbal man of lower-class background (father was a junk-dealer). Patient went to University of Minnesota for almost two years, premedical course. Quit ostensibly because family needed money (in depression years), but he was flunking physics at the time and his overall grades would not get him into Medical School. Older brother Herman used to reprimand him for laziness. Brother got top grades, Ph.D. in chemistry. For a while patient worked in brother's drugstore. Brother was a pharmacist, who finished graduate school while continuing in drugstore business to make a living. I knew (from wife's therapy) that the brother had mysteriously died during surgery in what was thought to be a fairly routine operation for stomach ulcer, the family having been greatly impressed by the fact that on the night before the surgery, Herman expressed with great dread an unshakeable conviction that he would die on the operating table. (Some physician colleagues have told me that—whatever the explanation of this phenomenon, which has been reported in medical folklore before—they would have considered it undesirable to operate on the patient under those circumstances.) During the first interview the patient related to me in a mixed fashion, including: a) nonchalance, unconcern, "minor problems"; b) jocularity; c) deference; d) competitiveness. (I won't fully document these impressions as noted after the first hour, but an example: He fluctuates between addressing me as "Dr." and "Mr.," and once corrects himself twice in row—"Dr.,—uh, Mr.,—uh, Dr. Meehl.")

In the fourth session he had reported a dream that was quite transparent, even prior to associations, about a waiter who provided poor service, making the patient "wait a long time before providing anything. Also, I couldn't say much for the meal [= Meehl; I have learned, as have most analytic therapists whose names readily lend themselves to punning, that dreams about food, meals, dinners, lunches, etc. frequently are dreams of transference nature—as in my own analysis, I learned that, like some of my analyst's previous patients, if I dreamed about "buses," it likely referred to him because 'Bus' was his nickname]; and the bill was exorbitant, so I refused to pay it." He went off on discussion of ways his wife had and had not improved, alluded to the parking inconvenience on campus, time taken coming over here, expressed "hope we can get this thing over with fairly rapidly." At end of hour asked what bill was [$10—my modest fee back in the ancient days before inflation!] and "tell me something about the waiter" (blond, blue-eyed, crew-cut, mustached—an exact description of me in those days). I asked directly what thoughts he had about our sessions. He said he had wondered why I wasn't saying anything much. He contrasted our sessions disappointedly with some sessions his wife had told him about, in which she had been fascinated by interpretations of her dream material. I then interpreted the dream and summarized the corroborating associations. He asked "When did you figure out what I had on my mind?" I said I guessed it at the beginning, as soon as he reported the dream. (Debated matter of technique: Because of my Radovian analyst and supervisor, I frequently depart from the classical technique, following the practice of Freud himself and some of his early colleagues that part of the "educative" phase early on is to gain leverage by establishing a conviction in the patient that the process has a meaning, even if that involves saying something about the therapist's inferential processes and when they occurred. The dangers of this are obvious, but doing it carefully to avoid gross one-upmanship is part of what I believe to be involved in intellectually mobilizing suitable patients by engaging their cognitive needs, their need to understand themselves, and the sheer element of intellectual interest that is part of what aligns the observing ego to enter into the therapeutic alliance. Part of what one does early on is to engage the patient's reality-based, mature cognitive interest in the psyche and its machinery. Persons who cannot think psychologically and cannot distance themselves from their own puzzling experiences are unsuitable for psychoanalytic therapy; even patients who are able to think psychologically about themselves and others usually lack a firm, concrete, gut-level conviction about the unconscious. Any professional in psychiatry or clinical psychology who has had analysis can report his surprise during early sessions at the fact that "all this stuff is really true, even for me!") My patient seemed to be very impressed by this, chuckling and repeating, "By God, that's fascinating," and, "To think I didn't know what I was talking about, and you did!"

In the fifth session, following the session about the waiter dream, patient enters smiling, remarks before reclining, "Sure was interesting last time, *you knew* [emphasis!] all along what was going on and I didn't." First dream: *An Oriental, some kind of big shot, Chinese ambassador or prime minister or something—he's hurt—he has a big bloody gash in his abdomen*. Second dream: *I am measuring out some pills from a bottle*.

His associations continued as follows: Drugstore with brother—patient was partner but not registered pharmacist—at times he put up prescriptions when brother not in—nothing to some of them—silly to take the task so seriously—brother's Ph.D. degree—"he studied—I was lazy—lacked ambition—still do—make more money if I had more ambition—brother also brighter to start with—how bright am I, really?—always caught on to things quickly—poor work habits—wanted to be a physician, but not hard enough—I don't like doctors much—haven't seen one in years—some are pretty dumb"—(long discussion of wife's gynecologist who missed her diagnosis when patient got it right)—in drugstore patient used to advise customers about medication—often felt confident he had diagnosed a condition their doctor wasn't treating them for—(rambles on about various incompetent physicians he has known, detailed anecdotes with use of medical terminology, including narrative of brother Herman's unexplained surgical death, insensitivity of surgeon in pooh-poohing Herman's fear, then back reference to Herman's getting the Ph.D. by his brains and hard work). Comments on "experts who don't necessarily know much more than an intelligent amateur." But doesn't want to "overgeneralize that." (Pause—the first even short pause in the stream of the associations. Here one asks the tactical question whether to wait the silence out, which is sometimes appropriate, but in my experience, frequently not. I believe the tendency to wait it out regularly, as developed in this country during the twenties and thirties, is one reason for interminable analyses of people who are actually good prospects for help. We want to know what the patient is thinking that makes him pause, and I know of no really persuasive technical reason not to ask.) (Q Thinking?) "Still can't get over our last session—that *you knew* what was on my mind when I didn't—not just that I didn't—that's to be expected—but that *you knew*, that really gets me!"

Here, after the third over-stressed phrase "You knew," I had an association. The previous night I had been reading TIME, and saw a photo of the Burmese prime minister U Nu. So one hypothesizes a pun, mispronouncing the name, hence there is a linkage to me via this pun: Meehl = "You knew" = U Nu = Oriental in dream.

So I asked him for further thoughts about last session. "Surprised" [Pause] (Q Go on) "Impressed—what else can I say? [Pause] (Q Just keep talking) "Taken aback, sort of—why didn't *I* recognize it, if you did?—pretty obvious—then the $10 bill and all that stuff—shouldn't take an expert to see that!" [Laughs] (Q Any negative feelings at all?) "No—no negative feelings—irked with myself." (No resentment at all, toward me?) [At such moments one must be persistent] "No—or if so, very faint—I'd hardly call it resentment even." (Q But a feeling as if I had sort of won a round, or had one up on you, perhaps?) [Laughs] "A bit of that, sure—it's kind of humiliating to go yackety-yacketing along and then find out *you knew* all along—so I suppose you could say there was a little element of resentment there, yes."

I then asked if he was reading last night ("Yes, TIME"). Pressed for recall—"business, foreign affairs" (Q Picture in foreign affairs?) "Hey! By God—that oriental in the dream was a photo in TIME". Patient can't recall name— I tell him "U Nu" and point out that again, as last session, a play on words is involved. The dream shows how strong this reluctantly reported and allegedly faint resentment is, in that he has me wounded (killed, castrated, made into a woman?), linked perhaps by the associations to his wife's vaginal problem and the professional's misdiagnosis; then there is the obvious connection with the abdominal wound that surgically killed his competitor, harder-working Ph.D. brother Herman. The interpretation of this material led to some further fruitful associations regarding his competitive feelings toward his business partner, whom he had originally described to me as "entirely compatible" and "a sympathetic person." I had external evidence, not discussed with him before this session, from his wife that in fact the business partner tended constantly to put down the patient, to underestimate his abilities, to pontificate to him about cultural matters in which the patient was as well informed as he (the business partner, like brother Herman, had completed his college education with high grades). As a result of this interpretation, some of that ambivalence toward the business partner came out, and several subsequent sessions were especially fruitful in this regard.

I should be surprised if any psychoanalytically experienced readers disagreed about the essentials of this dream's meaning. (As stated earlier, I bypass here questions of optimal technique, the "output" side of therapist interpretation, except to remind ourselves how *avoiding* intervention helps avoid theory-contamination of the patient's associations). I have found in every audience of nonanalysts several listeners whose sudden facial "Aha!"-expressions showed the moment they "got it," sometimes with the irresistible bubbling up of laughter that so often accompanies a good interpretation (thus fitting Freud's theory of wit). It is equally clear that the lay audience (that includes some clinical psychologists in this context) displays wide individual differences in how soon various members begin to "catch on." And, of course, some tough-minded behaviorists or psychometrists (while smiling willy-nilly) shake their heads at the gullibility of Meehl and the other audience members. So Achensee—justifiably—is with us yet! The two diagrams say most of what needs saying by way of reconstruction. In Figure 1, the session's associative material is presented running sequentially (as it occurred) along the left and bottom. My first intervention (neglecting any unintended signals of changed breathing, throat-clearings, or chair squeakings) occurs after the first short pause by

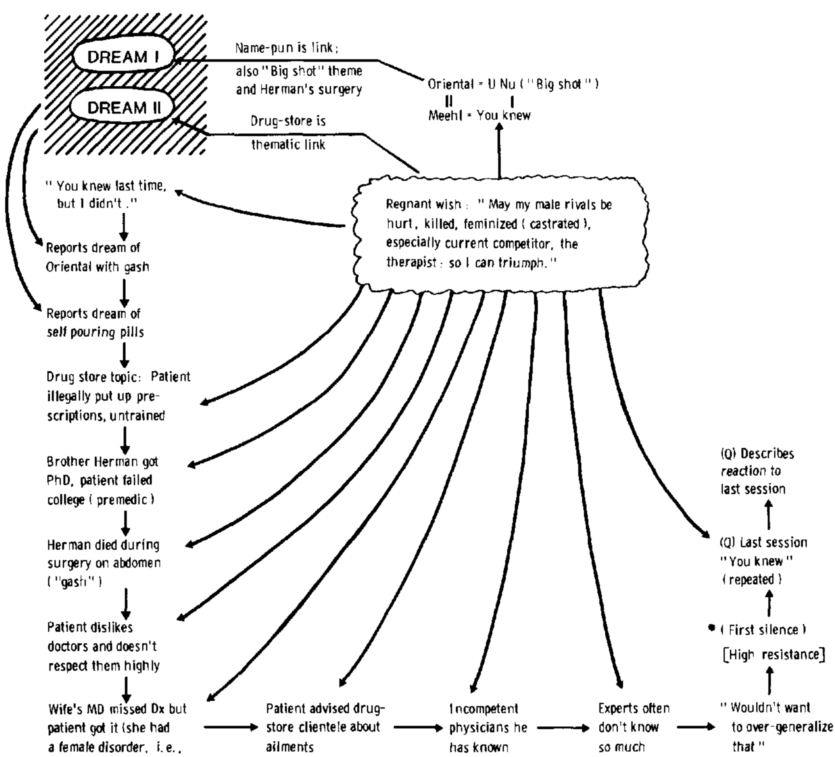

Figure 1.

the patient ("editing," in violation of the Fundamental Rule), after his mentioning not-so-knowledgeable experts he "wouldn't want to overgeneralize about." The postulated guiding theme ("unconscious wish-fulfilling fantasy," if you will) is shown at center. One sees that associations viewed as "topics" are all loosely connected with the second dream's manifest content, and with each other. In Figure 2, I have avoided the "causal arrow" in favor of nondirectional lines (without arrowheads), as here we merely conjecture associative linkages that perhaps strengthen some of the final verbal operants; but we do not say which way the causal influence runs, nor assign any time-order. The strengthening of associations here is loosely "contextual," and some connections are obviously more speculative than others.

The first dream finds no plausible place in this network, except via the (hence crucial) "U Nu = You knew" word-play (and, of course, the dream-day event involving TIME). We also, bootstrapping (Glymour 1980), invoke Freud's rule of thumb (Freud 1900/1953, pp. 315-316, 333-335, 441-444) that two dreams of the same night deal with the same thing and often express a cause-effect relation, the first dream usually (not always) being the antecedent of the causal "if. . . then." Read here: "*If* Meehl [= U

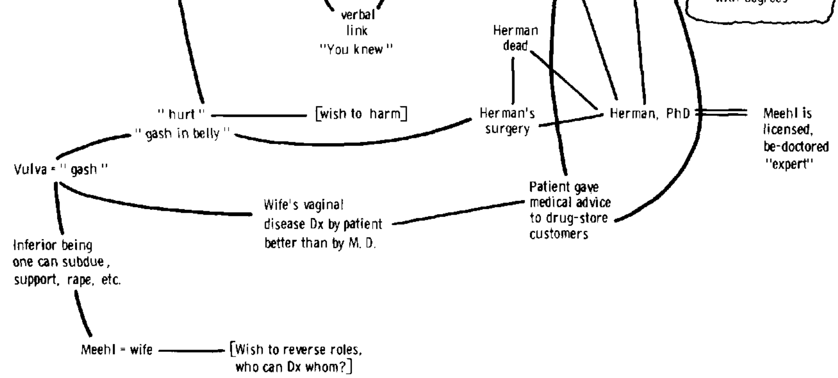

Figure 2.

Nu = "you knew"] is killed, like my earlier sibling competitor Herman, *then* I will be the triumphant, learned, be-doctored expert who is perfectly capable of prescribing pills, etc." This inferred latent structure I do not here pretend to "quantify," and I am not convinced it needs to be quantified. All that the theme-tracing method to be proposed would perhaps do for us is to reduce somewhat the "subjective ad hockery" component in the skilled clinician's discerning the "red thread" allegedly woven into the associations. (For a similar approach to a non-psychoanalytic interpretative problem of psychology see Meehl, Lykken, et. al., 1971).

What is nomothetic and, in principle, "computerizable" contributes to our understanding, but is rather feeble here unless combined with the idiographic components. A male figure with an abdominal wound would presumably occur in a psychoanalytic content analysis dictionary tagged with "castration" and "aggression" themes. But we don't have a place for pouring out pills, we don't have a place for Orientals, and we certainly couldn't get to the postdiction about TIME via the pun on U Nu. The pincers that "close together" to make the Achensee Question hurt do so, in this kind of situation, because the complex ontology (one pincher) requires a complex imposition of thematic content by the analytic listener and hence the other pincer (subtle epistemology) closes simultaneously. Detecting the "red thread" of allusions in the associative material, performing our psychoanalytic Vigotsky on blocks varying in many ways other than shape, size, and color, invalidates the jigsaw analogy, at least in the eyes of the skeptic. We have to discern what is common in the blocks of verbal output, but "what is common" resists any simplistic semantic or syntactic categorization. At the risk of overstating my case, I repeat, one must begin to formulate his conjectures before he can discern that a certain speech sequence tends to confirm them. To quote a previous paper of mine on this subject:

> Skinner points out that what makes the science of behavior difficult is not—contrary to the usual view in psychoanalytic writing—problems of observation, because (compared with the phenomena of most other sciences) behavior is relatively macroscopic and slow. The difficult problems arise in slicing the pie, that is, in classifying intervals of the behavior flux and in subjecting them to powerful conceptual analysis and appropriate statistical treatment. Whatever one may think of Popper's view that theory subtly infects even so-called observation statements in physics, this is pretty obviously true

> in psychology because of the trivial fact that an interval of the behavior flux can be sliced up or categorized in different ways. Even in the animal case the problems of response class and stimulus equivalence arise, although less acutely. A patient in an analytic session says, "I suppose you are thinking that this is really about my father, but you're mistaken, because it's not." We can readily conceive of a variety of rubrics under which this chunk of verbal behavior could be plausibly subsumed. We might classify it syntactically, as a complex-compound sentence, or as a negative sentence; or as resistance, since it rejects a possible interpretation; or as negative transference, because it is an attribution of error to the analyst; or, in case the analyst hasn't been having any such associations as he listens, we can classify it as an instance of projection; or as an instance of "father theme"; or we might classify it as self-referential, because its subject is the patient's thoughts rather than the thoughts or actions of some third party; and so on and on. The problem here is not mainly one of "reliability" in categorizing, although goodness knows that's a tough one too. Thorough training to achieve perfect interjudge scoring agreement per rubric would still leave us with the problem I am raising. (Meehl 1970, p. 406)

I say again, we require the subsuming powers of the clinical brain, but we need a reply to the skeptic who says that there is so much play in the system that we can subsume arbitrarily, any way we want, by some mixture of general theoretical preconceptions and the prematurely frozen conjectures that we arrived at from listening to the dream and first association. My fifth proposal for making a dent in this problem is not very elegant, and I have not worked out any fancy statistics for doing it, partly because I think that they will not be necessary. We first have a clinically naive but intelligent reader break the patient's discourse into consecutive blocks, which I shall label "topics." This initial crude categorizing is done without reference to inferred motives by someone ignorant of such things as defense mechanisms, symbols, and the like, essentially in the way a high-school English teacher instructs students to paragraph a theme by topics. Passing intrusions from the manifest content of some other block are simply ignored (e.g., a one-sentence allusion "as I said, Jones was the sergeant" does not fractionate a block of discourse dealing with a single "non-Jones" episode of barracks gambling). In Table 1 I have done this by three crude topic designations running along the top of the table. The purpose of this breaking up by crude manifest topics is essentially to provide separable chunks of material sufficiently large for a clinician to discern possible themes, but sufficiently small to prevent his contaminating himself by

Table 1. Themes Discerned by Five Analysts Independently Reading Discourse Block I (Crude Topic: Brother Herman). Hypothetical Data.

	Theme Rubrics
Analyst A:	"Competition with males"
	"Sibling rivalry"
	"Object-loss" [Herman's death]
Analyst B:	"Object-loss"
	"Competition with another male"
	"Childhood family"
Analyst C:	"Sibling rivalry"
	"Educational failure"
	"Intellectual snobbery"
Analyst D:	"Competition with males"
	"Object-loss"
	"Self-sabotage" [didn't study]
Analyst E:	"Inadequacy-feelings"
	"Childhood family"
	"Sibling rivalry"

themes discerned when looking at other blocks. So each crude topic block of discourse is submitted to *different* psychoanalytic therapists with instructions to write down whatever theme occurs to them as "present in it," "underlying it," "alluded to by it." This requires several teams of readers who do not have access to any of the crude topic blocks that the other teams are reading. We then type up (on 3 x 5 cards) the set of conjectured themes that have been generated in our analytic readers by a particular block. These cards are given either to another team (or, in this phase, I see no harm in the same team doing it) and we ask them to rank (or rate, or Q-sort—I think the psychometric format unimportant here) each theme as to its likelihood (or strength?) as a thematic contributor to that block. Writing the instructions for this third phase will be tricky, because there is a certain opposition between base rates on the one side and low probabilities (as being stronger evidence) on the other side, which is one of the reasons we need clinicians as judges. We employ the 2-phase rating scheme because we believe that a clinician especially skilled (or hyper-responsive to a particular theme) may *sometimes* discern something that the other clinicians will quickly see as a good bet even if they didn't come up with it themselves.

When these batches of rated themes are colligated in a single table, one now reads horizontally instead of vertically, to see whether the thematic

Table 2. Summed (Weighted) Ratings of Themes Discerned within Blocks by Three Independent Sets of Analysts. Hypothetical Data.

Block I: Brother Herman		Block II: Drug-Store		Block III: Wife's Physician	
Theme	Σw_i	Theme	Σw_i	Theme	Σw_i
Competition with males	8	Self-aggrandizement	7	Intellectual snobbery	6
Sibling rivalry	6	Economic insecurity	5	Self-aggrandizement	6
Object-loss	6	Negative transfer	5	Competition with males	6
Childhood family	3	Hostility to experts	4	Negative transfer	5
Inadequacy feelings	3	Super-ego defiance	3	Dislike of doctors	4
Educational failure	2	Sibling rivalry	3	Object-loss	3
Self-sabotage	1	Competition with males	3		
Intellectual snobbery	1				

"red thread" is apparent. In Table 2 I have illustrated this with fictional ratings. The summed (weighted) ratings for "Competition with males" being the largest ($\Sigma w_i = 17$) the red thread would be crudely quantified by these imaginary results. I do not have an appropriate significance test for evaluating the end result of this process, but I am not much interested in statistical significance testing anyway. A couple of obvious possibilities are to establish a crude baseline for "chance congruency" by slipping in blocks that belong to a different session or even to a different patient. One interesting question is how often we can "make sense" of the associations given to a dream even if the manifest content was not dreamed by the associater—a claim made against psychoanalysis forty years ago by Rudolf Allers in his book *The Successful Error* (Allers 1940) and never, to my knowledge, answered.

Space does not permit an adequate treatment of such a method's limitations, but there is one major defect that must be mentioned. Sometimes the allusions are few in number, perhaps *only* two or three, buried in high-resistance "sawdust," but are given evidentiary weight because of some delicate mix of very low nomothetic base-rate ("expectedness" in the denominator of Bayes' Formula) with very high idiographic linkage ("conditional probability" in the numerator of Bayes' Formula). In such sessions, the Topic Block Theme Tracing method would fail utterly; and, I fear, so would all the others.

Summary

Summarizing this essay is rather like pulling together the material from a murky psychoanalytic hour, which is perhaps diagnostic of my cognitive condition. I *do* have a theme of sorts, but it's hard to verbalize briefly. In a word, I am ambivalently saying that Fliess's Achensee question deserves a better answer than it has yet received. Granted that there are respectable documentary disciplines (like history) that rely mainly upon qualitative evidence, a mind's discernment of intentionality, and the argument from convergence—disciplines that are neither experimental nor statistical in method; and granted that the "experimental/quantitative" (often called the "scientific") is not coextensive with the empirical, nor with the reasonably believable; and granted that the usual behaviorist and psychometric objections to the *concepts* of psychoanalysis (e.g., not "operationally specified") are simplistic and philosophically uninformed; granted all this, it remains problematic just what *is* the state of our evidence from the best source, the analytic session. I have suggested five directions we might take in an effort to ascertain how much of what the "thought reader" reads—admittedly using his own mind—is also objectively there, in the mind of the other.

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