

IS PSYCHOANALYSIS ONE SCIENCE, TWO SCIENCES, OR NO SCIENCE AT ALL? A DISCOURSE AMONG FRIENDLY ANTAGONISTS

When I was invited to give this plenary address I surmised that people would be interested in learning about my research. The invitation also offered the opportunity to place my research in the broader context of research in psychoanalysis, a complex and even controversial subject. What you will hear is my attempt to present that context in the form of a discourse among friendly antagonists. My research will be introduced at an appropriate point so that its relevance to the larger picture of research in psychoanalysis can be evaluated.

THE FRIENDLY ANTAGONISTS

On a rainy August afternoon, somewhere near Amagansett, three psychoanalysts gather in the summer home of Professor di Sapienza, an academic colleague who is a psychologist knowledgeable in the philosophy of science.

Dr. Case is a staunch believer in the primacy of the clinical situation as the bedrock of psychoanalytic science.

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Dr. Sample, a careful and sophisticated thinker, although as convinced as Dr. Case of the centrality of the clinical situation to psychoanalysis, is seriously concerned about its scientific foundations and a believer in the necessity of systematic research, albeit limited to the clinical situation.

Dr. Link, noted for his efforts to build interdisciplinary bridges to other sciences, believes that a comprehensive theory incorporating the insights of the clinician and recent research in related fields will serve to broaden and deepen psychoanalysis and increase its acceptance among skeptical scientists.

Professor di Sapienza has an ulterior motive in inviting her friends to her home on this rainy afternoon: She has finally decided to stop being so kind to her psychoanalytic colleagues, to be so ready to listen without criticism to their admittedly fascinating but empirically and systematically anecdotal effort at science. She has always secretly been convinced that fundamentally psychoanalysis is not really a science, but is nevertheless an important rational enterprise. She is prepared to have it out with her colleagues as a first important step in her plan to write a telling critique of psychoanalysis and a recommendation as to its true place among intellectual pursuits.

FIRST MEETING

Di Sapienza: Thank you all for coming. As I mentioned to each of you over the phone, what better way to spend a rainy afternoon than to get together and discuss matters of mutual interest? I asked each of you to come prepared to discuss your view of psychoanalysis as a science. To some this is an old chestnut, to others it is a burning question, and to still others it matters not at all. What I would like to invite you to do first is for each of you to state your position as succinctly as possible in this first go-around. After each of you has stated your point of view, we will then see where we are and if there is reason to continue further. I would like to invite Dr. Case to begin.

Case: My position can be put simply, and I believe it constitutes a classical view of psychoanalysis as a science sui generis. The psychoanalytic situation is the locus not only of discovery but also for testing

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hypotheses. The data of psychoanalysis can *only* be obtained in the psychoanalytic situation. Past efforts to investigate psychoanalytic phenomena in the laboratory, or in the field, have largely failed because they cannot duplicate the phenomena or benefit from the full richness of the theory. The debacle of so-called repression research in the 1940s is a telling and grim reminder of how all such misguided efforts must fail. I maintain that the sheer growth, the dynamism, and the actual results of psychoanalysis are themselves proof of the validity of psychoanalytic propositions. There is no need to discount the experience worldwide of thousands of psychoanalysts who agree on fundamentals derived from their practice: the role of the unconscious, transference, resistance, the discoveries of infantile sexuality, and the central role of conflict. On the basis of phenomena brought to light by psychoanalysis, as Brenner has demonstrated (1982), the richest, most complex theory of the mind in conflict has been forged. Moreover, a science can be qualitative and empirical (that is, nonexperimental) and yet be lawful and capable of proof within the domain of its application. Take the example of geology for long periods of its development. Other sciences are certainly welcome to select from among our findings and hypotheses and see how they are relevant to their own domain of application. Only the psychoanalytic method itself can inform us about what goes on intrapsychically and, by way of reconstruction or construction, what the nature of earlier intrapsychic experience was like.

My conclusion, therefore, is that psychoanalysis is indeed a science in its own right, with its own domain of application, phenomena, discoveries, theories, and criteria of proof, and I would be more than happy to provide at some later time more detailed evidence in favor of my position.

Di Sapienza: Dr. Sample, I would anticipate that you have a thought or two about what Dr. Case has just propounded.

Sample: With the greatest respect for Dr. Case's forthright presentation of a classical view of psychoanalysis as a science, I would like to argue for a significantly modified version of his position. Certainly I agree that psychoanalysis is a science in its own right; however, I am much less sanguine than Dr. Case concerning the psychoanalytic situation as a locus for testing and proving hypotheses. In the clinical literature, composed mainly of case reports, often

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the criteria of proof cited by psychoanalysts amount to a list of positive instances. But what philosophers of science refer to as *enumerative inductionism* does not constitute proof. Edelson (1988) reminds us of the turkey who comes to believe that 364 days of well-fed contentment is a guarantee of immortality. Positive instances, no matter how many, will not suffice. The need is to meet the criterion for *eliminative inductionism*, which requires that the *absence* of a given factor be correlated with the *absence* of its consequent. Seldom if ever do we find this criterion met in the psychoanalytic literature, nor does one often encounter the discipline involved in formulating alternate hypotheses and submitting them to proof.

Further, psychoanalysts practice a method based on certain presuppositions for which the procedure or the method itself cannot provide proof. These two presuppositions can be stated quite briefly in Freud's own words: "When conscious purposive ideas are abandoned, concealed purposive ideas assume control of the current of ideas, and . . . superficial associations are only substitutes by displacement for suppressed deeper ones" (1900, p. 531). What independent evidence can be found in support of these presuppositions—the first having to do with a causative psychological unconscious, the latter with the crucial role played by such transformations in consciousness as displacement, reversals, and symbols as the presumed complex effects of unconscious causes? At this stage, perhaps little, yet scientists often work productively for years without examining their methods and presuppositions. For the longest time, chemistry assumed that something like molecules existed, though they could cite no independent proof. It just made too much sense to assume it. So, immediate examination of these presuppositions is not urgent, but they should be recognized as assumptions, ultimately in need of independent proof.

Much more urgent is to deal with the clinical situation in a more systematic way by introducing the usual essentials of the scientific method: controls, reliable measures, and above all testing of alternate hypotheses so that the criterion of eliminative inductionism can be met. The need is for sophisticated outcome and process research devoted to the psychoanalytic situation.

Although I would not rule out in principle relationships to other sciences, the pursuit of these relationships is at this time premature—concepts that can "bridge over" to other sciences are lacking and will likely remain so for some time to come.

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I conclude that psychoanalysis as a science has not yet achieved its fullest flowering but is capable of benefiting from a more rigorous application of scientific method to the phenomena obtained within the psychoanalytic situation.

Di Sapienza: Well, Dr. Link, it's your turn. I know you depart radically from the positions just outlined by Drs. Case and Sample.

Link: You are exactly right. In my opinion, Dr. Sample's position encounters three serious problems, two of which apply also to Dr. Case's position. First, they both narrow the purview of psychoanalysis to the clinical situation itself as the locus of scientific inquiry. As a result, they sell short the powerful theories developed by psychoanalysts constituting what psychoanalysis has to say about the mind itself, its biological roots, its development, and, in short, the implications of these theories for other sciences. Reiser (1984, 1990) has explored some of these possibilities in two recent books. Second, Dr. Sample's position advocates enlisting all psychoanalytic scientific resources in the service of psychotherapy research, which, while necessary, is inherently limited, as I will try to show. Finally, by relying only on psychoanalytic data, Dr. Sample ignores his own acknowledged need for independent evidence for psychoanalytic presuppositions, which he has quite concisely identified, while Dr. Case does not seem to be concerned with this problem at all.

As a consequence of these three problems, Dr. Sample's position, while shrinking psychoanalysis to the limits of a purely applied science, requires at the same time that it perform as a basic science. This is not possible. We should recognize that psychoanalysis is really two integrally related but separate sciences: an applied clinical science and a basic science of the mind. As an applied clinical science, it must be concerned with treatment outcome and process, the limits of its applicability, and the training of clinicians. But at the same time it must be recognized that insofar as it is an applied science it applies a body of knowledge and theory—based on certain presuppositions or assumptions—to a given patient's emotional disorder. It is the task of the psychoanalytic basic science of the mind to raise up this edifice of knowledge out of clinically derived insight, systematic, experimental, and nonexperimental research, and bridge building to other sciences. This task requires drawing upon the findings of these other sciences and their methods for convergent, as well as

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potentially contradictory, evidence. The optimum relationship between an applied and a basic science requires a separation of methods and at least of some of its investigators while maintaining an active commerce between the two.

I conclude that psychoanalysis has in reality always been two sciences—an applied science and a basic science—but they have through the long history of psychoanalysis been confounded so that the advantages and power of each have been vitiated. Let us finally recognize that psychoanalysis is really composed of two sciences, separate them, and keep them creatively interacting.

Di Sapienza: Perhaps you were all expecting that I would remain in the role of interlocutor, but I suspect that I will now surprise you by advancing the argument that psychoanalysis is not a science at all but rather is a rational but distinctly nonscientific enterprise. You have all assumed that psychoanalysis must be a science, either by nature or by fiat, if it is to hold its head up among intellectual disciplines and be taken seriously. This need not be the case at all. Teaching and governing are both rational enterprises with worthwhile goals involving bringing about change in individuals or in society at large, and yet are not sciences, neither applied nor basic. Nor are teaching and governing to be confused with hermeneutic science; neither teaching nor governing requires exegesis of a text or following rules of textual interpretation, but are quintessentially rational human enterprises dedicated to bringing about change in one form or another. Efforts to convert teaching and governing into applied sciences continue to fail, even though educational theoreticians and political scientists may offer theories and findings from time to time that can be of some limited use.

A rational enterprise such as teaching, governing, or psychoanalysis must be distinguished on the one hand from applied science, and on the other from an art, though it draws upon both and may erroneously be confounded with one or the other. Neither teaching nor governing nor psychoanalysis are basic sciences, or sciences in any true sense, but they are important rational enterprises conducted by rational change agents with highly valued goals. For teaching, the goal is to assist individuals to acquire knowledge and skills. For governing, it is to provide the greatest good for the greatest number. And, for psychoanalysis, it is to assist an individual to acquire self-knowledge and self-mastery.

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Why is psychoanalysis not a science? The problem is not in the need to be rational and systematic. All human pursuits require a modicum of rationality and discipline. The problem resides in the nature of the subject matter, the practitioner, and in the nature of the goal. Teaching, governing, and psychoanalysis share as their subject matter humanity itself, albeit from different perspectives. Human beings, both individually and in groups, are complex, changing, growing, deceptive, and unpredictable. The practitioner is a human being also, to whom the same considerations apply.

It is this relationship between two human beings—each complex, changing, growing, and highly individual—having the goal of bringing about change that makes it necessary to resort to what I will call *improvisation*, or inspired guesswork. The reason I referred to teaching, governing, and psychoanalysis as rational *enterprises* is to emphasize that each requires action rather than simply contemplation. By action I mean making decisions that affect people's lives. In view of the sheer complexity of the circumstances—its constantly changing, evolving character—each action must to a significant extent be different and suited to highly specific circumstances. It is in this sense that these rational actions must be considered improvisations. To those who would say that guesswork is also part of science, my response would be that what are initially guesses can be tested under similar conditions, whereas this cannot be the case for teaching, governing, or psychoanalysis.

Why is psychoanalysis not an art? Artists work their will on relatively inert material lacking a will or purpose of its own, nor is their aim to transform the material for its own sake, but rather to serve their own ends. When people refer to psychoanalysis as an art, what they seem to mean is that there is a technique or a set of guidelines as to how to proceed with an analysis, rather than referring to *creating* a work of art. To attempt the latter as a psychoanalyst would simply be hubris.

My conclusion is quite different from all of yours. Psychoanalysts are to be congratulated and respected for valiantly taking the Socratic admonition to "know thyself" beyond the limits of the purely philosophical and cognitive to incorporate the rest of the human psyche and, further, to aim at bringing about significant changes not only in self-knowledge but in self-mastery. As such, they are in

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the special and elite company of teachers and governors who in their own ways attempt to facilitate the growth of mastery and human satisfaction. It is unworthy of the nature and goal of psychoanalysis to require that it be a science, either applied or basic. The criteria of science when applied to psychoanalysis curb and hamper its ability to perform what it alone can accomplish as a rational human enterprise. The often heard complaint of psychoanalysts that research findings are of little help to them can best be understood in this light. It is not that the research is meaningless; it is simply irrelevant.

I would like to propose that we not wait for another rainy day but meet at this same time next week. I propose that you each present a more fully worked out statement of your position and that we then have a go at each other.

SECOND MEETING

Di Sapienza: Welcome. Again, I would ask Dr. Case to begin.

Case: I will argue and provide evidence in favor of the sufficiency of the clinical situation for a full-bodied science of psychoanalysis. But I will start with the flat statement that psychoanalysis is neither a purely clinical nor an applied science, and certainly not a practical enterprise like teaching or governing. In the strictest sense, psychoanalysis is a science in its own right, using a unique method enabling the psychoanalyst to get in touch with phenomena that no other method can deliver.

Psychoanalysis is not a clinical science in the usual sense, because its method is inherently *investigative* as well as ameliorative. Psychoanalysis, for example, does not provide a medication whose ameliorative action is based on principles and knowledge derived from some presumably more basic science like biochemistry. Rather, the investigative method of psychoanalysis in its own right aims to disclose the principles and knowledge upon which the cure depends, as well as to create the conditions for that disclosure. This point is of decisive importance. The psychoanalytic method, properly employed, *elicits* as well as *elucidates* the phenomena of interest. The patient's resistances and transferences, although always potentially present, are drawn forth by the operation of the method so that

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these resistances and transferences can be experienced by the analysand. The psychoanalytic method is not simply a telescope or microscope making something visible that is already there; it is more like an alembic in which materials are transformed, distilled, and fractionated, or, if you like, analyzed. For these reasons, no other science can be "basic" to psychoanalysis, because only the psychoanalytic method both elicits and elucidates the phenomena of interest. For these reasons, no further "proof" is necessary—the rightness or wrongness of the results are apparent to analyst and analysand, much as one knows when one has recovered the right name after a brief search. You do not need someone else to tell you that the name is right or wrong. In fact, you are the only one who *can* decide that. Psychotherapy research at best only gilds the lily, while research requirements such as recordings and sampling destroy the organic integrity of psychoanalysis. The psychoanalytic method is *not* a series of inspired improvisations, but requires systematic application and rigorous attention to the unfolding of the process, a process that is subject to the discipline of a method. For *all* these reasons, I maintain that psychoanalysis is a science *sui generis*. It is neither an applied science requiring adjunctive proof or the elucidation of its underlying principles by a more basic science, nor a nonscientific yet rational improvisational enterprise.

Di Sapienza: After that brilliant cadenza, Dr. Sample, it is your turn.

Sample: As a science *sui generis* as described by Dr. Case, psychoanalysis will not, I'm afraid, stand up to scrutiny. Its unique method has serious limitations, and other methods must be applied to the clinical data to make up for them.

To start with one obvious yet important point: the data of psychoanalysis are not public. A *sine qua non* of any science is that its data be fully available to other interested scientists. Although Dr. Case makes an eloquent argument for the uniqueness and power of the psychoanalytic method, how do we know that it is always practiced as preached? Despite elaborate procedures for evaluating the quality of psychoanalysis practiced by our graduates, these procedures depend almost entirely on examining analyst summary reports of their cases—an inadequate substitute for the data themselves. Of course, this is the very same problem we have with published case

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studies. Although some may balk at this, it is only through audio and video recordings of an analysis that a public record of the data can even be approximated.

But having a public record is only the beginning. Certainly, for one, the limitations of these audio and video data as a full record must be acknowledged. Much of importance obviously goes on in the minds of analyst and analysand that is neither spoken nor visible. Nevertheless, the recordings, though they do not present, for example, the analyst's fantasies or conjectures, are a real record of a significant portion of the analytic data and should not be sniffed at. Aside from the need for public data, one can also wonder if it is so clear that the method works as Dr. Case has described. Again, "rightness" to analyst and analysand is a purely subjective, as well as private, criterion. The analyst in effect tells his scientific colleagues: "Take my word for it. This is what happened in this psychoanalysis."

We must heed and respond to Grünbaum's searching critique of the psychoanalytic situation as the locus of hypothesis testing. How can we respond to his assertions that suggestion cannot be ruled out as a contaminant, and that correctness of interpretation cannot be demonstrated by therapeutic gain, the so-called tally argument?

Suggestion can be ruled out only by an examination of a public record. The critique of the tally argument can be answered only by developing methods that can assess correctness of interpretation independent of treatment outcome. Fortunately, methodological and empirical advances in psychotherapy research have been made on both these counts. Let me illustrate with two research findings. I offer these illustrations as examples of methodological solutions to the problems stated; no agreement with the theories employed in the research is required in order to appreciate the strength of these methods.

Weiss and Sampson (1986) have developed a method for identifying, in advance of treatment, certain pathological beliefs. They have been able to show that when the analyst addresses these pathological beliefs, the patient improves.

Based squarely on the central psychoanalytic conception of transference, Luborsky and coworkers (Luborsky and Crits-Christoph, 1990) have developed a way of inferring reliably the presence of what they refer to as a Core Conflictual Relationship Theme

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(CCRT). When these themes, formulated in advance and thus independent of their therapeutic effect, are worked with interpretively in the treatment, the outcome is more likely to be favorable. These psychotherapy researches point to the right path to follow if our data are to be public and some of our basic hypotheses confirmed.

I would conclude, in agreement with Dr. Case, that the clinical situation can and should be the locus of scientific investigation and proof, but that additional, clinically based research methods are necessary to provide that proof. Although other sciences may provide converging evidence, they are not essential to investigating psychoanalytic hypotheses best studied in the psychoanalytic situation itself.

Di Sapienza: Well, Dr. Link, we are ready to hear from you.

Case: I would like to hear how any science can be basic to psychoanalysis. I hope you will cite chapter and verse.

Sample: Yes, in particular about bridge building to other sciences.

Link: I will try to satisfy you, but you will need to bear with me as I will have to go into some detail. But first some comments on Dr. Case's and Dr. Sample's positions. Dr. Case raises for me a fundamental question: Can any science be unique and sufficient unto itself? My answer is an emphatic no. The constraint on this uniqueness is to be found in its underlying presuppositions. Every science must assume some things to be true in order to get on with its work. These presuppositions may turn out to be true or false, with fateful consequences for the science.

I must take issue with Dr. Case on another point. It is not logically feasible that independent evidence in favor of psychoanalytic presuppositions of such central importance to psychoanalysis as a causative unconscious making itself known indirectly and through various transformations can be found within the psychoanalytic situation. Other independent methods are required.

I must also take issue with Dr. Sample's position. First, the psychotherapy researches he cites are based on the same presuppositions as the psychoanalytic method itself and thus cannot provide independent evidence for these presuppositions. Further, these researches do not explicitly and systematically incorporate distinctions between conscious and unconscious, or take into account the occurrence of transformations. For example, in the Luborsky research.a

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consciously stated wish is taken at face value; the possibility that the wish may be a reversal, displacement, etc. is not considered. Thus, the Core Conflictual Relationship Theme method does not incorporate significant elements of the psychoanalytic model, though there is much of importance that it accomplishes.

I would also take issue with Dr. Sample's position that bridge building and collaboration with other sciences is premature. One can find out how premature it is only by undertaking the task. Otherwise, what criteria can one rely on to identify in advance whether the time is ripe for bridge building and collaboration?

And this leads me to another quite important consideration, touched on by Dr. Case when he recalled the dismal efforts of researchers in the 1940s to demonstrate the existence of repression. To be relevant and probative, research must incorporate the essential elements of the psychoanalytic model based on the two presuppositions previously described. If the research intends to deal with such clinical concepts as defenses, transference, and conflict, it must provide that its method take into account unconscious as well as conscious processes, transformed as well as transparent contents, by which I mean presumed "substitutes by displacement," as well as presumed direct effects of unconscious causes, as in a telling slip. If the research intends to address these two basic presuppositions, it must demonstrate how it can do so independently of the clinical method based on these presuppositions. Research of this nature, to succeed, must either discover new consequences of these presuppositions independently arrived at, or provide evidence of what underlying factors or preconditions cause them. I will describe an example of each later on. These criteria constitute a tall order, but in my judgment define what I mean by a science basic to psychoanalysis. It is not at all difficult for me to sympathize with Dr. Case's position in light of these admittedly demanding criteria that successful psychoanalytic research must meet. I also very much sympathize with Dr. Sample's position on psychotherapy research, because the task the psychotherapy researcher faces is a formidable one and the successes he cites must be judged against the exacting standards that must be applied. On the other hand, if we can become explicitly aware of the extent of the task and of its absolute necessity, we are in a much stronger position to know what we need and to judge what we have accomplished.

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Now for the research. You will need to judge how relevant the findings are and how true to the essentials of the psychoanalytic model and its presuppositions. I will, of course, try to make out a case for both.

I will draw on two pieces of research conducted by Shevrin and colleagues at the University of Michigan (Shevrin et al., 1992, in press; Snodgrass et al., 1995). I will be selective and, I hope, concise and reasonably clear, but I will go into sufficient detail so that you can judge for yourselves whether the research accomplishes the ends I envision.

I argued earlier that a psychoanalytic basic science should address the fundamental presuppositions of our clinical method while remaining true to the essentials of the psychoanalytic model. I would quickly add, and then try to defend, the proposition that a true basic science of psychoanalysis can do both—contribute importantly to support fundamental propositions and remain true to the psychoanalytic model, without, I stress, itself being a psychoanalysis.

In the first study I will describe, Shevrin and colleagues invented a method drawing on three sources: (1) the psychoanalytic clinical method, but not as applied to a psychoanalysis; (2) cognitive laboratory methods involving subliminal and supraliminal procedures; (3) a psychophysiological method based on obtaining brain responses to these sub- and supraliminal stimuli (Shevrin, 1988; Shevrin et al., 1992, in press). A brief word about each and then how they fit together. The subjects for the study were mainly patients within the neurotic range suffering from social phobias. Subjects were evaluated by experienced psychoanalysts. Each evaluation observed three essential aspects of the psychoanalytic clinical method: (1) a series of in-depth, unstructured interviews were conducted in which both content and process were addressed; (2) an account of the complaint and relevant history were obtained, with attention paid to the manner in which the patient engaged in the evaluation; (3) initial transference and resistance issues were noted and where indicated addressed as a further source of diagnostic information. The essentials of the psychoanalytic clinical method was here employed for diagnostic but not therapeutic purposes. Pace Dr. Case, the interviews were also audio recorded and transcribed. A team of three analysts and one psychodynamically trained clinical psychologist

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then studied the verbatim transcripts, and each arrived at a psychodynamic formulation, including the patient's account of his presenting complaint, the patient's own understanding of the complaint, and hypotheses concerning the underlying unconscious conflict causing the complaint.

But this was not the end of the clinicians' task. They were then asked to select words or brief phrases used by the patient that conveyed the patients' conscious experience of the complaint, but also words and brief phrases that, in the clinicians' judgment, expressed in derivative form the hypothesized underlying unconscious conflict. These words might be drawn from anywhere in the protocol—reported memories, dreams, fantasies. This research application of the clinical method follows exactly the essential steps of the clinical method: (1) obtaining the basic clinical data in a psychodynamic manner (as I have just previously described); (2) organizing them into psychoanalytically relevant propositions by applying the psychoanalytic model and its presuppositions. It departs from the usual application of the clinical method in two ways that do not, however, subvert the clinical method: (1) in place of developing interpretive statements offered to the patient, words and brief phrases are selected that would be incorporated in such interpretations; and (2) in place of new clinical data usually obtained following interpretations, physiological responses are instead obtained to the words selected.

Once the words have been agreed on, the research subjects move from the consulting room to the laboratory. There the words selected are presented to the patient in a tachistoscopic viewing box. Initially the words are flashed so quickly that they cannot be seen consciously; thereafter they are presented slowly enough so that they can be. In brief, this constitutes the cognitive sub- and supraliminal laboratory method. Its importance, however, is critical to the research: it is only through the subliminal presentation of the stimuli that an operational method for investigating the *descriptive* unconscious can be achieved. No inference from the patient's conscious communication is required in order to demonstrate that the stimuli in question are indeed unconscious. They have been rendered so by the subliminal method itself. Whether or not they are *dynamically* unconscious will then emerge as a finding determined by the fate the different types of words undergo when presented subliminally and thereafter supraliminally. But more of this a bit later.

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Each time a word is presented either subliminally or supraliminally, a brain response is obtained that is electrically recorded for future analysis. On the basis of extensive work in Shevrin's laboratory and elsewhere, there is good reason to expect that brain responses related to the subliminal stimuli will be obtained (Barkoczi et al., 1983; Brandeis and Lehmann, 1986; Libet et al., 1967; Kostandov and Arzumanov, 1977; Shevrin, 1973; Shevrin et al., 1969, 1970, 1972; Shevrin and Fitzler, 1968a, 1968b; Shevrin and Rennick, 1967).

The use of brain responses is of special methodological importance as well as of substantive interest in its own right: These responses are the new consequences following from the presupposition of a causative psychological unconscious; they would constitute discoveries that support the validity of that presupposition while building a bridge to another science, neurophysiology.

In the research these brain responses are analyzed into sequences of time-frequency features (Williams and Jeong, 1989). When applied to the data this mode of analysis yielded significant patterns or sequences of five brain frequencies at specific points in time, not too dissimilar from a melody—which is, of course, no more than a sequence of auditory frequencies called notes. When these sets of five time-frequency features are analyzed, striking differences emerge. When the unconscious conflict words were presented *subliminally*, the time-frequency feature analysis showed they are associated with each other. When the same words were presented *supraliminally*, the brain responses failed to show them as associated. If I can put it in these terms and get away with it, what the brain/mind “knew” unconsciously, it seemed not to “know” consciously, but only for the words related to the unconscious conflict as selected by the clinicians beforehand.

Let me tell you briefly about one subject, a young man of twenty suffering from a public eating phobia. The clinical team agreed that the underlying unconscious conflict was oedipal in nature, with prominent negative oedipal elements. The phobia was related to the regressive homoerotic wish to submit to an oral phallic attack and the great fear of it. The words for the conscious experience of the symptom were plain enough from his account; they were words such as cafeteria, swallowing, nauseous. A primary source for words related to the unconscious conflict derived from a disturbing dream

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in which he was stabbed by a close male friend, John, with a sword that was thrust upward through his throat while he was lying on his back. As he told the dream to the interviewer, he felt a sensation in the same throat muscles that would tense up and inhibit his swallowing in a public eating place. The telling of the dream was itself a revealing transference communication. It is also notable that John was the same friend from whom he had taken a girlfriend away; once having done so, he lost interest in her, a pattern he had repeated with several friends. He had in fact taken the same girl away from John twice, given her back each time, and each time returned to a close younger brother relationship with John in which much carousing without women occurred. Some unconscious conflict words selected were *John, stab me, on my back* from the dream, as well as several words like *massaging muscle* and *men hugging*, drawn from elsewhere in the protocol and derivatives of the hypothesized regressive homosexual position.

When the brain time-frequency features were analyzed in greater detail, it turned out that there was one pattern for unconscious conflict words and another for conscious symptom words. For the unconscious conflict words, the highest and lowest frequencies traded places in the five-note "brain melody" as a function of consciousness; subliminally the *highest* frequency was early, the *lowest* frequency later; this order was reversed supraliminally. For the conscious symptom words, subliminally it was the *lowest* frequency that occurs early and the *highest* frequency later; the reverse of this order was found supraliminally. The "brain melodies" for conscious symptom and unconscious conflict words were *inversions* of each other. When one goes up, the other goes down, and it all depends on whether they are related to consciousness or not. The temporal organization of neural processes differs dramatically as a function of consciousness and conflict.

To summarize, the research—based in part on the psychoanalytic clinical method, but conducted in such a way as to test the presupposition of a causative psychological unconscious—resulted in the discovery of unique patterns of brain responses supporting the validity of that presupposition while at the same time potentially building a bridge to neurophysiology.

The second experiment I would like to tell you about speaks to the second presupposition: that unconscious causes are revealed

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through manifest expressions, or transformations of the content of these unconscious causes in the form of displacements, superficial associations, and symbols.

Let me begin with an old but useful saw in the form of a question: When is a cigar a cigar, and when is it a phallus? When do we judge that the patient is really talking about cigars or that he is really talking about phalluses? Or, in technical terms, when do we have warrant to infer from conscious communications that the explicit communication is really about something else that is stirring unconsciously.

From a cognitive psychological standpoint, the problem can be recast, interestingly enough, as a question of categories: When is a cigar mainly categorized with other smoking items, and when is it mainly categorized with phallic objects? Shevrin and colleagues posed two questions: (1) Would the principles of categorization be different unconsciously as compared to consciously? (2) Would the presence of unconscious conflict make a difference with respect to these principles? (Note how the research incorporates the essentials of the psychoanalytic model by including distinctions between conscious and unconscious, unconscious conflict, and categorical transformations.) The research in progress I will describe concerns only the first question at this point. The words to be categorized were entirely conflict-neutral and the same words were given to all subjects. Two principles of categorization were played off against each other: categorization on the basis of similar relationships *among* attributes and similarity of *individual* attributes. For example, a whale is a mammal because of complex relationships among such attributes as giving birth to live young, nursing, evolutionary history, etc; it also shares a number of attributes with fish—fins, living in the ocean, etc. One might consider the latter basis for categorization as superficial and even misleading in the light of our fuller knowledge. In the experiment, you shall see that these two principles are played off against each other with provocative initial results.

Suppose the word *pea*, presented initially either sub- or supraliminally, is paired with one of four words presented supraliminally only—*spinach*, *carrot*, *emerald*, *ruby*. In both conditions, sub- and supraliminally, each of these four words is presented twice with *pea*; once with the question *vegetable?* and once with the question *green?* The

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question *vegetable?* requires categorization on the basis of complex relationships among attributes—i.e., edible, form of plant, etc. The question *green?* requires categorization on the basis of a single attribute, color. The subject is to respond yes or no as quickly as possible to the question by pressing the appropriate button. How will the previous presentation of the word *pea* affect the time to answer the categorizing questions, and will it make a difference if *pea* is sub- or supraliminal—that is, if the categorization of *pea* with each of the other words is prompted unconsciously or consciously? Note that *spinach* is both a vegetable and green; *carrot* is a vegetable and not green; *emerald* is green and not a vegetable; *ruby* is neither. Thus, these four words cover the four different relationships possible with *pea*, for the two types of categorization. In the experiment, fifteen such sets of words were presented.

Categorization turns out to be different sub- and supraliminally in an interesting way: When *pea* is subliminal, categorization by single attribute, in this case *green*, is much faster than when *pea* is supraliminal. The answer yes to the question *green?* for *emerald* is made much faster sub- than supraliminally—in fact, much faster than to the multiple attribute question *vegetable?* for either *spinach* or *carrot*. Subliminally prompted categorization appears to favor single attributes over complex relationships among attributes—the whale as fish rather than as mammal. To go back to cigars and phalluses: when an unconscious cause is at work, it is the shape of the cigar, a single attribute, that is more likely to be used as a basis for categorization, while in the absence of an unconscious cause it is the complex relationships among features—made of tobacco, capable of being smoked, etc.—that provide the basis for categorization.

Since these results are based on conflict-neutral words, it is possible that categorization by single attribute is the way categorization works *unconsciously*, and that more dynamic unconscious processes “use” the principles already available unconsciously rather than introducing new ones. Additional experiments will need to address this question.

The research provides independent support for the second pre-supposition of the psychoanalytic method, that unconscious causes are inferred from so-called displaced and substitute formations. This might now be restated as follows: unconscious causes are revealed

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through properties, such as color or shape, that are superficial or inessential and that are often unrelated to the function or customary meaning of the items. It is these superficial associations that constitute the basis for unconscious categorization. In fact, Freud borrowed the term "superficial associations" from the psychologist Wundt as a way to characterize primary process displacements. This new evidence suggests that even neutral stimuli are categorized through what appear to be superficial attributes when the categorization goes on unconsciously.

To summarize, the second experiment identifies an underlying cause or precondition of the second presupposition having to do with substitute formations. The underlying causes are differences in categorization, a basic psychological and not uniquely psychoanalytic process, and thus the second presupposition is shown to be related to the psychology of categorization, another potential bridge.

As I have tried to illustrate, other sciences *can* be brought into creative interaction with clinical psychoanalysis. The first experiment demonstrates that other sciences, in this instance neurophysiology, can be basic insofar as they discover new consequences of psychoanalytic presuppositions, thus providing independent convergent evidence for their validity. The second experiment demonstrates that other sciences, in this instance cognitive psychology, can be basic by identifying preconditions for these presuppositions and in so doing increase our understanding of their nature.

Case: As I listened to your exposition of these experiments, and I know that you cannot do them full justice, I must confess I am reminded of those complicated Rube Goldberg devices for opening a door, when all you really have to do is turn the knob. Yes, you can open the door with a Rube Goldberg device, but why do it that way? Your arguments about presuppositions are a bit overdrawn. What if I were to say that they are reasonable generalizations based on clinical evidence and not assumptions at all? Also, I am not convinced that the so-called clinical method in the first experiment does incorporate the essentials of the psychoanalytic model. Only a psychoanalysis can do that. Yet, the brain results are fascinating. What exactly they are about, though, is not so clear. I am sure that more research is on the way and we will one day hear about it.

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Sample: It's a long way from brain responses to a psychological process, especially one as complex as the dynamic unconscious related to unconscious conflict. How do these intriguing brain frequencies translate into psychological meaning? At best, these are correlations across the great divide separating mind and body. You have not proposed any bridging concepts. I don't yet see how "time-frequency" features and "brain melodies" help us much in this regard. As best as I can see, the results you have cited do provide some convergent evidence on the basis of independent methods for certain basic presuppositions. But the methods and measurements do not yet define the essential intervening steps.

Link: Most new scientific methods when they are first created seem laborious and indeed can seem rather like a Rube Goldberg contraption. The first computers were massive and filled entire rooms; now they can fit on your lap. But those early mammoth computers embodied for the first time significant principles of lasting importance. But when you claim, Dr. Case, that rather than being presuppositions these propositions are really empirical generalizations; you have in fact demonstrated how presuppositions arise in the first place—from usually unsystematic observation or even prejudice. But no matter how these propositions arise, it is logically necessary that they be independently confirmed and their place in the larger scheme of scientific understanding situated (Brakel, 1994). As for Dr. Sample's admonition concerning the failure to specify the links intervening between brain and mind, I respond by saying that a bridge isn't built in a day. Nevertheless, I think it would be correct to assert that Shevrin's research has sunk piers on either side of the great divide Dr. Sample refers to and that these piers are in fact across the way from each other so that we at least know where the span should be erected and even some hint concerning the nature of the bridging materials—patterns of brain frequencies and principles of categorization.

Di Sapienza: Now it is my turn. I have listened intently to all of your arguments. Ironically, I can advance my position that psychoanalysis is not a science at all, albeit a valuable rational enterprise, by citing in turn each of your own objections to the other two positions. Dr. Case has already dismissed psychotherapy research as unnecessary meddling in the psychoanalytic process. He has argued

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forcefully against any science being basic to psychoanalysis because the psychoanalytic method is, in his view, a unique *investigative* method through which whatever is basic can only be discovered and tested in the psychoanalytic situation. Dr. Sample points out eloquently that Dr. Case is forced to defend a science without truly public data and without independent means to demonstrate the probative status of its hypotheses. He scores against Dr. Link by asserting that science can progress for years without paying attention to its method or presuppositions. In any case, he argues that efforts to develop fruitful interactions between psychoanalysis and other sciences, basic or not, are premature. Dr. Link argues against Dr. Case's position that psychoanalysis is a science *sui generis* because admittedly it is based on presuppositions for which we need independent support and against Dr. Sample's reliance on psychotherapy research as short-sighted and sorely limited by its failure to incorporate essential elements of the psychoanalytic model.

Let me now confront you with the inescapable implication of your obvious standoff: You can't agree on exactly what way psychoanalysis is or can be a science, because in fact it isn't a science at all.

Case, Sample, and Link: No!

Di Sapienza: No matter how loudly you may all shout "No," it will not make it so. I have heard interesting argumentation and intriguing experimental results, but I have yet to be convinced how it all hangs together. Quite otherwise, you seem hardly to have in any way modified your individual positions in the light of what you have heard. It seems rather to me that you will leave this room reinforced in your own opinions. You will carry on with your analytic practices convinced, perhaps rightly, that you bear the future of your profession in your hands. Dr. Sample's handful of psychotherapy researchers will ply their recondite trade on the few fully recorded psychoanalyses available, and talk to each other while bemoaning the lack of interest of their clinical colleagues. What Dr. Link's basic psychoanalytic scientists will do is beyond me. I doubt whether they will have anyone to talk to; perhaps their fate is to talk to themselves and, like Beckett's Krapp, obsess over their pretty results with increasing despair.

If I cannot convince you to chuck overboard this superfluous scientific baggage, about which pieces to keep you cannot in any way

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agree, and take your rightful place in the ranks of the powerful agents for rational change in our society, then you must put your heads together and arrive at some mutual understanding. If psychoanalysis is in fact a science, and if it is as sturdy and self-reliant as Dr. Case insists, and as ready for closer systematic scrutiny of its data as Dr. Sample believes, and as rich in its conception of the human mind and as capable of benefiting from the methods and findings of other sciences as portrayed by Dr. Link, then perhaps some day you *could* convince me of the scientific status of psychoanalysis, if—and this is a big if—you can deliver the goods. Although for the reasons I have already given, you have not yet convinced me, I do believe you have much to offer to each other. The first bridges I would build are those connecting your different positions.

Case: I suppose it is worth a try. I have certainly enjoyed these interchanges, though I must confess I retain a bit of skepticism about where it will get us.

Sample: As long as it doesn't hold us back from what really needs doing. There might be some gain if in fact Dr. Link's optimism about bridge building is warranted, as perhaps the research he cited suggests.

Link: I have myself been excited by this opportunity to see the diverse pieces of the analytic puzzle laid out so clearly. I would like very much to see how we can put these pieces together and, if need be, find the pieces that are still missing. The rich intuitive skills and theoretical grasp of the experienced analyst, the careful, systematic attention to the nitty-gritty of the clinical process practiced by psychotherapy researchers, and the power of bringing to bear different and complementary methods to psychoanalytic issues by experimental scientists all working together holds forth the promise of scientific progress.

Di Sapienza: The time is getting late. Perhaps next summer we can meet again and see what the intervening year has brought to light. Thank you and good luck!

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Introduction: Lawrence Friedman

How scientific is psychoanalysis, and what sort of research is relevant to it? Today's harsh public scrutiny and bare-bones health financing bring new urgency to the old question.

For his plenary address to the American Psychoanalytic Association, Howard Shevrin has devised a playful and powerful teaching device that allows the reader to hold in imagination all four of the principal conflicting beliefs regarding psychoanalytic research. Shevrin's dramatic dialogue renders clearly visible the elements of the controversy, which are then measured against fascinating examples taken from his own research.

We have added another seven voices to Shevrin's four, and from all of these it is likely that readers will first identify their own position, then come to question it, and finally discover positions they had never thought of. They may also find that arguments about scientific status have a way of picking out shades of meaning within concepts (such as the unconscious) that might otherwise have gone unnoticed.

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Wilma Bucci

The action takes place at the home of Dr. Nomological Network, in a newly renovated loft building in Soho, during the Labor Day weekend. We first meet Drs. Hermeneut and Philo. They enter a large elevator, decorated like a Victorian bird cage. Dr. Philo speaks first.

Philo: Haven't seen you in a while; where have you been? I felt sure you would have been invited to those soirees (*spoken with a deeply rolled r*) of di Sapienza's in Amagansett. I of course never leave the city in the summer.

Hermeneut: Who is di Sapienza?

Philo: I'm not surprised you haven't heard of her. She thinks of herself as something of a philosopher of science. My colleague, Dr. Link, who apparently has some interest in her ideas, has told me that she views psychoanalysis as a rational (*sic*) enterprise—comparable to teaching or governing—whose subject matter is humanity, and which is dedicated to bringing about adaptive change. It is distinguished, in her view, from applied or basic sciences, which require replication and systematic testing, and quite different from your kind of work, Hermeneut. It is also distinguished from art, in that psychoanalysis works with material that has a will and purpose of its own, while the artist works with inert material, which she transforms in the light of her own aesthetic vision.

Hermeneut: Enough, I get the idea. Perhaps di Sapienza can justify her view that education and government are rational enterprises, though I would expect someone who lives on Long Island to know better. However, I must say that she seems mired in a predeconstructionist conception both of art and of my work. I have much more I could say about her ideas, as I am sure you do, and this is all very interesting, but haven't we been in this elevator quite a long time? I believe our position in this shaft has not changed. Is there some meaning for this? (*They begin to look around them.*)

The scene shifts to the lobby of the building. Entering together are Dr. Brain and Dr. Mind. They notice an "out of order" sign near the elevator, infer that it is not working, and decide to take the stairs. Mind speaks.

Mind: Times have certainly changed, for us to be invited to the same gathering. It used to be that anyone who was interested in one of us would have ignored the other, and I was generally not invited to respectable scientific meetings, anyway.

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Brain: We can thank our host, Dr. Nomological Network, for bringing us together. (*They ring the bell and enter his loft, which is in the process of construction.*)

Network: I assume you know why I have brought us together today. You have seen the report of the intellectual extravaganza organized by di Sapienza in Amagansett. Of course, I'm not surprised she failed to invite me; she might have had to read something written during the past half century to know about my role. Anyway, I never leave Manhattan in the summer. I wouldn't have given the whole event another thought, but it seems to have aroused some general interest in the field, and I think we have an obligation to continue the discussion so as to present a more balanced view.

I'd like to talk first about the presentation of Dr. Link, and we can talk about Case and Sample when the others arrive. As a cognitive scientist interested in psychoanalysis, what do you think, Mind? By the way, I hope you don't mind (*he chuckles softly*) my calling you Mind. I admire your "coming out," although I imagine you still make some psychologists and philosophers of science a bit uneasy, particularly those who have not accepted the cognitive revolution.

Mind: Of course it is largely because of you that I have been able to "come out," as you put it, as Bucci (1989, 1993) has discussed.

Network: Thank you. I do not like to grumble, but I do sometimes wish that more people understood that, from the perspective of science, mental representations and processes have the status of theoretical constructs, which are defined through links to other concepts, and through operational indicators, events that may be jointly observed. Psychological constructs have the same theoretical status as particles, dark matter, the big bang, and life in the Bronze Age; all are constructs, defined within, may I say, a nomological net. All sciences depend on the construction of such a theoretical framework. I assume Link understands this, though he did not see fit to cite me directly.

It is obvious that the mental representations *of other persons* must always be inferred; each of us does that constantly, in intuitive, at times unrecognized, ways. Science recognizes the need for systematic development and validation of such inference. If only people appreciated the power of my approach in this respect. Subjective meanings can indeed be part of a scientific theory, once a theoretical

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framework has been set up, within which concepts may be defined and systematic inferences made; subjective experience cannot be part of a scientific enterprise without such a framework. The failure to develop systematic definitions for experiential terms has contributed to a splitting of psychoanalysis from the scientific field, and has even drawn some leading psychoanalytic researchers to follow in the direction of Dr. Hermeneut. (I wonder where he and the others are, by the way; oh well, perhaps they are walking down the garden paths.)

Mind: You are preaching to the converted here. The application of your approach to mental representation and information processing is the day-by-day work of the cognitive science field. We recognize that nomological networks in the social sciences differ from those in the physical sciences in that the hypothetical constructs and correspondence rules are far less systematically defined, and we are working to remedy this; but the status of our theoretical constructs and the principles of our scientific approach are the same as for all science. I agree that it would have helped Link's argument to have cited your work, and I will certainly discuss this point further today.

Brain: (To himself) I'm sure you will.

Mind: (Ignores interruption) First, however, I need to tell you that Bucci (1989, 1993, in press) has expanded the cognitive domain to incorporate *emotional* information processing in the models of mind, and to account for the processes and functions with which psychoanalysts are concerned. I am convinced, as she is, by the need for this reformulation of cognitive science (although I must say that not all cognitive scientists are). Many emotion theorists, such as Lang (1994), Mandler (1984), and Scherer (1984), take a similar position. So let me ask that you now recognize me as Emotion-Mind. Emotions, like mental processes, as they figure in a scientific theory, are hypothetical constructs, defined in terms of other constructs and in terms of observable behavior, within a theoretical framework or, as we say now, a nomological net. "Rage" and "fear," "happiness" and "love"—all are constructs in this sense. This does not diminish their emotional meaning, but allows it to be studied in a systematic and productive way.

Network: I accept the correction, Emotion-Mind. But now what about the work of Dr. Link?

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Emotion-Mind: I applaud Link's attempt to integrate psychoanalytic concepts with cognitive ones. However, I believe his call for two separate psychoanalytic sciences, one basic and one applied, is misguided, and indeed reflects his not acknowledging your role, Network. Psychoanalysis is neither "two sciences" nor even "a science." It is an aspect of scientific psychology, the aspect that concerns the interaction of mental, emotional, and somatic functions in an interpersonal context. Psychoanalysis was the first cognitive science (Baars, 1986; Erdelyi, 1985), but fell into the trap of focusing on certain types of cognitive-emotional contents without accounting for their integration within the overall functioning of the human organism. Interestingly, this is the same trap into which many cognitive psychologists have also fallen, from the opposite direction, in attempting to account for cognitive functions out of the context of emotional and somatic ones. The theoretical framework for psychoanalysis must emerge from a general psychological model that accounts for the interaction of cognitive, emotional, and somatic processes. A systematic account of pathology and of the process of change in treatment may then be developed within that general framework (Bucci, 1993, in press).

Brain: You've already taken up so much time here that I will not have time to outline my approach. That's okay, it's your show, Emotion-Mind, but let's at least try to be clear about the relationship between us. May I say first that the scientific approach that you outline applies to neurophysiology as well as to cognition and emotion. Neurophysiological, like cognitive constructs, are hypothetical entities, defined conceptually within a theoretical framework in terms of their relationship to other neurological constructs and defined operationally in terms of observable events. In other words, I am as much a construct as you.

Network: What you say is very important, Brain. Each theoretical level chunks the functional system in a different way, and each is needed to account for interactions in its own domain. Scientists do not use computations involving particles to study the forces that will enable them to build a rocket to the moon, although the action of particles is involved in all physical systems. As we can see, what we have is not a "mind-body problem" but a set of different types of theoretical relationships—among constructs of mind and emotion,

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their neurophysiological substrate, and somatic-visceral events—as Bucci (in press) has discussed. While psychological theories are not *reducible* to neurophysiological ones, we of course agree that the two levels must ultimately be *translatable* or mappable onto one another. The research of neuropsychologists such as LeDoux (1989) has provided promising and fascinating physiological data concerning the type of interaction between brain and body that is intrinsic to psychoanalysis, and that we study on a psychological level. We also need to remember—as I am sure you both are aware—that theories do not account for other theories; they account for observable events. Thus a theory of brain does not account for a theory of emotion or mind; each accounts in its own way for certain observable events.

Emotion-Mind: I couldn't have said that better myself. I am sure you will agree, Brain, that physiological theories will necessarily take a back seat to psychological ones, in this enterprise of developing a theory of cognitive and emotional functions, which may be applied to the data of the psychoanalytic process. We require psychological models to account for the psychoanalytic process, although we know, on another level, that physiological functions are involved in all mental and emotional ones.

Here is another twist for you to chew on (since our host has not yet seen fit to provide more concrete nourishment). We should recognize that neurological observations themselves may constitute observable data that provide evidence for a psychological theory, just as behavioral evidence may provide evidence for neurological theories. The former is precisely what Shevrin is doing in his experimental paradigm. He is not linking neurological and cognitive *concepts*; he is using neurological observations—along with behavioral ones—as evidence concerning psychological propositions.

Network: Can we move on now to discuss Shevrin's experimental work, as Link presents it. What do you think of Shevrin's research, Emotion-Mind?

Emotion-Mind: I must say that his results are so elegant and complex that one is first somewhat awestruck. Nevertheless, one may find reason to cavil—not to mention quibble—about certain points, in spite of the beauty of his experimental work.

Brain: (*sotto voce to Network*) Perhaps *because* of it?

Emotion-Mind: (*ignores Brain*) There seem to be some inconsistencies in his findings, though of course this may be a problem in

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Link's presentation. One possible inconsistency seems worth mentioning. At one point Link says that when unconscious conflict words were presented supraliminally, "the brain responses failed to show them as associated." Later, however, he says, "For the unconscious conflict words, the highest and lowest frequencies traded places in the five-note 'brain melody' as a function of consciousness; subliminally the *highest* frequency was early, the *lowest* frequency later; this order was reversed supraliminally." In other words, "The 'brain melodies' for conscious symptom and unconscious conflict words were *inversions* of each other." The latter formulation, of a systematic inverse organization for unconscious conflict words when presented supraliminally, is quite different from absence of association, and does not seem readily accounted for by Shevrin's theory.

The results of Shevrin's experiment on principles of categorization are also fascinating. There are many questions and problems about this interesting research paradigm and its findings, some of which may arise because of the superficial nature of Link's presentation. Shevrin's results suggest that unconscious categorization is dependent on properties such as color and shape, which he characterizes as "superficial and inessential," whereas conscious classification is more likely to favor what he terms "complex relationships among attributes." Thus, for example, association to a single attribute, such as *green*, is faster when the word *pea* is presented subliminally than when *pea* is supraliminal. Also, for subliminal presentation, associations to single attributes are faster than to complex categories such as *vegetable*. We would then like to know how speed of response varies for different category types in *supraliminal* presentation. Link implies that supraliminal presentation would favor association to multiple attribute categories such as *vegetable*, but does not provide evidence to support this. This point would be important in building a consistent model of subliminal vs. supraliminal organization.

Along these lines, I would also like to mention that Bucci's multiple code theory (1993, in press) would place Shevrin's results in a more general cognitive framework. From her perspective, properties such as color and shape are not at all "superficial" or "inessential," but are the ways in which the nonverbal representational system is organized; this organization plays a crucial role in the process of

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symbolizing emotional experience. Organization into higher-order categories, such as *vegetable* or *plant*, is a function of the verbal symbolic system; it is a different type of organization from that which obtains in nonverbal systems, but not necessarily a more essential one. Viewing the categorical distinctions in this more general, information processing framework opens specific questions as to how subliminal processing may operate to favor nonverbal and subsymbolic processing systems, which were not addressed by Shevrin or Link.

This brings us to the basic issue of how we view the overall organization of the human emotional information processing system, and leads to the more general questions that I have about Shevrin's work. The major information processing architectures today, including symbolic and parallel distributed processing models, are based on aspects of mental structure *independent of their accessibility to awareness*. Similarly, within the functional rather than architectural approaches, cognitive scientists focus on distinctions that are independent of level of awareness, such as procedural vs. declarative memory, and episodic vs. semantic memory. It is also true that cognitive scientists today are interested, more than ever before, in systematic memory functions that are implicit, operating outside of awareness, and have raised new questions about the construct of consciousness and its characterization within a scientific model (Uleman and Bargh, 1989). From this perspective, they are beginning to tease apart the complex relationships among various functional dimensions that have been customarily assumed to covary, such as awareness, intention, automaticity, and control.

On the basis of current research, we must recognize that the construct of the unconscious can no longer be assumed as necessarily associated with pathology, regression, infantile thought, and wish fulfilling fantasy. From the psychoanalytic perspective, we may also note that the causative psychological unconscious, as defined and tested in Shevrin's paradigm, does not derive from either the topographic or the structural model of the psychic apparatus. In assuming accessibility to awareness as the major determinant of mode of cognitive processing, Shevrin appears to follow the topographic model. However, in relying entirely on verbal stimuli, he assumes linguistic organization in the unconscious, which is obviously at variance with

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the topographic formulation. Thus, his work cannot be said to test any existing psychoanalytic theory, though it yields results that seem of considerable interest to the psychoanalytic approach.

From either the basic cognitive or the psychoanalytic perspective, it seems crucial that we systematically formulate the model of the mental apparatus that we are testing, in order to understand the meaning of the empirical results. If there is indeed a causative *linguistic* unconscious, as Shevrin's work would indicate, the systemic implications of this within psychoanalytic theory need to be traced. If processing dimensions other than accessibility to awareness are more basic, as many cognitive scientists claim, the implications of these must be traced as well.

The doorbell rings. It is Dr. Qual.

Qual: Sorry to be late. I was trying out some alternative paths. By the way, I noticed a light flashing by the elevator and pounding coming from there. I wonder if someone would like to verify that observation.

Network: I will investigate; why don't you carry on? You probably all know Dr. Qual and his methods of discovery-oriented qualitative research. (*Exits*)

Emotion-Mind: We were just talking about Shevrin's work, which Link discussed at di Sapienza's happening—to which of course none of us was invited.

Qual: I wasn't surprised not to be asked. I rarely am included anywhere, although things have been getting a bit better lately, with respectable psychologists like Elliott (1994) and Mahrer (1988) speaking up for me. Of course, I never leave the city in the summer. I have quite a lot to say about the discussions at the di Sapienza meeting, but I am a bit out of my element in talking about laboratory research methods. Can we turn instead to the work of Drs. Case and Sample? I believe that Link does not sufficiently value the psychoanalytic situation as a research context, and therefore . . .

Emotion-Mind: I agree with you completely; that is part of the point I have been trying to make. Psychoanalysis is a part of scientific psychology, neither a separate basic science nor an applied science, although we may, of course, expect that clinically useful results will emerge from psychoanalytic research. Link appears to deny, or at least to downplay, the basic scientific potential of systematic psychoanalytic process research, and classifies this instead as applied research. In the context of current cognitive science, I would have to

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argue against this view. Cognitive scientists today have moved away from their initial reliance on laboratory experimentation and computer modeling to a recognition that most complex mental functions (at least those carried out by protoplasmic information processors rather than transistorized ones) need to be studied in naturalistic or quasi-naturalistic contexts. They are "discovering" research methods such as "protocol analysis" (Simon and Kaplan, 1989), which are strikingly similar to techniques that analysts and psychoanalytic researchers know (Bucci, in press). Of course, cognitive scientists fail to recognize the roots of protocol analysis in the psychoanalytic technique of free association, just as they fail to recognize their debt to psychoanalysis in a more general sense.

We need to use our strengths, not deny them, if we are to make a contribution to scientific psychology, and also to provide a systematic basis for addressing clinical questions. In contrast to Link's position, my claim is that the psychoanalytic situation provides a controlled, naturalistic research context ideally suited for testing the propositions of a general theory of emotion and mind. In the context of a consistent psychological model, measures developed by Luborsky (Luborsky and Crits-Cristoph, 1990), Weiss and Sampson (1986), Dahl (1988), Perry (1993), Bucci (1993), Mergenthaler (1992), and other psychoanalytic process researchers may be understood as operational indicators of significant mental and emotional constructs. By this means, it is also possible to define the nature of pathology and the goals of treatment, and to identify and evaluate mutative factors in the treatment in systematic ways.

Qual: That is very interesting, but . . .

Emotion-Mind: If I may make just one final point. While I agree with Dr. Case and Dr. Sample that it is important and quite feasible to carry out systematic research in the therapeutic context, I must also agree with Dr. Link that experimental research will contribute as well to the development of psychoanalytic theory. However, I cannot say too often that . . .

Qual: Apparently not . . .

Emotion-Mind: As I was trying to say, this approach requires that concepts as examined in the experimental setting have the same meaning as these concepts as clinically understood. The debacle of the experimental studies of repression in the 1940s, to which Case

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refers, occurred precisely because the process of repression, as studied in the laboratory, was quite different from the psychoanalytic concept of repression. We have seen a similar theoretical problem in the implicit characterization in Shevrin's work, of the unconscious as including linguistic components.

Unfortunately—and this brings me to a disagreement with Dr. Sample—the same problem arises for clinical concepts as studied in much psychotherapy research. For example, the clinical concept of *transference* is defined operationally as a Core Conflictual Relationship Theme in Luborsky's terms, as a judgment of disguised allusions by the patient to the relationship with the therapist by Hoffman and Gill (1988), and as a frame structure by Dahl (1988). In the absence of a systematic general theory, we have no way of knowing to what extent these concepts correspond to one another, much less to the notion of transference as clinically understood. Both experimental and psychotherapy process studies may potentially contribute to our understanding of psychoanalytic concepts, but only provided these are systematically defined in a theoretical framework or nomological net. Your type of research may contribute to the nomological net as well, Qual, do you not agree?

Qual: Yes, but . . .

Emotion-Mind: It seems to me that Link's critique of Case's and Sample's work ignored some new developments and possibilities, including qualitative research methods. Of course, that is partly because Sample himself does not tell us very much about the power and flexibility of single case designs. From a scientific perspective, the problem with the traditional case report is the absence of a public record that can be examined by observers other than the two participants, not the intensive focus on a single case. Spence (1993) has discussed possible ways to build checks and balances into the case report method. Fonagy and Moran (1993) have summarized a variety of scientific single case research designs, including qualitative as well as quantitative methods. Dr. Qual, you have hardly spoken; perhaps you would like to tell us about some of these new techniques.

Qual: Thank you; you are too kind.

Just then Network returns, with Philo and Hermeneut.

Network: I'm very sorry about your being trapped. But did you not see a sign that said the elevator was out of order, and another that said what to do in emergencies?

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Hermeneut: We weren't sure about the meanings of those signs.

Philo: It doesn't matter; we were deep in conversation.

Hermeneut: I need a drink.

Network: I am a terrible host to have you all here for so long without offering you anything. I have a complex Meritage from California, which I have been saving for this occasion; may I offer you some?

They all drink and discuss the wine for a while, each in his own way.

Qual seems eager to resume their previous discussion of science.

Qual: I am glad to have Dr. Hermeneut here; I have been wanting to speak with him. I may have more in common with him and, in one sense, with Case, than perhaps the rest of you do. I know that some of you are unlikely to remain quiet long enough for me to explain the qualitative approach. However, I suggest that it might be useful for Case to become aware of the possibilities of this paradigm; he and his colleagues may be able to systematize their observations using my techniques. Qualitative research focuses on the development of systematic means for analysis of the type of observations that have been made intuitively. The goals of qualitative research include identification and description of relevant variables and dimensions, and discovery of relationships among them. We rely extensively on narrative descriptions and text interpretations, and systematically incorporate the perspective of the observer, his problems and concerns, and his relation to the subject matter, in developing our category systems.

Of course—and Case would need to recognize this—such techniques do not obviate the need for public records or for shared observations, but make such verification more than ever necessary. The fundamental principle of good qualitative research is what Campbell and Fiske (1959) called “triangulation”: using converging data from multiple sources. The possibility of public and replicable observations constitute the sine qua non for qualitative research, as for all scientific research. At the very least, this generally requires recordings that permit a public record of a treatment; we cannot depend on the single report of a privileged observer, whose own involvement is complex. Along these lines, an excellent example of qualitative research is the study by Eisenstein et al. (1994), which included analyst, patient, and multiple observer reports. Much of

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the work of psychotherapy process researchers, including identification of clinically meaningful variables, and development of scales, category systems, and rating procedures, may be seen as within the qualitative research paradigm. Only after the qualitative studies have been done to identify and define relevant variables, dimensions and categories, can systematic and meaningful quantitative designs be developed.

I certainly recognize the problem of intrusion of observers into the therapeutic relationship; one could argue that therapies that are taped are a distinct and unrepresentative set. Of course, one could make the same claim concerning any supervised treatment. This is an interesting dilemma but, I claim, a soluble one within the qualitative approach. Recognition of the impact of the observer on the phenomena being observed is central to the qualitative research paradigm, and techniques are available to account for this. In general, the treating analyst needs to be vigilant for derivatives of the recording, and to address these in the treatment; the research needs to examine the emergence of these derivatives and their vicissitudes in the treatment process.

Emotion-Mind: I agree with you completely as to procedures. At certain stages, we must use research techniques that are oriented toward discovery rather than hypothesis testing, and must build the nomological network interactively in this way. However, I see your approach not as a separate paradigm but simply as a stage in creative scientific thought. If we are clever enough, we can develop research designs that account systematically for the impact of the observer. For example, one could ask an analyst to begin a long-term treatment taking detailed process notes, then later to begin recording, and carry out systematic comparisons of the material in the different phases of treatment and in the two methods of recording. If you do your work systematically, Qual, you could become one of us.

Qual: Thanks a bunch.

Network: I know I missed part of the conversation while I was attempting to locate Philo and Hermeneut, but I think I can infer the major concepts from what I have heard. I will also try to integrate what we have said here with the proceedings of di Sapienza's group. We need to develop a general theoretical framework within which

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basic clinical concepts, including the nature of pathology and the process of change in treatment, may be defined. This needs to be a framework of general *psychological* concepts applying to all types of contents, not psychoanalytic contents alone. Assuming the concepts of the theory are systematically defined, in terms of one another and in terms of observable indicators, observations of language, behavior, and somatic expression in naturalistic contexts, such as the psychoanalytic sessions beloved of Sample and Case, as well as observations in the experimental contexts used by Link, will all serve to build the nomological net. Qualitative as well as quantitative research designs contribute in their own ways. Neurological data may also be used as evidence for cognitive and emotional events, as Shevrin has done. I notice Brain has not spoken for a while, but I can see that he is registering what is being said.

Brain: I understand that you are talking here about a psychological model, and a neurological theory does not explain the psychological one. Theories explain data, not other theories. Your theory is not reducible to neurons, but operates on a different level. I will just say that the neurological theory also predicts and explains observations of language, behavior, and somatic events, and in some cases perhaps better than the psychological models we have today.

All speak at once. Philo becomes somewhat aggressive at this point.

Philo: None of you has made a clear statement about the epistemological and ontological status of psychoanalytic propositions and the probative value of psychoanalytic data. In addition, I am not at all convinced by Network's, Emotion-Mind's, or even Brain's cavalier disposal of the mind-body relationship. We cannot turn away so easily from issues of translatability and reducibility. We also need to address issues of explanation, prediction, and clinical inference, not to mention transduction, construction, reconstruction, and deconstruction.

Emotion-Mind, Brain, and Qual (in unison): Be quiet and let us do our work.

These three walk together to the window. Hermeneut sits quietly in the corner, holding his wine up to the light. Philo continues to formulate his arguments. Network sits and listens to him for a while, then joins the three at the window, observing the passing scene.

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One of the very first meetings of the Chicago Psychoanalytic Society that I attended as a fledgling candidate was highlighted by a presentation by Howard Shevrin. I remember none of what he said but of late that is a common enough condition of mine that it bears no connection to the excellence of his paper. But I do remember that Merton Gill rose to say something provocative about science and psychoanalysis, and I clearly remember Shevrin's plea not to divide science into two modes or forms: the Geisteswissenschaften and the Naturwissenschaften. Since I could then neither spell nor comprehend those words, I was mightily impressed with both Shevrin and Gill, and remain so to this day. Now, however, I have improved somewhat both in my spelling and my comprehension, and my respect for Shevrin remains undiminished. It remains so not only because of his continued and productive research, but also because he has kept alive those knotty questions of the proper position of psychoanalysis in the scientific community—questions that too many of us dismiss or disregard or secretly hope someone will one day solve and explain in simple, easily spelled terms. To be sure, these are primarily philosophical questions, and Shevrin's plenary dialogue is properly to be read as a discussion among philosophers who aim not so much to resolve questions as to create a state of wonder and bewilderment. And so I finished reading this plenary dialogue with

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a condition quite similar to the one with which I left that earlier meeting in Chicago. I think the best way to describe that condition is one of worry. However, I do believe and hope that that is the ideal condition for most practitioners of psychoanalysis, and so I did not feel at all burdened by the task of discussing this paper.

I worried about the paper for just a little while, because I think I rather quickly saw it as a snare: a trap for the philosophically unprepared. Though there seemed to be a conversation among three persons staking out positions for the consideration of the scientific status of psychoanalysis, I soon sniffed out a fourth party to that discussion—Howard Shevrin himself. And what he never told us, in this argument about whether or not psychoanalysis is a science, is just what a science is. Shevrin, in his presence in the background, seems to know; and in this bit of knowledge he joins with all of those other critics and friends of psychoanalysis, from Adolf Grünbaum to Robert Wallerstein. They all claim to know what science is, and I confess that I do not. They therefore are at a tremendous advantage in arguing about and determining whether psychoanalysis can properly join the ranks of science, because of my ignorance as well as the sophistication of Shevrin's discussants. Like the naive listener at a gospel meeting, I do believe! But, sadly, I seem to agree with the last person who has spoken and so am a pushover for each of Shevrin's clever protagonists.

However, not only can I spell somewhat better these days, but I have also developed a bit of competence and confidence in avoiding snares and traps. If indeed we subscribe to one or another definition of science, then I fear we shall all be befuddled and confused by the positions offered by Shevrin up to and including the presentation of his elegant research, which, I guess, is offered to show us a way to a form of freedom. But my own position, radical as it may seem at first, is that any claim to know what science *really* is is born of arrogance and presumption. That is to say that it is arrogant to assume that there exists any single, agreed-upon definition of science, and so it is fairly easy to answer the question of Shevrin's title. Let me buttress my own radical opinion with a quote from the philosopher Hilary Putnam (1987): "this appeal to the scientific method is empty. . . . there is no such thing as *the* scientific method. Case studies of particular theories in physics, biology, etc. have convinced me that no one paradigm can fit all of the various inquiries

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that go under the name of 'science' " (p. 72). Thus, I claim, like a good rabbi, that everyone is right and justified. But this is not to say that simply everything goes. Being right and correct flows essentially from a tradition, which comes from a community of scholars with shared procedures and a common vocabulary. And each of Shevrin's speakers represents a single tradition that for him or her claims an unspoken definition of science; but each such definition is different while still able to enjoy a status of correctness.

Psychoanalysis cannot be forced into a definition it is unable to qualify for, and no one is licensed to tell us what science is, because science simply is not one thing. Shevrin's article demonstrates three traditions of scientific pursuit. I suspect that none of them properly is relevant singularly to psychoanalysis, which certainly has a need for its particular scientific tradition to be clarified. However, that clarification is not presently available to analysis. It is not *sui generis*, which means it is like no other, and that precious state cannot be claimed until we know of all the others. Nor need it fulfill the criteria of "eliminative inductionism," another demand of opinion rather than of fact; nor is it two sciences. All of the friendly antagonists of Shevrin miss the point, because they start at the wrong place and/or subscribe to an implicit doctrine forced on us by many philosophers of science who are now (see, e.g., Feyerabend, 1981) seen to be simply in error about the nature of science. In truth, we today have a golden opportunity to stake out a new definition of psychoanalysis as science. We await a philosopher who will undertake that task while not chastising us for not being what someone else wants us to be. Alas, none of the people involved in the conversation was qualified to be that person.

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In his skillful and entertaining tour de force, Shevrin calls for an integration of approaches to gathering and collating information for the development of psychoanalytic theory. His dialogue morality play deftly outlines four positions on the problem of whether psychoanalysis is a science—basic, applied, or both—or can become a science by means of research techniques. He addresses the question of whether the treatment setting of psychoanalysis is by itself a means of scientific observation, or instead must be supplemented by modifying its methods and/or importing data from other fields. That is, can psychoanalytic theory be proved by a scientific method of observation consisting of the therapeutic setting, experimental methods outside that setting, or some combination of methods? His discussion and critiques of therapeutic psychoanalysis and psychotherapy research as sources of observation for the development and support for psychoanalytic theory serves as a background for presenting his own research.

By presenting these issues in the form of a debate, he demonstrates that much of our discussion on these matters emphasizes critiques, limitations, and differences among approaches. Such critiques can be supplemented by developing ideas on the ways diverse approaches to the acquisition of knowledge can be integrated, and the information gained by each method can be used to construct a multifaceted picture of mental function. I believe his paper, by pointing out what each method discussed can and cannot do, points the way to the utilization of various sources of data in formulating a psychoanalytic theory with some testable inferences.

It is readily apparent that the various pithily summarized positions refer to different notions of what psychoanalysis is, and are determined by different immediate interests and goals. These interests are different aspects of the conglomeration of concerns that are all referred to as "psychoanalysis." Shevrin's paper not only helps us to see the range of interests that term covers today, but also points to the need to clarify the relationship between the kinds of things learned clinically and through research—that is, the nature of psychoanalytic knowledge.

The particular concerns and points of view presented have to do with proving fundamental psychoanalytic assumptions, demonstrating the operation of a specific clinical phenomenon by experiment, supporting the self-validating aspect of the clinical situation,

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and—finally—the role in society of psychoanalysis as a quasi-institution. However, there are other interests that come into play in traditional psychoanalysis. Among these are the role of developmental conceptions and developmental observations in clinical work, and ways of validating particular interpretations in any ongoing analysis. Although any of these areas of interest might appear at first to belong to one of the others, I believe they do not fit neatly into any of them. The characteristics of any psychoanalytic interest under discussion will determine what assumptions, observations, and data are essential to it and available. For these reasons, any discussion of whether psychoanalysis is or isn't anything can make sense only if the specific aspects of psychoanalysis, the interests and goals, are specified.

The question of whether "psychoanalysis" is a science is difficult because of these complexities, but also because of changing ideas about the essentials of science and scientific theory construction. Much of what is said in this paper mingles discussions of what constitutes proof with considerations of method on the way to conviction based on decisive experiment. The paper understates the point that much work acknowledged as science does not involve decisive experiments or exploration of fundamental concepts. I will return to this issue later, after reframing the positions summarized in the paper.

Each of the participants in Shevrin's encounter group appears to mean something different by "psychoanalysis." Their opinions may be summarized and interpretively elaborated by the following formulations.

First, according to one view, the term "psychoanalysis" refers to the clinical situation, which is said to be a science in itself. This means that the clinical situation is the *source* of hypotheses, the *test* of hypotheses, and the *proof* of hypotheses. The resulting generalizations, which are derived from and apply to the clinical situation, are considered to be generally valid descriptions of mental function. The argument suggests that "analytic method" is, in a sense, a technology, the only method capable of producing the observed results and having its own rationale. According to this view, the data are unique and cannot be tested elsewhere. Other observations and formulations may be consistent with those made in the clinical situation, which is not, however, dependent on them for its validity. This view

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asserts that the goals and procedures of therapy and research are compatible. (See Freud [1912] pro and con on this matter.)¹

Second, "psychoanalysis" is regarded as a set of concepts *derived from* the clinical situation and the theory referring to it. These concepts can be studied experimentally in isolation so that they can meet the requirements of science for an experimental science with public data of observation to be examined by others. This would be a science of psychoanalysis that would test, prove, or modify the working ideas of the clinician. This assumes that "psychoanalysis" is a theory (or perhaps alternative theories) that can be stated as a set of propositions, with potentially publicly presentable, supporting, repeatable data, that excludes alternative hypotheses.

Third, "psychoanalysis" consists of two sciences, one basic and one applied. According to this perspective, the clinical method itself, or aspects of it, can be a source of data and hypotheses to be correlated systematically and publicly with behavioral and physiological data. Basic propositions of psychoanalysis can be tested and proved, establishing a basic science from which other propositions of psychoanalysis can be examined and demonstrated. Since according to this view *clinical* psychoanalysis is *applied* science, we might assume that the basic science may be useful in clinical psychoanalysis. This approach would depend on a chain of interpretations from clinical or quasi-clinical observations to arrive at something to be correlated with physiological data.

Fourth, there is something called "psychoanalysis" and it is a "rational enterprise," a helpful activity in which experience and judgment are useful. It is not a science because it does not have the recognized characteristics of science. From this point of view, no comment is made about the scientific aspects of the research discussed, presumably because they are not yet sufficiently integrated, encompassing, or demonstrably relevant to be considered "psychoanalysis"—i.e., the whole field.

I share the view, assigned by Shevrin to Professor di Sapienza, that the desire to say that "psychoanalysis" is or isn't a science is an issue of social prestige and, I would add, a matter of epistemological

¹"One of the claims of psych-analysis to distinction is, no doubt, that in its execution research and treatment coincide; nevertheless, after a certain point, the technique required for the one opposes that required for the other" (p. 114).

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preference. Such a question leads inevitably to some decision about the definition of a science. This is related to the role of group psychology in science as a social institution. As such, the definition is often another covert argument about social acceptability. In any case, my position in what follows is that "psychoanalysis" is a term covering too many more or less scientific, or unscientific, activities, interests, and points of view to be characterized by a single term like "science."

In each position, including di Sapienza's, a different area of activity is called "psychoanalysis," implying that in some way the limited area described represents the whole of psychoanalysis. The series consisting of rational enterprise/clinical situation/clinical science/basic science, starts with rational social concerns and ends with fundamental laboratory science.

This series might be described as going from a focus on the most general view, social interests, to the narrowest, most basic concepts and laboratory science. This last, of course, is a subject of social interest without having that interest as its subject. The series cannot really be said to go from less basic to more basic, or from more complex to simpler. The reason is that each position is concerned with working out a different set of problems. They are all interested in *psychoanalytic ideas* from some angle, but the subject matter of their inquiries is different. For each area of inquiry there is a different set of basic assumptions for its working conceptions and methods. They are equally complex in their efforts to solve the problems they have set out to study, and in their relation to their version of psychoanalytic theory.

A similar point may be made with respect to different theoretical approaches to, or so-called schools of, "psychoanalysis." That is, those who practice according to any one approach observe in a way somewhat different from that employed by those following another approach. Some aspects of the clinical material that are regarded as valuable from one point of view are considered indifferent according to a different model. Any thorough and systematic attempt to compare two approaches in order to clarify the ways they overlap and are different would be difficult, although there have been some interesting efforts to do so (see Bernardi, 1989). However, we don't have a systematic canon of psychoanalytic theory because it is difficult to

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give an adequately detailed account of any one version of psychoanalysis. This makes it difficult to describe the relationships of different clinical conceptualizations to one another, and to the different points of view (and methods) described in Shevrin's paper.

Any version of psychoanalytic theory requires a kind of translation of its concepts if it is to be compared with any other version or utilized in a nonclinical setting. Since the reverse is also true, the transfer of information from one area to another alters to some extent both the concepts and their application. This consideration leads to a more general point relevant to theory formation: the interrelation among domains of thought and their rendition in different media and in action.

One important point is that all of the perspectives summarized by Shevrin represent the interests of people currently interested in psychoanalysis. To characterize the series narrowly is to take a position both on the relationship among these different kinds of interest and on their relative value.

To Professor di Sapienza, who represents the view from outside anything called psychoanalysis, "psychoanalysis" looks like any other enterprise in which "rational" people are trying to accomplish something while disagreeing on the facts, what to accomplish, and how to accomplish it. The discussions of the other participants, according to di Sapienza, provide the evidence for this rational (but unscientific) conclusion. Di Sapienza's rational argument is itself an example of the kind of reasoning and relationship to observation that this point of view ascribes to its narrow perspective on "psychoanalysis."

However, from the "outside," it is possible to say much the same thing about "science." In fact it *has* been said, and cogently argued. One view argues that the technological success of science has been mistaken as proof of its underlying and untested metaphysical assumptions—i.e., articles of faith. Only the narrowest definition of science as a series of decisive experimental proofs can overlook the degree to which other factors play a role in the development of theories and their acceptance by their adherents.

One of the implications of Shevrin's presentation is that, whatever we may think a scientific theory *should* be, "science" involves a process of expanding knowledge and understanding in which discovery and belief are rarely based on decisive experiments that prove

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theories. Science involves an evolution of knowledge and ideas, with various steps from unacknowledged assumptions through systematic testing of specific limited processes by experiment of whatever kind.

Knowledge, understanding, and the philosophy of science are continuously changing, often revealing the inadequacy of prior proofs and dogmatically asserted conclusions. In these developments, some of the vagaries of mental life, as described by analysts, play an important part. However, this merely means that the process is probably an endless human enterprise, at least but not only a "rational enterprise." Aside from the fact that it is difficult to know what Professor di Sapienza means by "psychoanalysis," the mere fact of mutual disagreements and criticism does not rule out the possibility that it, or *some of its aspects*, may be or become science, after all. The various psychoanalytic points of view related to hermeneutic, dialogic, and textual analysis orientations are not considered directly in this paper, although their contributions to a clearer account of the clinical situation are relevant to some of the discussion.

Where the status of psychoanalysis as science is concerned, the definition of science often becomes a search for the right analogy for psychoanalysis. This search for an analogy has a certain value, since it leads to more detailed description of the ways knowledge is established, and how it can be tested. Acknowledged sciences vary considerably with respect to these questions, as do various aspects of any science at any given time. At any particular time, in any science, there are many conjectures that organize knowledge but for which no strict proof can be found. Proofs such as those correctly specified in Shevrin's paper are perhaps an end stage on a long road of hypothesis refinement and testing.

The theory of evolution by natural selection provides the best scientific analogue for psychoanalysis. It was used by Freud as a model for psychoanalysis, and it is similar to psychoanalysis with respect to the roles played by observation, history, and reconstruction in establishing knowledge in the two fields. In both, the recognition of the interplay of adaptation, chance, and history is a central organizing conception. Equally interesting are the vicissitudes of the social acceptance of Darwin's theory as "science" and the long road to its recent complex experimental demonstration. Darwin is said to have doubted that direct observation of natural selection would be

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possible. However, more than a hundred years of work with reconstructive methods, piecing together the results of careful observation of many different kinds, coupled with a variety of conceptions at different levels of complexity, has apparently been verified recently by prospective observation for twenty years of evolution of finches' beaks and other traits under changing environmental conditions (Grand and Grant, 1989; Weiner, 1994). More recently, experimental observations demonstrated the operation of natural selection in fish and bacteria (see Weiner, 1995).

This does not mean that the acknowledged limitations, and the many possibilities for error in the reconstruction of the past can be dismissed. It means that it is nonetheless useful to work with unprovable assumptions and hypotheses until it is possible to demonstrate the operation of complex processes in prospective studies. The research cited above employed methods that could not have been imagined when the first reconstructions of evolution were presented and hotly contested. At the same time, we are reminded that this is possible because of the use of methods of reconstruction based on the amassing of information based on various kinds of observations and organized according to *meaningful* schemata.

The parallel to the problem of psychoanalysis lies in the need for observing unfolding events in relation to issues of classification with many variables. The activities of observation involve simultaneous observation of phenomena whose interrelation cannot be examined until each phenomenon has been observed according to the methods appropriate to it. The end points of these observations can be combined, correlated, and integrated to provide a system based on these observations.

A specific hypothesis with a specific proof is the end point of a series of contributory investigations. These investigations themselves depend on methods which may have no relation to the methods employed in the other investigations. This may be thought of as a kind of parallel processing in the development of knowledge.

Shevrin's work and other research described in his paper are, like the observations of psychoanalysts conducting analysis, contributions to the parallel processing. While I regard this as a worthwhile undertaking, many respected colleagues do not share this interest or find it useful in their work. However, I believe this is what commonly takes place *as one step* in the development of any science in

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which a chain of inferences draws on many sources of information that are presumed to be relevant to the issue in question. Shevrin's own research is an example in which the idea that semiotics has relevance to the difference between conscious and unconscious thinking leads to experiments using different kinds of categories derived from a different area of knowledge. In his discussion of the relationship between the concept of the unconscious as an empirical generalization and then as a fundamental assumption of theory, he points to the idea that "science" refers not to a thing but to processes (see also Hull, 1988; Waelder, 1970).

If Professor di Sapienza presents one extreme from outside analysis, at the other extreme, from way inside the "psychoanalytic situation," nothing but that situation is regarded as *really* psychoanalysis. For Dr. Case, there is no other way to study psychoanalysis because its essence resides in particular kinds of "inner experiences" within a particular setting. Within the clinical setting, some phenomena can be observed repeatedly in ways not possible in other situations. Varying the parameters of the setting alters the ways in which the mental processes of the analysand can be *recognized* by both analyst and analysand. Other methods of studying the mind may come to conclusions similar to those reached via the psychoanalytic method, but this is not required and, I suspect, may not even necessarily be of interest.

Dr. Case's position, while in one sense claiming a great deal for clinical psychoanalysis, in fact understates the real value of the psychoanalytic situation as a source of information that can both draw on and enrich areas of knowledge outside of clinical psychoanalysis. Analysts necessarily use ideas drawn from many sources, wittingly or unwittingly, in their daily work. While it may be true that there are phenomena that are unique to the analytic situation, it ought to be possible to describe such phenomena with increasing precision. If the uniqueness of the setting is required to produce the phenomenon, we should not exclude the possibility that it can be rationally constructed from observations of component features obtained by other methods.

Dr. Sample accepts the idea that the psychoanalytic situation is what needs to be studied, because it is "psychoanalysis." He believes, however, that the psychoanalytic situation cannot be studied using

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public data and does not permit hypothesis testing that meets the requirements of "eliminative inductionism." Instead, he thinks concepts, exemplified by Luborsky's Core Conflictual Relationship Theme (an idea derived from the concept of transference), can be studied in other psychotherapy settings. In this way, some essential features of the psychoanalytic concept can be studied more rigorously. Everything depends on whether what is being studied is transference as the clinician understands it, or at least an important aspect of it. If not, what is being measured and how it is related to the analytic situation remains unclear. From Case's point of view, this is not psychoanalysis but an interest related to it.

The analyst-at-work takes for granted the flexibility and ambiguity of concepts like transference because the analyzing analyst is sorting out observations with his/her concepts in the background. The multilayering of clinical concepts becomes a problem when reflection on the concepts intrudes and definitions are attempted. At this point, the orientations of different points of view about clinical formulation diverge because they wish to limit the concept for purposes of differentiation from other phenomena. Proponents of different points of view want to emphasize different aspects of a concept like transference. The use of a clinical concept for research purposes faces the problem of an uncertainty principle of observation and definition: the phenomenon is best observed where it can be least defined in a precise operational way, while each effort to define the concept precisely so it may be tested in some other situation departs even further from the original concept. The same process accounts for the different ways concepts are used by the various psychoanalytic "schools." However, this is the way concepts and their applications evolve.

Of course, any fractionation of the complex situation of analysis for purposes of study creates a new area of interest, with its own goals and methods (see Kaplan, 1988). These new areas must then be reconstructed for a new view of the clinical situation. The points of view outlined in Shevrin's paper are such areas of interest. Although presented as competing, they are really supplementary with respect to a general or comprehensive theory of psychoanalysis.

Like Dr. Sample, Dr. Link believes that the *working ideas* of the psychoanalytic practitioner can be studied by research exploring

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their concepts. His approach is to prove underlying assumptions of psychoanalytic concepts. He believes that the properties of unconscious mental activities can be studied and the different characteristics of unconscious and conscious mental processes described. Using methods of subliminal stimulus presentation and neurophysiological measurements, Shevrin has demonstrated differences between conscious and unconscious perception, and differences between responses to conflict-related and conflict-unrelated words. At present, he has begun to demonstrate differences between conscious and unconscious categorization. Accordingly, he has begun to show the fine structure of the basis for the clinician's judgments from the combined viewpoints of semiotics, neuropsychology, and neurophysiology.

Shevrin addresses some fundamental processes underlying the concepts taken for granted by the practicing psychoanalyst in the consulting room. Since the practicing analyst uses fundamental presuppositions every day and does not investigate them, interdisciplinary proofs of basic processes are far from his immediate concerns. However, in my view, such questions as how unconscious processes can be recognized in clinical material by the way categories are organized are relevant to daily work, even if their systematic demonstration is not. Just how relevant Shevrin's observations can be for the practicing clinician remains to be explored. It is not difficult to show that analysts use assumptions about such matters all the time. When relevant aspects of the working analyst's concepts can be studied experimentally, the clinical analyst has a new basis on which to reexamine his assumptions. The repeated reexamination of some aspects of the analyst's own assumptions and thought processes is an essential part of any psychoanalysis.

Combined with other psychoanalytic interests, the arguments sketched in Shevrin's paper indicate that different kinds of psychoanalytic ideas taken together constitute a heterogeneous area of interest and knowledge. The problem for people interested in psychoanalysis *as a field of knowledge* is to understand how such different ways of using psychoanalytic ideas, findings, and assumptions can be usefully integrated, or at least, rendered mutually enriching. The discussion in the paper is to some extent about the ways any point of view looks from the other points of view.

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The problems involved are formidable and can hardly even be articulated adequately in a brief discussion. The reason for this is that none of the interests outlined in Shevrin's paper, or in this commentary, is homogeneous. The experimental studies mentioned can be called "scientific," but they all involve methods, concepts, and interests that are not in themselves psychoanalytic, and that are not necessarily uncontroversial. On the other hand, the psychoanalytic situation is not homogeneous in its activities, some of which are not exclusively psychoanalytic. In both cases, it is a question of whether we are speaking of methods or rationales for activities, or of ways of inferring and drawing conclusions. The meanings and methods of verification and proof remain difficult problems in each area.

Finally, Shevrin's paper subtly brings out still another issue. The various points of view in dialogue demonstrate the problem of communication among people who share an interest in psychoanalysis as a system of related ideas but do not share the same observational contexts. This is a problem even when there is more of an overlap, and even if the overlap is that they all do analysis in addition to their particular investigations. In fact, this problem of psychoanalytic communication is one way of looking at the problem of comparing different clinical approaches to analysis. Much of what is said about the existence or nonexistence of common ground is questionable because people do not generally approach the problem as one requiring sufficient familiarity to do translations from one system to the other. Shevrin has presented cogent arguments for integration and greater attention to the problems of communication of ideas in our field.

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A proper response to this clear, fair, forceful presentation of the three positions would require fifteen thousand rather than fifteen hundred words, so I apologize if the requirement of brevity makes my remarks sound dogmatic. The dialogue participants zero in on the big question—whether psychoanalysis is a science—as well as I can imagine anyone doing it. Nevertheless, it is useful to inquire, *Why do we care? We care because science has the best-attested knowledge claims.* Even laymen know that disciplines indisputably scientific (e.g., chemistry, physics, genetics) can lay better deserved claims to genuine, well-credentialed, objective knowledge than can other purported “cognitive” enterprises, such as theology, metaphysics, epistemology, aesthetics, ethics, politics, literary criticism, and journalism. Why do we regard the indisputable science so highly? It is because of the “scientific method” (consisting more of guidelines than strict rules, as philosophers of science now know). Its components include a general attitude of skepticism (“prove it”), the use of reliable observational procedures, and powerful modes of data analysis, especially mathematics and statistics. The third component is sometimes unnecessary (although never inappropriate); the first two are not. These characteristics achieve (ultimately) consensus among all competent persons who inquire. Does psychoanalysis have methods of observation and inference with these properties? The answer is clearly no.

The proof may be seen in the simple social fact that a century after Freud’s interpretation of his dream of Irma’s injection and publication of the Breuer-Freud *Studies on Hysteria* (1893–1895), it is

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possible for fair-minded, rational, but skeptical persons, trained in medicine or psychology, to deny some, much, or almost all of the Freudian corpus of beliefs. The conversation resulting in the rupture of the Fliess-Freud friendship concerned the *subjectivity* of the psychoanalytic method (Meehl, 1983). Further, the method itself is far from standard among presumed experts in it. The only systematic investigation of techniques (whether rules or merely guidelines) employed in the psychoanalytic hour is Glover's classic questionnaire study of British psychoanalysts in 1933 (Glover, 1940). Those analysts were probably more homogeneous, with regard to their training analyses and as a result of their high density of face-to-face discussions, than any group since. Yet one sees, already at that time, wide disagreement about important interview tactics, and subsequent conversations resulting in a supplementary questionnaire still did not lead to high consensus. (Discouragingly, one of the few principles commanding near-perfect assent, to emphasize transference interpretations, is not supported by quantitative research; see Silberschatz et al., 1986.) Since that second generation of analysts were either not analyzed or were analyzed by those of Freud's original group who themselves had not had analysis with Freud or with anyone else, it would be surprising if the results had been otherwise.

While we have no contemporary data comparable to the Glover study, anyone even slightly acquainted with psychoanalysis, whether conducted by physicians or by psychologists, would confidently predict that the diversity today would be far greater than in 1933. The notion that there exists a "standard (classical) analytic technique of investigation" is plainly false. I can personally attest, as can anybody who has had couch time with more than one analyst, to striking differences. The application of the "method" has not resulted in convergence, either in the sense of convincing outsiders or of preventing multiple splits within the broadly defined psychoanalytic group. A philosopher or historian of science, however sympathetic to analysis, would be forced to say that psychoanalysis at this time shows the major symptoms of a Lakatosian degenerating program (Barron et al., 1992; Eagle, 1984; Edelson, 1988; Lakatos, 1978; Meehl, 1993). Consider a few names that come readily to mind of analyzed therapists who deviated—on the basis of their clinical experience—in varying degrees, from making revisions in theory or technique to totally ceasing psychoanalytic practice: Franz Alexander,

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Aaron T. Beck, Kenneth Mark Colby, Albert Ellis, Sandor Ferenczi, Daniel X. Friedman, Erich Fromm, Karen Horney, Melanie Klein, Heinz Kohut, Sandor Rado, Otto Rank, Wilhelm Reich, Roy Schafer, Melitta Schmideberg, Harry Stack Sullivan, Thomas Szasz, D.W. Winnicott, a fraction of those one could find by a literature search, not to mention the many who deviate silently. What scientific status would we accord physics, genetics, microbiology, or classical psychometrics if a comparable list of "big names," educated and experienced in their respective traditions, came to fundamental disagreement about the photoelectric effect, the structure of DNA, the germ theory of disease, or the high factor loadings of subtests vocabulary and block design on the *g* (general intelligence) factor? While Lakatos admits it is sometimes rational to stick to a degenerating program, what is forbidden is to falsify the track record, to deny the social fact of degeneration. But this is what Dr. Case, unwittingly, urges us to do.

The main reason for this unsatisfactory epistemological state is a fundamental error in Dr. Case's approach. Dr. Case believes that the informal, nonquantitative case study method suffices to produce a science. It certainly does not. We should honestly face the fact that the "case study method" and "clinical experience" are simply honorific labels for what in the field of comparative psychology has long been denigrated as "the anecdotal method." *'Clinical experience' is a phrase applied to the discredited anecdotal method when it is practiced by persons with Ph.D.s or M.D.s.* Historians of medicine tell us that prior to around 1880 or 1890 almost everything that physicians did was either irrelevant or counterproductive. Of the three standard treatment procedures—bleeding, purging, and blistering—the latter was perhaps merely irksome to the patient, whereas bleeding and purging were actively harmful. George Washington was probably killed by his physician, Dr. Benjamin Rush, the founder of American psychiatry and one of the signers of the Declaration of Independence. There are people now in their fifties who are totally blind due to the epidemic of an infantile disease new in the 1940s, retrolental fibroplasia, caused by excessive oxygen tension in the treatment of premature infants. Both those who thought it was caused by not enough oxygen and those who thought it was caused by too much invoked their clinical experience; the argument was settled by combining *statistical study* of premature babies delivered in homes by

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midwives, who could not use the oxygen intervention, and *experimental study* producing identical pathology in the eyeballs of kittens.

Military surgeons disagreed about the efficacy of wound debridement for four generations of clinical experience, a controversy finally settled by outcome statistics and experimental research on animals (Wangensteen and Wangenstein, 1978). The anecdotal method was inadequate to ascertain the relation between an objectively describable surgical procedure and the patients' survival or death a few hours or days later. How then can we argue that a variable, vague, personalized interview strategy (e.g., "interpret from the surface down") can be reliably correlated with alteration several *months* later (e.g., "decreased superego harshness")? While physicians may appropriately object to the slowness of federal bureaucracies (e.g., FDA, NIH, CDC), I am not aware of any practitioners or researchers who think there should be *no* requirements of statistical evidence on the admissibility of a new drug or treatment. While I myself have gained more conviction from my experiences on the couch and behind it than from reading a mostly irrelevant research literature, I realize this is a William James "over-belief." One need not apologize for having over-beliefs, since we cannot get along in daily life or even in science without them; but one should be crystal clear that they are over-beliefs rather than scientifically defensible positions.

Do we dismiss "ordinary, nonquantified" reports of clinical experience as valueless? Of course not. They can be a source of conjectured generalizations and (sometimes) refutations of them. They are usually indispensable for developing skills in applying confirmed conjectures. This practitioner shaping is not peculiar to psychoanalysis or to the healing arts generally, inasmuch as *using* theoretical (verbal) knowledge always requires perceptual and instrumental skills of noticing, classifying, distinguishing, and acting that are psychological functions discrete from formal conceptual knowledge. For example, I have known experimental psychologists who "knew" operant behavior theory, but who—not having been trained by a Skinnerian mentor—could not effectively shape a white rat's behavior into a simple response chain. There is nothing mysterious about this—it is merely the ordinary language philosopher's difference between "knowing that" and "knowing how." But what ordinary, informal clinical experience cannot do is to strongly corroborate complex

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generalizations. The psychological reasons for this inability have long been known (see, e.g., Francis Bacon's idols). They are inherent in the fallible operations of the human intellect, and the notion that acquiring a Ph.D. or M.D. (or having a training analysis!) eliminates them is a narcissistic illusion. Scientific research, from the study of the astronomer's "personal equation" to contemporary research on judgment and problem solving, suffices to deflate such a grandiose idea (see Arkes and Hammond, 1986; Dawes, 1988, 1994; Faust, 1984; Meehl, 1992, 1993; Nisbett and Ross, 1980; Plous, 1993).

"But surely we can learn about facts and their relations without conducting controlled experiments or computing statistics! We know that thunder occurs after lightning, that a wineglass shatters if dropped on a tile floor, that if you regularly say cruel things to people they will dislike you." Yes, of course. But humankind has also "learned" a large number of erroneous relations, about black cats and witches and petroleum dowzers. We label these superstitions—the ones *we* disbelieve in. The clear message of history is that the anecdotal method delivers both wheat and chaff, but it does not enable us to tell which is which. For a list of properties of relations that render this crucial discernment difficult, see Meehl (1983, pp. 18–21). For a long list of error sources (shared by clinicians *and* scientists), see Meehl (1992, pp. 353–354).

As to ignoring Dr. Link's research, there is a simple logician's answer to that dismissive proposal: the Total Evidence Rule. This does not depend on one's philosophy of science or one's ideology; it is learned in a freshman logic course. You cannot by fiat exclude evidence that has a logical bearing upon a conjecture any more than you can make irrelevant evidence germane by fiat. Thus, I would hold that research showing that non-Freudian therapists' ratings on patients' preferences among the major defense mechanisms predict the patients' similarities on a variety of descriptive trait characteristics far better than other theoretical concepts do (Meehl, 1960) tends to support the reality of the defense mechanisms and their importance in mental functioning. On the other side, studies in child development showing no relationship between toilet training and adult anal character traits cannot be dismissed as irrelevant; they speak against Freud's (1908) article on anal eroticism (Fisher and Greenberg, 1977).

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My conclusion is that Dr. Sample has the best of the argument, but Dr. Link cannot be dismissed. Dr. Case is simply mistaken about the history of human knowledge. Given the beautiful formulation of the problem by the three discussants, I find Professor di Sapienza's "resolution" disappointing. Her choice of analogies is unfortunate. (Was it suggested by Freud's famous remark about the three impossible occupations?) As for governing, since I hold with the Swedish statesman Oxenstierna that we little know how badly we are governed, and I believe with Machiavelli that man is ruled by force and fraud, analogizing psychoanalysis to governing does not help me one bit. The analogy to education is simply incorrect as a matter of fact. Beginning shortly after World War I and extending to the present time, educational psychologists have published thousands of experimental and statistical studies of educational method. It is true that some of these are trivial, and there are always some studies that are not well done, but there is a solid body of knowledge about the educational process that has come from those investigations. Any competent psychologist knows that massed practice is inferior to distributed, that almost all learning curves are decelerated, that there are interaction effects between modes of instruction and the intelligence and other characteristics of the learners, that active recitation is superior to passive reception of material and the superiority is greater for relatively meaningless rote material, that class size influences achievement but the graph is markedly decelerated, and the like (see Gage, 1978; Hedges, 1987). Fred Keller's Personalized System of Instruction, based on Skinner's operant behavior theory, has consistently outperformed the traditional lecture as an instructional form. That the psychoanalytic process is one aimed at something practical, valued by two cooperating participants, does not provide a good reason for abandoning the rules of evidence as we find them in the scientific laboratory, courts of law, or the rational conduct of our ordinary affairs.

Nor will di Sapienza's seductive covering phrase, "rational human enterprise," do the trick. One must distinguish between *intending* to do something rational (i.e., using abstract categories, terms of art, formal definitions, purported relations, empirical findings) and *actually proceeding* in a manner that is effectively rational (i.e., conduces to the epistemic aim of truth). Kraemer and Sprenger, the

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scholarly authors of *Malleus Maleficarum* (1487), believed themselves to be pursuing a rational enterprise in detailing symptoms that diagnose witchcraft. Despite their scholarly efforts, we know today there are no persons who have made a solemn pact with Satan and thereby gained preternatural powers. If asked to support their theoretical system and the technical procedures warranted by it, Kraemer and Sprenger would doubtless have invoked the medieval equivalent of "clinical experience."

The yield of such studies as Shevrin's and those of Luborsky (1988) and Mahl (1987) may as yet be rather small for the ingenuity and assiduousness displayed, but it is the best we have and the best place to look. Had I space, I would try to answer Adolf Grünbaum's critique (1993) of that view, held by me and by others. His core objection, the epistemological difficulty of inferring a causal influence from the existence of a theme (assuming the latter can be statistically demonstrated), is the biggest single methodological problem that we face. If that problem cannot be solved, we will have another century in which psychoanalysis can be accepted or rejected, mostly as a matter of personal taste. Should that happen, I predict it will be slowly but surely abandoned, both as a mode of helping and as a theory of the mind.¹

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¹This commentary reads more "antipsychoanalytic" than I personally think or feel (cf. Meehl, 1983). I came to psychology by reading Karl Menninger's *The Human Mind*; I found my personal analysis therapeutic, intellectually fascinating, and professionally invaluable; and for forty years I earned part of my living practicing psychoanalytic therapy. I believe I was helpful to many patients. But given limitations of space, I am forced to focus on the core error of Dr. Case, who in his naive trust of the unquantified case method lulls us into a false security in the face of grave problems and powerful criticisms. Psychoanalysis originated with the idea that warding off painful truths rather than confronting them squarely is bad for us. The psychoanalytic movement, like the individual neurotic, is sure to suffer from the return of the repressed. Recent attacks (e.g., Esterson, 1993; Grünbaum, 1993; Kerr, 1993; Macmillan, 1991) cannot be dismissed by saying they mostly revive the kinds of objections made in the 1920s, because *those old criticisms have never adequately been answered*. Dr. Case is like a physician who assures a patient with malignant melanoma that he is well, and Professor di Sapienza applies a poultice. It just won't do.

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Psychoanalysts have paid relatively little attention to the recent flurry of interest in psychiatric diagnostic categories. The reason for this is easy to understand. Unlike descriptive psychiatrists, psychoanalysts are interested in the meaning of the individual patient's experience, how it stems from his earlier life and will go on to shape his future, and how it is reflected in, and can be modified by, the analytic process, rather than the description and labeling of its surface manifestations.

When psychoanalysts shift their attention from their patients, where they are expert, to the world of scholarly dialogue, where they are amateurs, they often shift strategies as well. Now they are intensely interested in categories—for example, whether or not psychoanalysis is a science. This question may interest some philosophers of science, at least those who are concerned with demarcating the boundaries of their discipline rather than exploring fascinating phenomena wherever their method may prove fruitful. However, I don't understand why psychoanalysts should care, and I suspect that, other than for reasons of pride, most of them don't. How to learn more about people, about mental life, about the analytic process, about the efficacy of psychoanalysis as a therapy—which

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methods might lead to new knowledge, which are useless, which may be premature but deserving of future reappraisal—*these* are interesting questions. How to catalog these enterprises and whether they fall within, astride, or outside the boundaries of science is a question best left for future historians of science. The American Association for the Advancement of Science accepts our dues, and there doesn't seem to be any other arena in which the answer really makes any difference.

However, this is the question that Howard Shevrin, in the role of Professor di Sapienza, uses to frame the dialogue. He (she) points out that "to some this is an old chestnut, to others it is a boring question, and to still others it matters not at all." However, neither the professor nor any of her guests seems to belong to the last group (which includes most other psychoanalysts), so this fundamental issue is never addressed. Had I been invited, I would have suggested that it would be more fun to hear Dr. Case describe a patient, Dr. Sample review his survey data, or Dr. Link discuss his experimental strategies. Professor di Sapienza, if she desired, might tape the proceedings and play them for her undergraduate seminar, a far more appropriate group to address the fundamental question of whether their enterprises should be labeled as science.

It seems that I would have been outvoted, however, and we would have spent two August afternoons in Amagansett engrossed in the unlikely activity of discussing psychoanalysis and science. Drs. Case, Sample, and Link all make cogent points, and don't actually seem to be disagreeing with each other to any great extent. However, they are solely in need of a philosopher to help them unearth their fundamental points of agreement. The imprecision of their language confuses them. For example, they all talk about psychoanalysis, but the word has a different meaning for each. For Dr. Case, psychoanalysis is the clinical method and process of the practicing analyst, from which some theoretical generalizations have been abstracted. For Drs. Sample and Link, it is a method of treatment which, in Dr. Link's words, "applies a body of knowledge and theory . . . to a given patient's emotional disorder." For Dr. Sample, however, its body of knowledge is mentalist psychology, while for Dr. Link it embraces the science of brains and synapses as well. Their strategies follow from these different definitions and therefore are not really in conflict. Philosophers may debate which are scientific strategies and

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which are not; for most of us the question is which are interesting, generative, and worth our time and effort. Professor di Sapienza herself is concerned with a different question, one of power, but she seems to have forgotten Bacon's dictum that knowledge is power when she argues that in her view research is irrelevant. How does she think the powerful tool of psychoanalysis was first developed?

Shevrin himself is each of these four protagonists, but most of all Dr. Link, who in fact presents Shevrin's research. He invites us to decide whether bridge building is premature by having us join with him in undertaking the task, and offers as exemplars two bridges, one from psychoanalysis to neurophysiology and the other to cognitive science. He promises that his studies will take into account unconscious processes and transformed contents—which he considers “essential elements of the psychoanalytic model.” (Dr. Case, and I, would agree—just as we would agree that the alphabet is essential to Shakespeare, though we might balk if asked to conclude that alphabetology is the essential basic science of English literature.)

Shevrin's first study is said to draw on “the essentials of the psychoanalytic clinical method.” (He means an unstructured interview and psychodynamic formulation—perhaps “one of the essentials” would be more accurate.) The subject is exposed to emotionally charged and neutral control words. Shevrin demonstrates that the pattern of EEG responses to subliminal and supraliminal exposure varies with the emotional valence of the stimulus and suggests that this tests the “presupposition of a causative psychological unconscious” and supports its validity. It would be more cautious to say the experiment fails to disprove the presupposition, but then it isn't clear that any experimental results would lead one to question the notion of a dynamic unconscious. However, the real question is, Do such an experiment and such results constitute a bridge, or do they suggest that the effort is still premature, that we would do better to map the neurophysiology of simpler and more superficial mental phenomena (an area where great progress is being made) before trying to build a bridge over a river that is so wide?

Shevrin's second study is a purely psychological one. It demonstrates that subliminal exposure to a priming word facilitates superficial, but not more complex, categorization. (For example, *Freud* would speed up linking *analysis* and *therapy* but not *analysis* and *natural science*.) He concludes that this provides “independent support

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for the . . . presupposition of the psychoanalytic method, that unconscious causes are inferred from so-called displaced and substitute formations." Once again, although it certainly doesn't disprove the presupposition, it doesn't really provide much in the way of support. This is not a bridge that can bear much weight.

Shevrin points to Reiser's brilliant monograph (1984) as a foundation for his position. In a 1987 review of that book, I commented:

At present, our very best neurobiology has relatively little to say about how we carry out the simplest of mental activities—recognizing a figure, identifying a word, connecting the two, or remembering the connection. We have only the vaguest of notions about the difference between sleep and wakefulness, and less about the varieties of arousal—anger, anxiety, attention, desire, etc. In short, we do not yet have much convergence between neurobiology and the most elementary of cognitive psychologies. It would seem reasonable to wait for that to develop before using neurobiology to tackle the much more complex, and less easily formulated, concepts of psychoanalysis [Michels, 1987, p. 535].

The question, then, isn't really whether psychoanalysis is a science, or even whether the methods of these experiments are valid, but which psychoanalytic inquiries are interesting and to whom. Practicing psychoanalysts are interested in clinically relevant studies—Dr. Case's histories and, to a limited extent, Dr. Sample's data. Those who train therapists, triage patients, develop programs, and allocate resources are interested in Dr. Sample's findings, but are often disappointed by them and find personal clinical wisdom to be a better guide. Scientists who once wanted to be psychoanalysts and psychoanalysts who once wanted to be scientists are fascinated by Dr. Link's approach, and look forward to the day when its promise can be realized.

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Psychoanalysis as Science

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How does one write a commentary on a tour de force? Howard Shevrin is a gifted psychoanalytic clinician, educator, and researcher who is also a novelist and a poet. His impressive literary talents make this presentation truly unique among all of the plenary addresses ever published in this journal. Readers who are clinicians (I count myself among them) will immediately identify with Dr. Case and his passionately held belief that psychoanalysis is a science *sui generis*. But I would hope that most clinicians will remember that we are celebrating this year the hundredth anniversary of the birth of psychoanalysis—the publication of *Studies on Hysteria* in 1895. Over the course of this century, I would also hope, generations of clinicians will have accumulated enough wisdom to appreciate the plea of Dr. Sample for more sophisticated outcome and process research devoted to the psychoanalytic situation, and enough humility to respect the position of Professor di Sapienza that psychoanalysis is not a science at all, but rather one of the great humanistic enterprises (like teaching and governing) whose goal is to bring about significant rational changes both in individuals and in society. What is stunning about his presentation is that Shevrin, through his persona, Dr. Link, then asks us to stretch our professional identities still further to consider the possibility that psychoanalysis is actually two separate but related sciences, an applied clinical science and a basic science of the mind. Are we really ready for this challenge after only one century? Shevrin would answer that even though the unconscious may be timeless, life—and the scientific necessity of forming bridges between psychoanalysis and other basic sciences of the mind and brain—is not.

Shevrin proceeds to summarize his thirty-five years of elegant research, in which he and his colleagues provide experimental, laboratory evidence for two fundamental presuppositions of psychoanalysis. The first presupposition is the existence of a causative (dynamic) unconscious, demonstrated through data derived from the basic science of neurophysiology. The second presupposition is the existence of various substitute transformations in consciousness (displacements, reversals, symbols, and superficial associations) that are brought about by this causative (dynamic) unconscious, demonstrated through data derived from the basic science of cognitive psychology. Shevrin is well aware that these are only beginning “links,”

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“piers on either side of the great divide” separating the mind of psychoanalysis from the brain of the neurosciences. But still, what a beginning!

There is a third fundamental presupposition of psychoanalysis, explicitly stated and embraced by Dr. Case, and presumably supported by Dr. Sample and Professor di Sapienza as well, namely that the elicitation, elucidation, and interpretation of conflicts present in the causative (dynamic) unconscious (manifested by their derivative transformations in consciousness) can bring about intrapsychic and interpersonal change. Where does Dr. Link stand on that issue? Is it conceivable that Shevrin and his colleagues will someday be able to demonstrate such changes in their laboratory? If so, which of the neurosciences will provide the data? Will it be neurochemistry, through changes measured in brain neurotransmitters? Will it be neuroendocrinology, through changes measured in the hypothalamic-pituitary-adrenal axis? Will it be neuroanatomy, through changes measured by various brain-imaging techniques? I hope that “time’s wingéd chariot” brings Shevrin back to us in another decade or so with the beginnings of his bridges for this third, and ultimately crucial, presupposition, crucial at least for psychoanalysis as an applied clinical science. And by then, what further challenges will he be presenting to us? Though we cannot (and must not) make the sun of psychoanalysis stand still, we must yet be thankful to Howard Shevrin for inspiring us to make it run.

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Among the four viewpoints Howard Shevrin presents in order to dramatize contemporary debates on the scientific standing of psychoanalysis, he situates his own research within the tradition championed by the fictional Dr. Link. I shall therefore focus my commentary on this aspect of his wide-ranging presentation.

Shevrin’s conception of psychoanalysis rests on a basic postulate regarding its scientific foundations, namely, that “psychoanalysts

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practice a method based on certain presuppositions for which the . . . method itself cannot provide proof." The presuppositions he refers to are (1) that *conscious phenomena have unconscious psychological causes*, and (2) that *unconscious causes are represented indirectly in consciousness, due to the effects of various transformations*. Shevrin points out that "these presuppositions may turn out to be true or false, with fateful consequences for the science." This conception of the scientific foundations of psychoanalysis is attributed to both Dr. Sample and Dr. Link, but only the latter bases his entire argument on it. Whereas Dr. Sample calls for a "more rigorous" clinical research methodology, Dr. Link asserts that only "a science basic to psychoanalysis" can provide independent evidence for its presuppositions, for "if the research intends to address these two basic presuppositions, it must demonstrate how it can do so independently of the clinical method based on those presuppositions." He concludes that "research of this nature, to succeed, must either discover *new consequences* of these presuppositions independently arrived at, or provide evidence of what *underlying factors or preconditions* cause them" (emphasis added). He then summarizes two examples of Shevrin's research, to demonstrate how these criteria can be met.

In order to evaluate these arguments we need to determine, first, whether the basic postulate on which they are based is valid and, second, whether Shevrin's research satisfies his own criteria for "a science basic to psychoanalysis." I shall address these two questions separately.

Psychoanalysis is indeed based on a fundamental presupposition. Freud stated that presupposition repeatedly throughout his scientific writings, but nowhere did he do so more clearly than in the following passage:

The psycho-analytic assumption of unconscious mental activity appears to us . . . as an extension of the corrections undertaken by Kant of our views of external perception. Just as Kant warned us not to overlook the fact that our [external] perceptions are subjectively conditioned and must not be regarded as identical with what is perceived though unknowable, so psycho-analysis warns us not to equate [internal] perceptions by means of consciousness with the unconscious mental processes which are their object. Like the physical, the psychical is not necessarily in reality what it appears to be [Freud, 1915, p. 171].

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The fundamental presupposition of psychoanalysis is therefore the notion that *conscious perceptions represent an unconscious reality, which can never be known directly*. A subsidiary premise is that *there are two such classes of conscious perception: (a) physical perceptions (of things which exist in the world around us), and (b) psychical perceptions (of things which exist inside of our own selves)*. These two classes of perception are presumed to arise from "two sensory surfaces" of consciousness (Freud, 1900, p. 574), which represent the "two terminal points of our knowledge" of the reality that lies beyond perception (Freud, 1940a, pp. 144, 196). Psychoanalysts study reality from the viewpoint of the *internal* surface of consciousness. Freud's detailed justification for describing this aspect of reality in *psychical* terms (1915, pp. 166–171; 1940b, pp. 282–286) need not be repeated here.¹ The essential point is that a psychical conception of the internal sources of consciousness "enabled psychology to take its place as a natural science like any other" (Freud, 1940a, p. 158), for it inserted our private thoughts and feelings into a natural causal framework. The alternative view, which held that subjective states of consciousness occurred inexplicably, in parallel with certain physical processes, "had the unwelcome result of divorcing psychical processes from the general context of events in the universe and of setting them in complete contrast to all others" (Freud, 1940b, p. 283). Insofar as psychoanalysis aims to provide a psychological account of the field of psychical processes and "to establish the laws which they obey and to follow the mutual relations and interdependences unbroken over long stretches" (Freud, 1940a, p. 159) it is a *basic* science of what Freud described as "psychical reality." In this respect it is analogous to physics in relation to material reality.

It is evident that the fundamental presupposition of psychoanalysis coincides with a limiting parameter of human knowledge. The domain of scientific inquiry lies entirely within the bounds of conscious perception; the "ultimate reality" that lies beyond is unknowable. The presupposition that conscious phenomena have

¹Freud always acknowledged that internal reality can *also* be represented physically, via our *external* sensory organs, for "the mental apparatus . . . is also known to us in the form of an anatomical preparation" (Freud, 1900, p. 536). Thus, the object of study of psychoanalysis can be described either psychically (from the viewpoint of internal perception) or physically (from the viewpoint of external perception). Psychoanalysis describes it psychically; neuroscience describes it physically.

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unconscious psychical causes is therefore no more open to *proof* than is the presupposition that external sensory perceptions are caused by real things existing independently of our knowledge of them. For that reason, as numerous philosophers have pointed out in recent years, all scientific knowledge is ultimately hypothetical, and every observational datum is also a theoretical inference. It is also within this context that we have been able to understand the fact—problematic both in physics and in psychoanalysis—that acts of observation determine the nature of the object perceived. Indeed it could be said that the more “basic” the science, the more this problematic indeterminacy of the perceptual object will be encountered.

Similar considerations apply to what Shevrin describes as the second presupposition of psychoanalysis. The presupposition that the unconscious determinants of psychical events are represented *indirectly* in consciousness, due to the effects of various transformations, is equivalent to the presupposition that the ultimate properties of physical matter are represented indirectly in perception, due to the effects of our external sensory apparatus. This is a fundamental presupposition, but nobody has called for a science basic to physics on those grounds. (I hope it is clear that I am not suggesting that psychoanalysis is a non-scientific enterprise; I am saying only that the presuppositions of psychoanalysis to which Shevrin refers are no different from the presuppositions of every other natural science. It is only when presuppositions of this sort are *questioned* that we engage in philosophical speculation.)

In our science as in the others the problem is the same: behind the attributes (qualities) of the object under examination which are presented directly to our perception, we have to discover something else which is more independent of the particular receptive capacity of our sense organs and which approximates more closely to what may be supposed to be the real state of affairs. We have no hope of being able to reach the latter itself, since it is evident that everything new that we have inferred must nevertheless be translated back into the language of our perceptions, from which it is simply impossible to free ourselves. But herein lies the very nature and limitation of our science. It is as though we were to say in physics: ‘If we could see clearly enough we should find that what appears to be a solid body is made up of particles of such and such shape and size occupying such and such relative positions.’ In the meantime we try to increase the efficiency of our sense organs to the

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furthest possible extent by artificial aids; but it may be expected that all such efforts will fail to affect the ultimate outcome. Reality in itself will always remain 'unknowable' [Freud, 1940a, p. 196].

The clinical method is therefore as incapable as any other scientific method of *proving* the existence of unconscious events, or of *directly* comparing them with their conscious transformations. In psychoanalysis, as in other sciences, we are charged with the task of developing "artificial aids" to probe the unknowable object of our study. Our estimation of the relative value of a psychoanalytic method should therefore be based on the extent to which it reliably translates "unknowable" unconscious processes into the language of our perceptions, so that we can construct a serviceable *model* of the unconscious processes themselves.

It would be unfair to ask whether Shevrin's methods achieve this goal better than the clinical method; clearly they do not. But his methods serve different aims. Shevrin aims to discover *new consequences* of the unconscious, and to reveal the *underlying causes* (or preconditions) of its modes of transformation in consciousness.

Do his methods achieve these goals? I believe that they do not; *stripped of the presuppositions they purport to examine*, Shevrin's methods reveal almost nothing about the unconscious and its modes of transformation. The first study that Dr. Link cites (Shevrin et al., 1992) demonstrates only that some indeterminate features of categories of words, when they are presented to the (external) perceptual apparatus in a particular way, correlate systematically with some gross feature of neural activity when it is measured in a particular way. The observed correlation between categories of words and patterns of cortical activity is a valid empirical finding, but the substantives in this correlation are wide open to *interpretation*. For example, the relationship between the so-called "unconscious conflict words" and the actual unconscious situation (assuming that such things exist) is highly debatable, especially in view of the small amount of clinical evidence on which the hypothesized conflicts are based. The unconscious consequences of the subsequent subliminal presentation of these words is equally debatable. Word-presentations that are exposed to the mind via the external surface of the perceptual apparatus (however briefly) cannot be considered "unconscious" in the

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dynamic sense, unless it can be demonstrated that they have been *repressed*. Shevrin seems to concede this when he states that the subliminal presentations are only *descriptively* unconscious. However, he immediately goes on to claim that the distinctive patterns of cortical activation these words evoke are manifestations of a "causative psychological unconscious." This crucial claim is unsubstantiated. The finding that slow or rapid presentations of words (which therefore attract more or less attentional cathexis) evoke different patterns of cortical activation, and the finding that these patterns covary with the different categories of words, will surprise no one. These findings demonstrate only that psychological content can be processed pre-consciously; but nobody seriously disputes that.² This has nothing to do with the dynamic unconscious. Shevrin *presupposes* that the different patterns of cortical activation he evoked reflect the operation of a "causative psychological unconscious." However, many other interpretations are possible. For example, it could be argued that words which evoke an intense and immediate feeling of unpleasure (i.e., the "conscious symptom words") are correlated with patterns of cortical activation different from those correlated with words less intensely or directly evocative of unpleasure (i.e., the so-called "unconscious conflict words"), and that patterns of cortical activation vary in relation to the duration of the presentation of these words (i.e., in relation to the degree of attention they attract). These interpretations do not require the presupposition of a causative unconscious. The evoked responses therefore cannot be assumed to be "new consequences" of the unconscious.³

²The same facts were demonstrated long ago by Pözl (1917) in his experimental manipulations of the manifest content of dreams. More recently, preconscious mental processing was rediscovered by Pöppel et al. (1973) and Weiskrantz et al. (1974), in relation to the curious phenomenon of "blindsight" (Weiskrantz, 1986). Similar phenomena are described by the general concept of "implicit memory" in contemporary cognitive science (Jacoby, 1984). It is also important to note in this regard that the (descriptive) dimension of consciousness/unconsciousness admits of *degrees*; consciousness is not an all-or-nothing phenomenon.

³Although physiological methods are incapable of *proving the existence* of the unconscious, they do enable us to study the *external perceptual realization* of the unconscious (see footnote 1 above). The value of physical methods should therefore not be underestimated. However, it is questionable whether the evoked-response method is adequate for correlating the two "terminal points of our knowledge"; it unduly constrains and distorts the psychical phenomena and provides only gross and non-specific information about their physical correlates.

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The results of the second study Shevrin cites (Snodgrass et al., 1995) are similarly open to interpretation. It could be argued, for example, that if two word-presentations are categorized on the basis of a brief exposure to the (external) perceptual apparatus (i.e., on the basis of minimal attentional cathexis and therefore minimal perceptual information), they will more readily be categorized according to simple attributes than complex ones. Increased duration of exposure (i.e., more attention and therefore more information redundancy) will alter the rate and complexity of categorization. This is because categorical decisions are made faster, but less securely, on the basis of minimal perceptual information. These interpretations have no bearing on the presupposition of indirect representation. The principle of categorization in question therefore cannot be described as an "underlying cause" of this process.

I trust that these brief remarks suffice to demonstrate that Shevrin's research is no less dependent on the presuppositions of psychoanalysis than is our classical (clinical) methodology, and that his observations are no less open to interpretation than are the free associations of our patients. Our estimation of the value of these methods for psychoanalysis should therefore be judged by the same criteria as the clinical method.

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