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The Current Status of Psychoanalytic Theory

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In recent years, metapsychology has been effectively destroyed by a series of critiques, here summarized. Clinical psychoanalysis, its heart, is a testable scientific theory and need not be trivialized by being reduced to a hermeneutics, but it has been exposed by Grünbaum and Rubinstein as seriously lacking in empirical verification. Its genetic hypotheses are extremely difficult to test; the clinical case study is useful only as a means of generating hypotheses. As Rubinstein has shown, however, the clinical theory can be systematized and stated in probabilistic propositions testable by statistical research. Its fundamental propositions can be tested only by nonpsychoanalytic data, however. Object relations and self psychology have had a large vogue but do not address the fundamental theoretical problems. Those threaten the survival of psychoanalysis, but are being complacently ignored. Some possible solutions are discussed.

In attempting to survey the field of psychoanalytic theory, so that I might report to you on the state of the art, I found three major trends preoccupying most of the recent literature: First, the decline if not the actual death of metapsychology, accompanied by a small rise of interest in the clinical theory. Second, the question of what kind of discipline psychoanalysis should be, scientific or hermeneutic. Third, the rise into increasing prominence of object relations theory and self psychology. I will organize my remarks accordingly, though I see all these trends as interrelated.

Before going further, I should make it clear that, along with most other contemporary students of psychoanalysis, I find very helpful Rapaport's

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(1960) distinction between two rather different theories within it: a clinical theory, made of propositions about people and their problems, incorporating concepts that can be fairly easily defined empirically; and a more abstract, scientifically more ambitious theory called metapsychology, the propositions of which largely restate clinical hypotheses in a more austere and impersonal language.

THE DECLINE AND FALL OF METAPSYCHOLOGY

First, then, metapsychology—an old preoccupation of mine. In a series of papers (Holt, 1962, 1965a, 1965b, 1967a, 1967b, 1967c, 1968, 1972, 1975, 1976a, 1976b, 1978b, 1981, 1982), I have subjected it to a critical though friendly examination. I started in the hope of clarifying and systematizing the theory, along the lines Rapaport (1960) had begun. I am, of course, far from being the only or even the first critic of metapsychology. Kubie (1947) perhaps has the honor of priority; at least, he was the first I know of, and important critical contributions have been made by Applegarth (1971), Basch (1973), Eagle (1984), Galatzer-Levy (1976), Gill (1976), Grossman and Simon (1969), Holzman (1976, 1985), Klein (1976), Leites (1971), Rosenblatt and Thickstun (1977), Rubinstein (1965, 1967), Schafer (1976), Sulloway (1979), Wilson (1973), and Yankelovich and Barrett (1970), among others. I shall summarize the collective critique of metapsychology in a series of separate statements, mostly without attribution.

—*The relationship between metapsychology and the clinical theory has not been clarified.* Thus, the limits of each and the borderline between them are matters of dispute, and there is no consensus on what is the total body of clinical theory and of metapsychology.

—*Concepts are poorly defined.* Existing definitions are so vague, imprecise, and multiple that much of the theory cannot be pinned down enough to test it empirically, and different writers are free to use the same term in quite different ways. Consequently,

—*Concepts overlap one another partly or completely.* More than just the confusion of the neophyte is at issue when such a variety of terms exist, all referring to more or less the same subject matter, as, for example, instinct, libido, drive-derivative, cathexis, excitation, id, sexuality, pleasure principle, and tension-reduction. One result is a needless complication by surplus levels and layers of theory. Another is:

—*Concepts are often reified,* abstractions treated as if they refer to substantial entities. The worst form of the error is *personification* or anthropomorphism, treating concepts like drives or structures as if they were persons or had attributes such as striving and insisting that properly belong only to whole people.

—*Metapsychology contains many self-contradictions*, because Freud never went back over it, pulled it all together, and explicitly renounced ideas that had been superseded. The effort by Rapaport and Gill (1959) to do so is only a beginning, and has the further problem of incorporating two points of view not included by Freud and not universally accepted by other psychoanalysts.

—In developing its propositions, *Freud committed a good many logical errors and fallacies of reasoning*. Further,

—*Freud made extensive use of metaphor and other figures of speech at points of theoretical difficulty*, a practice that tends to conceal or divert attention away from the fact that problems were left unsolved.

—*Much of metapsychology is a translation into other terms of outdated physiology, anatomy, and early evolutionary biology*. Freud's first effort to go beyond clinical theorizing, the Project, was an explicitly mechanistic, biological model of the organism with particular reference to the structure and functioning of the brain. Though in many ways an astonishingly brilliant tour de force containing many anticipations of modern views of the nervous system, it was clearly a failure. Yet its anachronistic and erroneous notions of how the brain is organized and works, and of Lamarckian and Haeckelian evolution were revived (with slight terminological changes) in the metapsychology, by means of a metaphysical transformation.

After suppressing his early love for philosophy, Freud avoided facing philosophical aspects and implications of his theory. Therefore,

—*Metapsychology fails to take clear and consistent stands on basic philosophical issues*, for example, on the mind–body problem, or the problem of freedom and determinism, or the nature of reality. Much of the time,

—*Psychic energies, forces, and structures are assigned a metaphysical status separate from the world of material realities* like measuring instruments. Therefore, propositions involving these central terms cannot be tested, nor can any of the key entities be measured. Hence,

—*Metapsychology is a closed system*, which Rubinstein (1967) has shown can be translated into a purely formal model devoid of any content. But because it does not generate new propositions, as scientifically useful formal systems can, metapsychology has neither explanatory nor heuristic value.

In the course of studying the problems of metapsychology, I became aware of Freud's unique style of thinking and writing (Holt, 1974), and for a while thought that *it* might be the main source of trouble. A number of its features—for example, his tolerance for self-contradiction, his hyperbolic tendency to state generalizations in the most sweeping and universal form, and his fondness for figurative language—did make psychoanalytic theory a good deal messier, more difficult to understand, and harder to defend than it might have been. For a long time, I clung to the hope that if one could simply purge metapsychology of these stylistic defects it would prove to be not only helpful to the clinician but a fertile source of testable propositions for the experimenter and other systematic researchers in personology as well as clinical psychology. But after a couple of decades spent in studying metapsychology and attempting to make it scientifically useful, I reluctantly concluded that the task was impossible. Moreover, metapsychology's defects are intrinsic, not simply products of Freud's personal style of thinking and writing.

At present, I think it is fair to say that metapsychology is virtually dead. Fewer voices defend it explicitly, and more join the chorus of those who find it wanting. To be sure, most practicing analysts have not paid much attention to the demise, never having put much stock in "that abstract stuff" anyway. What they do not realize is that they are far more committed to it than they know. For everyday terms like ego, id, instinct, and psychodynamics are integral parts of metapsychology rather than the clinical theory. They share with more outlandish terms like anticathexis and defusion the fatal property of not referring to anything even indirectly observable and of contributing nothing to discourse beyond the nonrational gratification many of us get from speaking a recognizably psychoanalytic jargon. Eagle (1984) has demonstrated in some detail that those who, like Kohut, claim to work solely within the terms of the clinical theory actually go beyond it into metapsychological territory—as indeed they must, unless they are willing to be as austerely atheoretical as Schafer (1976).

I recognize the harshness of the above indictment, but do not make it lightly or with any pleasure. It is anything but a comforting reflection to realize that most of one's career has been devoted to as worthless a theory as metapsychology proved to be. Nevertheless, Rubinstein (1976a) is quite right, I believe, in stressing the fact that something like metapsychology is necessary to supplement the clinical theory.

A PHILOSOPHICAL CRITIQUE OF THE CLINICAL THEORY

A number of theorists, including my late friend George Klein (1976), reacted to the realization that metapsychology was bankrupt by counseling that we simply discard it and concentrate on systematizing and developing the clinical theory, claiming that it is capable of becoming a self-sufficient discipline on its own. Schafer (1976) meanwhile went a similar route, but produced his alternative, action language, which is not so much a theory in the usual sense as it is a controlled language for discussing clinical phenomena without falling into the fallacies of reification and personification that bedevil Freud's writings. After making a preliminary effort to extract the clinical theory from metapsychology, I withdrew in some discouragement, reporting (Holt, 1975) that the two were much more closely intertwined than Rapaport had suggested, and that there was no simple or obvious way to produce a set of excerpts from Freud's writings that would give the clinical theory definitive exposition.

Fortunately, a leading philosopher of science, Adolf Grünbaum, became interested in the problem of the scientific status of psychoanalysis in 1976 and began studying its literature. In the 7 years since then, he has published a series of papers (Grünbaum, 1976, 1977, 1979, 1980a, 1980b, 1981, 1983a, 1983b, 1983c, 1983d) and a book (Grünbaum, 1984), in which he thoroughly reviews the question of whether psychoanalysis is a science, and how good a one (see also von Eckhardt, 1985). Here at last is a philosopher who has done his homework before criticizing Freud. Happily, Grünbaum has concentrated his efforts on the clinical theory. For in his own words,

> when Freud unswervingly claimed natural science status for his theoretical constructions . . . , he did so first and foremost for his evolving clinical theory of personality and therapy, rather than for the metapsychology. . . . [He claimed] the *scientificity* of his clinical theory *entirely on the strength of a secure and direct epistemic warrant from the observations he made of his patients and of himself.* (Grünbaum, 1984, p. 6)

It happens that one of the great figures in contemporary philosophy, Karl Popper (1963), had attacked psychoanalysis as unfalsifiable, therefore no better than a pseudoscience. Grünbaum (1976, 1977, 1979) has earned the gratitude of all of us by taking Popper down a peg, showing that his arguments not only are based on ignorance of what Freud actually said, but have logical flaws as well. Citing several of Freud's propositions that can clearly be tested empirically and several passages in which Freud explicitly said that a single case running counter to one of his theories (e.g., that of paranoia) would refute it, Grünbaum has unanswerably established the claim of psychoanalysis to a place among the sciences by Popper's own criteria, as well as demonstrating that the serious challenge to the scientific credibility of psychoanalysis comes instead from the so-called eliminative inductivism. That is well known to psychologists as the underlying principles of experimental design and other forms of empirical research (Grünbaum, 1984, pp. 279–280). Plainly, psychoanalysis *is* testable inductively, though its record of genuine inductive validation is discouraging.

If much of the clinical theory is testable, how has it stood up? Are Freud's theories supported by the available evidence? Recall that Freud was benignly indifferent to attempts to test his theories experimentally, stating that they had been confirmed so many times clinically that such laboratory exercises as those of Rosenzweig (1983) were unnecessary. Meeting this claim head on, Grünbaum (1980b, 1983c, 1984) argues that clinical data are so epistemologically contaminated as to be virtually useless for the purpose of testing or providing support for any of the theories. This is an important point, so let us look more closely at the way he reached such a discouraging conclusion.

Wilhelm Fliess seems to have been the first to point out the possibility that Freud's method left him wide open to the charge that when he interpreted the patient's productions, he was reading his *own* thoughts into them instead of discerning the *patient's* unconscious meanings. The very data of the patient's productions are suspect, because in so many subtle ways as well as through the more obvious means of interpretation, the analyst steers and shapes what the patient reports in his/her mislabeled free associations. Add the motivation to please the analyst (the positive transference), and it is easy to understand why Freudian patients produce classical Freudian dreams, while Jungian patients' dreams are full of mandalas, wise old men, and other Jungian symbols.

Freud was aware of such influences under the general name of suggestion, and his main argument against it was that the patient's "conflicts will only be successfully solved and his resistances overcome if the anticipatory ideas he is given tally with what is real in him" (1916-1917, p. 452). And to the extent that psychoanalysis is therapeutically successful, according to this Tally Argument, as Grünbaum has dubbed it, the interpretations *must* be valid. That would be okay, he concedes, if it were in fact the case that neurotic conflicts and symptoms could *not* be overcome in any way other than by giving correct (Freudian) interpretations. Besides, it assumes that genuine cures *are* achieved by psychoanalytic treatment reasonably often, and that spontaneous remissions do not occur. But comparative studies of the therapeutic efficacy of psychoanalysis and other forms of therapy for neurosis unfortunately have not supported Freud's claim. The burden is definitely on psychoanalysts to prove that their treatment is better than any other; the *best* construction one can put on currently available data is that psychoanalysis may be no worse than nonanalytic therapies. All offer slightly better recovery rates than spontaneous remission. But what all psychotherapies have in common may be nothing much more than the familiar placebo effect; hence, there is *no* support for Freud's assumption that neurosis can't be cured except by accurate interpretations based on a true theory—his.

Another possible source of clinical evidence, the introspective testimony of analyzed patients, cannot come to the rescue, for there is no evidence that anyone—even after analysis—has direct introspective access to the kinds of internal processes postulated by Freud or to the causal linkages he asserted.

Thus, clinical data do not have the "probative value that Freud claimed for them," Grünbaum (1984, p. 245) argues. That by no means implies that analysts' interpretations are automatically false, he adds; they may sometimes be true, but there is no reliable way to distinguish the grain from the chaff among them.

But he has gone further, demonstrating that even if clinical data had no such liabilities and could be taken as perfectly trustworthy, they would not validate one of Freud's principal theories—the pathogenicity of repression. It is surely a cornerstone of the clinical theory, Grünbaum (1983b, 1984) says, that repressed material (memories of traumatic experiences, fantasies, and wishes) cause neurotic symptoms, dreams, slips, and the like. At first, it looked as if Breuer and Freud had an excellent method of testing this hypothesis, for it seemed that separate symptoms could be removed, one by one, through the undoing of specific repressions. Unfortunately, as Freud later put it (1925, p. 27), "Even the most brilliant results were liable to be suddenly wiped away if my personal relation with the patient became disturbed." And with this discovery of the transference, Freud lost interest in the attempt to find any other proof for his hypotheses about the effects of repressed memories and wishes, took them for granted as somehow established by his clinical experience, and failed to adduce *any* confirmatory data.

I believe that Grünbaum somewhat overstates his point concerning the contamination of clinical data, which is not an all-or-none affair, and overlooks the possibility that fruitful use may be made of data gathered by therapists who have other theoretical and technical persuasions. With enough recorded treatments from a sufficient variety of analysts of all schools, it should be possible to find out just how far their patients' dreams, fantasies, childhood memories, etc. do systematically differ, and to what extent hypotheses of Freud's clinical theory hold, regardless of the nature of the treatment. If positive findings occur disproportionately more often in classical psychoanalyses, we're in trouble, for that would imply that they are a consequence of suggestion or Rosenthal effects. But until the research is done, *we just don't know.*

Grünbaum, von Eckhardt (1982), and some other critics on whom I am now going to draw, agree that the large part of the clinical theory dealing with *genetic* hypotheses cannot be satisfactorily tested by means of the data available to psychoanalysts for yet another reason. All such retrospective clinical research falls afoul of one basic flaw, the lack of adequate controls to establish casual connections.

Take the simple case of Freud's early theory that hysteria was caused by so-called seduction—better called child abuse. Let's make the obviously implausible assumption that he *was* able to ascertain the true facts about childhood traumas by analyzing adults, and that he uncharacteristically had kept careful statistics about all his patients over a decade; he would have had an inadequately small sample. Very well; assume that he had been able to train a group of equally skillful analysts, scattered about in other major cities to be sure it was not just a Viennese phenomenon, and with a large enough clientele to provide a couple of hundred well-diagnosed cases of hysteria. Let's assume highly positive findings: convincing evidence of child abuse in 85% of the cases. The question next arises, how does that compare to the base rate, which was unknown? Even if it could have been discovered, he still would not have known what he needed to know: what are the chances that if a child is sexually assaulted, she or he will *develop* hysteria? There is no substitute for a prospective design to test such hypotheses, which are readily enough *formed* retrospectively: You have to get two samples of children, about half of whom are victims of abuse and half are in other relevant respects the same but are never assaulted sexually. Then you must follow them to adulthood and have them diagnosed by qualified clinicians who are blind to the childhood history. Incidentally, because such a study has never been done (and it would be extraordinarily expensive and difficult to do it), we still do not know for sure what the answer would be. Some recent research that approximates the above design indicates that child victims do suffer serious mental health problems in adolescence, but not classical hysteria. Even so, that near miss is actually about as good a record of validation as any of Freud's other clinical hypotheses possess.

Psychoanalysts must begin to face the fact that their primary and typical form of research, the uncontrolled clinical case study, is devoid of scientific value *except* as a source of hypotheses. A good deal can be done to test nonetiological hypotheses with the undeniably rich data of psychoanalyses, but only if they are fully recorded, of course with the informed consent of the patients, the data carefully if minimally censored to prevent recognition, and made available to all qualified researchers. Then at last we will have public and replicable data; then the researcher can be someone other than the therapist, and therefore truly disinterested and uncontaminated. Thanks to pioneers like Dahl (1972), Gill (1982; Gill & Hoffman, 1982), Luborsky (1977), and Weiss and Sampson (in press), such work is under way. We know how to reduce, order, and systematize the overwhelming masses of data to make it possible to process them, though it takes lots of willing workers who have both psychoanalytic and research training, to make the necessary judgments in controlled and replicable ways. Work is at last beginning on clarifying rules of inference, the processes of clinical judgment and interpretation, for the first time.

It is good to be able to report this progress. It shows that the situation is not hopeless, but it *is* grave. We have been living in a fool's paradise, believing that our clinical theory was soundly established when in fact very little of it has been, and virtually all of that thanks to the efforts of nonpsychoanalysts. It is not enough merely to reassure ourselves that psychoanalytic theory has great clinical value. It is, indeed, indispensible, but to affirm that does not release us from the obligation to test, purge, and improve it.

Let us turn next to the other most valuable body of work on the validation of the clinical theory—the papers of Benjamin Rubinstein. Rubinstein is as unusual among analysts as Grünbaum is among philosophers in having a deep and thorough knowledge of the philosophy of science as well as of his own field. Both have the special merit, also, of thinking and writing with great clarity.

Grünbaum relies entirely on direct quotations from Freud and from other analysts when he considers propositions from the clinical theory. Rubinstein, however, *reconstructs* the fundamental propositions of the theory, on the basis not merely of his psychoanalytic scholarship and many years of clinical practice but also from his participation in a long-term empirical research project on the process of clinical inference. Working closely with Hartvig Dahl and a group of cooperating clinicians, he has undertaken the task of logically reconstructing the inferential processes involved in (a) many concrete instances of interpretations offered by the participating analysts who all study the typescript of a recorded psychoanalysis, and (b) in their selections of evidence for a variety of clinical hypotheses.

In 1975 and 1976a, Rubinstein published a pair of remarkable papers, laying out the clinical theory's basic propositions or hypotheses, and specifically addressing the question of whether it could be made self-sufficient. He begins by distinguishing, within the realm of the clinical theory, between the psychoanalytic theory of therapy, which he does not address, and what he calls its cognitive theory.

All of the clinical theory, he notes, is made up of abstract hypotheses, which of course do not refer to particular persons. When you analyze someone, however, you construct as it were a theory of that person, made up of *particular* clinical hypotheses. Each of them is a statement about events in the person's life history and their consequences; typically, they employ theoretical terms such as unconscious wishes and translate the abstract hypotheses of the clinical theory into concrete terms pertaining to one patient. Those are testable, but there are complications.

One of Rubinstein's most notable theoretical insights is his demonstration that, when properly reformulated, all the hypotheses of the clinical theory are *probabilistic*, despite Freud's deliberate attempt to give them universal formulation.

So what *is* the distinction between a *probabilistic* law and a *universally valid* scientific law, also sometimes called nomothetic or nomological? It makes a great deal of difference to the way we write about theory and to the kinds of research we do, whether we believe we are working with one kind of theory or the other. Take one of Rubinstein's formulations of a special clinical hypothesis: "If insulted, a person is likely to feel hurt." Had Freud said it, he might have put it thus: "The person who is insulted feels hurt"; in fact, he would not have recognized the probabilistic formulation as a scientific statement. Yet in nuclear physics, though we can specify to a tiny fraction of a second the half-life of any given radioactive element, such statements are all probabilistic. About any particular atom, it is impossible to say more than that it is somewhat likely that it will undergo radioactive decay at any time (just how likely is expressed in the half-life, essentially a formulation of a probability).

True, some physicists hope someday to discover a "hidden variable" that causes a specific atom to decay at just the moment it does. Science advances to some degree by the discovery of unknown determinants of events which otherwise can be expressed only as more or less probable. No doubt it will be possible to learn a great deal more about what kinds of persons do and do not feel hurt (and to what extent) when insulted to specifiable extents in specifiable ways and in situations the parameters of which also remain to be described and measured. Obviously, however, the relevant research will not be done if no one realizes that it is needed. We don't even have any measurements of the degree of likelihood for most clinically observed behavioral sequences—how frequently we can expect to find them, the analogue of the half-life measurement.

The predominant tendency in psychoanalytic writing is to follow Freud's explicit or implicit claim for the universal validity of statements, but then to formulate them so vaguely that they are immune from the swift refutation that would otherwise so easily be their fate. The big difference in implications for research is that a universal hypothesis can allegedly be disproved by a single clear exception; hence, "crucial experiment" is the ideal form of research in this kind of science. If hypotheses are formulated in probabilistic terms, however, they call for *statistical* research. Right away that raises a question of which most traditional analysts have never even heard: What is the *power* of your statistical tests? Briefly, statistical research demands large samples, which are extremely difficult to obtain in psychoanalytic research.

THE STRUCTURE OF THE CLINICAL THEORY

To return to Rubenstein's argument, here is the basic schema of the clinical theory, as he outlines it:

> (a) . . . all activities in which a person engages . . . are motivated even if on the face of it they may seem to be; (b) . . . within limits people respond to external situations in more or less specific ways; and (c) . . . the presence in a person of certain motives and response dispositions may be explained, at least in part, by events early in that person's life. (Rubinstein, 1975, p. 10f)

From these three major points he draws three classes of *general* clinical hypotheses:

1. *Motivational* hypotheses (e.g., the hypothesis of the persistent manifestation potential of unconscious motives).
2. *Situational* hypotheses (e.g., the hypothesis of in part functionally equivalent situations).
3. *Genetic* hypotheses (e.g., the hypothesis of the development of situation-specific responses into more or less permanent dispositions).

There remain a group of *miscellaneous* general hypotheses (e.g., those involving the concept of unconscious fantasy and that of context fragmentation—that is, that the material of experience may be broken into separate fragments detached from their contexts, and then recombined as in dream condensations).

Then, Rubinstein includes a large set of *special* clinical hypotheses.[1] These are rather general too (e.g., "when faced with an obstacle, most people will try to overcome it") but they differ in being less abstract—they are statements about people, not processes. Other special clinical hypotheses are those concerning the specific types of defense mechanisms and the Oedipus complex. They too may be divided into motivational, situational, genetic, and other hypotheses. As Rubinstein comments, they are statements about people, or can easily be transformed into such statements. I refer you to his text for the full catalogue (which he says is incomplete) and his explanation of the details; I hope the preceding suffices to give some of the flavor of this effort.

[1] Most attempts to specify the clinical theory have concentrated on the special hypotheses, omitting the *general* clinical hypotheses, which obscures the critical role this part of the clinical theory plays in clinical inference.

Many of the generalizations of the clinical theory, when made explicit, sound almost embarrassingly commonplace—a property that led Sherwood (1969) to dismiss them as "platitudes." In my own effort to explicate the clinical theory implicitly used by Freud in the Dora case (briefly described in Holt, 1975), I too found that a great deal of it is common-sense psychology; and I agree with Rubinstein, *contra* Sherwood, that such generalizations play a critical part in psychoanalytic explanations. For the general and special clinical hypotheses, which together form the clinical theory, may be regarded not only as ways to explain behavior, but "as entailing certain rules of inference by the application of which particular clinical hypotheses are inferred." And the latter, you will remember, include the interpretations that make up the explanation of individual neuroses.

CAN THE CLINICAL THEORY BE CONFIRMED?

Now we are ready to come to Rubinstein's ideas about how the clinical theory may be tested. He first addresses the confirmation of *particular* clinical hypotheses (those dealing with single cases), noting that since they too are probabilistic, in approaching their validation we must ask not for a true/false answer but about the degree to which each hypothesis is probable. Second, he points out the fact that many particular clinical hypotheses contain theoretical terms—unobservables like unconscious wish, or repression. Therefore, they cannot be tested *directly*, only indirectly. Yet, "It is part of the logic of scientific procedure that only a hypothesis that is directly testable, or in some way logically connected with directly testable hypotheses, can be confirmed or refuted" (1975, p. 34).

Statements about unconscious wishes or fantasies can give rise to directly testable hypotheses, however, though those are predictions concerning *classes* of behaviors, not specific acts. Thus, if we hypothesize that someone unconsciously hates his father, we cannot be certain that he will have trouble with *all* persons in authority over him or any specific one, or that such trouble will take any particular form, but we can fairly confidently predict that he will behave in one or more ways that make up a describable *class of events* (e.g., "having trouble with authority figures"[2]). Or the analyst may similarly postdict a set of possible events that might have occurred in the patient's childhood to make him hate his

[2] To be convincing, a test of the prediction would have to use uncontaminated judges, applying the rules and definitions to unlabelled case data from the critical patient and from another, judged by the investigator not to hate his father unconsciously. A statistical test of the resulting data would be necessary to confirm the prediction.

father; and he may verify the postdictions when and if the patient reports such events among his childhood memories. Every time such a prediction or postdiction is made and verified, the probability increases that the hypotheses giving rise to the prediction are credible. I would add, parenthetically, that the diagnostic tester proceeds in exactly the same way, forming particular clinical hypotheses by applying the clinical theory to items of test data and then attempting to confirm or disconfirm them on other such data.

Ordinary clinical work is not sufficient, however: Predictions and postdictions must be regularly recorded, and then all relevant evidence recorded too, finally being judged blind, with control data, for relevance to the prediction (or postdiction). Needless to say, little such work has been published! Because so many thousands of us have successfully made predictions and postdictions about particular cases informally, however, the impression naturally arises that psychoanalytic theory has been thoroughly validated in clinical use. As we know, Freud said as much on several occasions.

Yet that is an illusion, based on an insufficient logical analysis of the process of clinical confirmation, which Rubinstein has now supplied. Note that if a particular clinical hypothesis yields correct predictions or postdictions, it does so via some *general* clinical hypotheses; hence it may be said to be confirmed to a certain degree, "but *only* if the general clinical hypotheses involved in the confirmation are assumed to be true" (Rubinstein, 1976a, p. 257).[3]

The situation is even more disquieting, however. With an actual clinical case, Rubinstein shows how a set of confirmed predictions and postdictions produce data just as compatible with another particular hypothesis as with the one that had in fact occurred to him.

> *The choice between the two hypotheses will be determined primarily by the nature of the general . . .hypotheses we decide . . . to adopt.* (Rubinstein, 1975, p. 43; italics in original)

And that choice cannot be made on the grounds of clinical evidence.

Here is one of our central theoretical dilemmas. On the one hand, every confirmation of a specific hypothesis by a successful prediction or postdiction somewhat increases the probability that the more general hypotheses used to make the predictions are true, also, though to a lesser and

[3] I omit here the complicating circumstances that, strictly speaking, any scientific hypothesis can be tested only with the aid of a considerable apparatus of auxiliary assumptions, stipulations about conditions of measurement, and so on. If the prediction fails, some part of this scaffolding may logically be responsible; so the matter is never simple.

unspecifiable degree. As Rubinstein points out, that is "commonly accepted scientific procedure" (1975, p. 50). All too often, the full set of data used to confirm a particular clinical hypothesis (and which therefore partly confirm the special clinical hypotheses entailed in it) are equally compatible with *another* set of general hypotheses. For example, we are familiar with the fact that followers of non-Freudian schools of analysis or of nonpsychoanalytic clinical theories are ready with their own explanations of our cases. It is commonplace that most of these theories, with incompatible general hypotheses, are about equally capable of accommodating one another's data. All of them seem to be confirmed in clinical practice, but they cannot all be true.

The reason for this curious situation, Rubinstein says, lies in the general insistence of psychoanalysts on working exclusively with clinical data from the therapeutic encounter. With that restriction, it is impossible to test a great part of the general clinical hypotheses. Thus, they

> have the status of largely unproven *presuppositions*. A theory based on such presuppositions would be *strictly* clinical in the sense that no nonpsychological considerations would be able to affect it. For this reason such a theory may be regarded as essentially a *system of rules of interpretation*, a hermeneutic system . . . [which is] neither falsifiable or confirmable *as such*. (Rubinstein, 1975, p. 52; italics in original)

"I doubt," Rubinstein drily adds, "that analysts who advocate a strictly clinical theory are willing to accept this consequence of such a theory." There is an alternative, however: to treat all clinical hypotheses "as true scientific hypotheses, that is, as referring to at present unknown processes" (Rubinstein, 1975, p. 52). Notably, the hypothesis of unconscious processes can only refer to hypothetical processes in the brain, probably similar in all respects except for one crucial (but unknown) one[4] to the brain processes accompanying conscious mental processes. So interpreted, the general hypotheses may ultimately be testable neurophysiologically. To be sure, at present that is only a program, but one that can "turn psychoanalysis from a system of rules of interpretation into a developing science" (p. 52). Notice that such a program entails expanding the clinical theory or supplementing it by what is in effect a metapsychology, old or new. And because we cannot yet develop a directly applicable neurophysiological theory of the general and special propositions of the clinical theory, we must be content with models that are, so far as we known, *structurally homologous* both to clinical theory and to neuroscience (Holt, 1981; Rubinstein, 1976b).

[4] The hypothesis that the involvement of the reticular activating system makes the critical difference is plausible but not verified.

Rubinstein (1974, 1976b), Peterfreund (1971), Peterfreund and Franceschini (1973), Rosenblatt and Thickstun (1977), Reiser (1984), and a few others have begun work on such alternative extraclinical theories to replace metapsychology; but it is too early to say what will come of these efforts, and I cannot deal further with them here.

PSYCHOANALYSIS: HERMENEUTICS OR SCIENCE?

Surprisingly enough, there have appeared a number of analysts (e.g., Spence, 1982) who do seem willing to abandon any claim at historical truth, touting the virtues of merely establishing a satisfying complete narrative or "hermeneutic circle" of meaning.

We have now arrived at the second major trend of the three with which I began. Ricoeur (1970) seems to have brought hermeneutics to the attention of psychoanalysts in a book where he argued that their theory was only partly if at all scientific, primarily belonging to another ancient group of intellectual disciplines that began with biblical exegesis. These hermeneutic disciplines, which deal with subjective meanings rather than external facts, were said to have their own methodology, distinct from but on a par with that of science. This dichotomy is a warmed-over version of one promulgated by Dilthey and a group of south German late romantic philosophers just about a century ago (Holt, 1978a, vol. 1, chap. 1). During all these subsequent years, it has been rediscovered periodically by those who hope to find some way to be intellectually respectable without having to exert themselves as strenuously as scientific method demands. And surely, for the last 15 years, many a psychoanalyst has come under the influence of this misleading doctrine, (e.g., Klein, 1976; Spence, 1982; Schafer, 1976, 1978). Of late, an effective counterattack has begun.

I commend to you Blight's (1981) paper and Holzman's (1985), as well as chapter 15 in Eagle's excellent new book (1984) and the 94-page introduction to Grünbaum's (1984). Blight shows how the hermeneuts' entire position rests on their acceptance of an anachronistic and unnecessarily narrow conception of science. Caricature natural science as most advocates of hermeneutics do, and it is easy to accept the proposition that psychoanalysis should be something else—indeed, that there *is* some alternative, equally respectable and defensible way of attaining knowledge. Following Popper (1963), Blight undertakes to show that there *is no* such alternate methodology.

Grünbaum is not satisfied with Blight's argument, if only because it rests upon Popper's account. But Grünbaum (1984) has mounted his own withering attack on hermeneutics from within the citadel of philosophy. He takes on Habermas (1971) and his claim that psychoanalysis is not a natural science but a "critical discipline," and makes sausage meat out of it, as well as the position of those like Ricoeur (1970), Steele (1979), Schafer (1976), and Klein (1976), who argue on such grounds as the assumed difference between reasons and causes, and the special role they claim for *meanings* in psychoanalytic theory. Meticulously, he takes all of the arguments apart, examines them logically and empirically, and shows them to be either trivally true, false, or based on misunderstanding. *Inter alias*, he demonstrates that Habermas's argument is based on an idiosyncratic and partial reading of Freud, and on patently ill-informed misconceptions of the nature of natural science. By and large, the same is true of authors like Schafer, who say that psychoanalysis cannot be a natural science because they seriously misunderstand what science is and how scientists work.

RECENT THEORETICAL "DEVELOPMENTS"

The third major happening of recent years in psychoanalytic theory is the rise first of object relations theory and now of self psychology. Many of the same people who have been swept along by these trends have also been preoccupied with narcissism and so-called narcissistic disorders such as the borderline syndrome. As Eagle and Wolitzky (in preparation) point out, what all these topics have in common is a concern with preoedipal sources of psychopathology, which is in part a healthy reaction against the monotonous tendency of so many self-styled classical analysts to trace everything to the vagaries of the oedipal situation and its aftermath. One hears less talk about ego psychology these days; it is no longer modish.

The last comment betrays my basic reaction to the content of this third trend: that it is largely a matter of current fads. It is not, I think, a mere coincidence that these theories have arisen as metapsychology was declining, but neither one is a substitute for it. Indeed, despite certain attractive features of both object relations theory and self psychology, they fail to make any serious or searching critique of metapsychology, and—like ego psychology—they retain a good deal of it. As rebellions, they are much too limited to accomplish the needed radical, indeed revolutionary, change. Surely it is closer to the truth to reject the primal hate of objects and to recognize the biological irreducibility of attachment, and if Bowlby (1969) is considered a member of the object relations school, the above remarks do not apply to his much more fundamental revisions. Fairbairn (1952), Guntrip (1969), and Winnicott (1958), however, all incorporate far too many of the defective parts of psychoanalytic theory to make their corrections much more than cosmetic, in my view (and in that of Grünbaum, 1984, pp. 246–247). Moreover, like Kohut (1971, 1977) and his followers, they show the same familiar obliviousness to the need for cogent evidence, the same willingness to accept unspecified "clinical experience" as a sufficient factual basis for confident assertions of fact as the traditional Freudians whom they claim to have transcended, but on scientifically unacceptable grounds.

THE CURRENT CRISIS

I would be surprised if some readers were not taken aback or even outraged by such a brief dismissal of most of what is interesting and controversial about current psychoanalytic theorizing. But I am trying to get across to you the depth and urgency of my feeling that when the foundations of our house are tottering, it makes no sense to argue about rival designs for new wallpaper. There are a few sound timbers under there, no doubt, but we have very little idea which ones they are; and we know that there is deep trouble in the philosophical footings themselves.

Perhaps I can clarify the reasons for my sense of crisis if I go back to the clinical theory and sum up its present status. Just as a theory, it shares many of the liabilities of metapsychology. It is a sprawling mess, without boundaries, without definitive formulation of its hypotheses or generally accepted definitions for its concepts, which are intermingled with metapsychological terms and some of disputed status (e.g., unconscious wish). The faults of reification and personification may be found here too. The philosophical foundations of the clinical theory are uncertain, no psychoanalyst having ever plainly specified them. The clinical theory is full of mutually contradictory hypotheses. Analysts keep making new observations, which clash with existing formulations. Instead of trying to figure out what sampling or situational parameters make the difference, the tradition has been merely to say, in effect, "No, *this* is how it is." And the resulting contradiction is never resolved.

So, at the least, the theory needs a tremendous amount of work, of a kind few psychoanalysts possess the qualifications for—notably, a good grounding in the philosophy of science. The unglamorous work of codifying, of defining concepts and formulating hypotheses has been well begun by Rubinstein, but in the years since his groundbreaking pair of papers, there has been no perceptible rush to pick up the baton and hurry it forward. It would be gratifying if these words were to stimulate some theoretically inclined young analysts to become interested in the task and to start preparing themselves for it.

As to the validity of the theory, the picture is quite murky. Remember that probabilistic hypotheses cannot be either clearly confirmed or refuted by a single experiment or other empirical study—a point grasped by few experimenters. Of course, if repeated trials find *no* positive instances, such a hypothesis can be said simply to be false; but I very much doubt that a lot of weeding out will take place that way. More often, it is a question of the proportion between positive and negative outcomes. It is fair to say that virtually none of the needed work has been done–attaching a specific likelihood to every hypothesis in the theory along with a statement of its parameters.

My own reaction after reading and reflecting on Rubinstein and Grünbaum was to do a sort of double take, and ask myself, "Can the situation really be *that* bad?" It is hard to admit how little *proof* there is for any psychoanalytic hypothesis after all these years of use, when the theory seems so clinically valuable and when such a large part of the intellectual world has adopted great hunks of the clinical theory and treats it not as a set of interesting hypotheses but as received knowledge. After all that, *must* we say, "Well, in fact we don't really know precisely what we mean when we talk about unconscious fantasies, or even whether they exist and have important effects"? Yes, we should! Actually, about all we can say on this particular issue is that it makes sense out of a great many otherwise puzzling observations to assume that such processes as unconscious fantasies are active and effective in people, and that alternative explanations lack the simplicity and power of the psychoanalytic hypothesis.

Further, we must admit that during the past three quarters of a century, no one has been able to push this part of the theory forward appreciably. We do not know how or when unconscious fantasies get started; we cannot reliably and confidently say about even a person in psychoanalytic treatment whether she or he has a given fantasy or not, how intense or active it is at any given moment when the person in question is not producing clearly recognizable derivatives,[5] or what determines whether it will be manifest in a dream, in conscious daydreaming, in some autoplastic symptom, in acting out, or in adaptive behavior. By "*What determines whether* . . ." I mean what situational circumstances other than subliminal activation of the kind Silverman (1982) uses in his important continuing researches, interacting with what aspects of personality in persons of various ages, sex, education, etc., and from what culture and era. Such parameters are lacking for every one of the hypotheses making up the clinical theory, to the best of my knowledge.

When it has been possible to test parts of the clinical theory by use of *non*clinical data, it has often come off badly. Results of recent surveys of the attempts to test and verify the clinical theory (e.g., Fisher & Greenberg, 1977) are about as mixed and spotty as those of Sears's (1943) original review. The main reason is, as Rubinstein points out, that we must go to a nonbehavioral realm, such as neurophysiology, to test a great deal of

[5] Which we can't even confidently call derivatives until they have been reliably detected as such by independent and uncontaminated judges.

the most distinctive parts of the clinical theory: Psychoanalysis is *not* autonomous, existing in self-sufficient isolation on an island remote from other sciences. No science can do that, and it was a great mistake for psychoanalysis to have cut its ties to the rest of the scientific world.

The role of whistle-blower doesn't particularly suit me. By nature I am a moderate, a compromiser who starts with the conviction that there is something valuable on each side of most controversies. But this time I feel I would be derelict if I did not seize this opportunity to say, "Hey, really, people—this is an emergency!" Things have gotten so bad that we have to start making radical changes. I believe that if psychoanalysts simply continue down your present path, making no effort at fundamental change, psychoanalysis will continue to shrink and wither, and will eventually collapse.

There is a kind of moral crisis in psychoanalysis about which very little is said, a clash between the values we profess and those we live out. In America, the mass media and the dominant corporations encourage an adjustive style of getting along and going along, but we profess an ideal of personal autonomy and integrity. Do we have the requisite guts really to live by those standards? We are surrounded by a culture of alarming mendacity and systematic disinformation, but we stand for and seek the truth. Yet some of us are happy to qualify that as "narrative truth"—not necessarily what really happened but what makes a good story. In a mixed democratic and authoritarian society, we profess democratic values, but run our organizational affairs in a largely authoritarian way. Above all, Freud taught what Rieff (1959) called "an ethic of honesty," an unflinching readiness to forsake self-deception and to face the facts about oneself. In practice, however, Freud himself was no paragon of self-criticism. He was unusually intolerant of outside criticism, too; I cannot think of a single incident where he really listened to a critic who wasn't part of the inner circle and considered the critique on its merits. Yet he is our scientific model, and analysts have faithfully copied him in this respect.

American psychoanalysis has lived for so long within a snug cocoon of myth that it seems unable to go through the predictable pains of metamorphosis into a viably progressive discipline. The protective threads it has wound around itself include warding off all criticism as resistance, idolatry of Freud, and faithful internalization of all his faults as a scientist and writer. It has therefore failed to develop any *standards* for or means for *improving* (a) the accumulation of facts; (b) the formulation of hypotheses; (c) the testing of hypotheses, or other means to confront theory with fact; (d) the consolidation and collation of the huge theoretical literature; (e) the resolution of disputes, whether over facts or theories; or (f) the training of new generations of psychoanalytic scientists capable of doing anything better than their predecessors. Without self-criticism and a concerted effect to improve, there can be only stagnation.

I'm sorry to have to say that though all of the above failings may be most egregriously true of many medical analysts from the Freudian mainstream, neither nonmedical psychoanalysts nor those of dissident schools have any reason to feel complacent. There are approximately as many medical and nonmedical analysts among the handful to whom one can point as sophisticated theorists or researchers in psychoanalysis.

SOME TENTATIVE PRESCRIPTIONS

Having gone this far in radical criticism, I feel an obligation to say at least something about what needs to be done.

For a good many years, I thought that the necessary reforms might be facilitated by bringing into psychoanalysis behavioral scientists who were already well trained and committed to scientific method and research. Briefly: We tried it and it didn't work. A sustained and fairly expensive attempt by the Foundations Fund for Research in Psychiatry did recruit and train a good many such scientists, but with a minimal impact upon organized psychoanalysis. Some became disillusioned and dropped out; some were totally coopted and became analysts indistinguishable from any other. Most continued to do their original kinds of research alongside a psychoanalytic practice, maintaining an impermeable barrier between the two activities. So I believe that we know pretty well that that is not the way.

Few of the critiques of psychoanalysis have explicitly recognized the special problems of developing it beyond its present status as a sort of protoscience, or at least a science in an early phase of development. The fact is that it would be extraordinarily difficult to take the next steps to improve it. Those should include a theoretical phase and a research phase; ideally, they should proceed concurrently, one helping the other along.

The first step, then, must be to survey the theory, attempting to collect and separately consolidate its metapsychological and clinical-theoretical branches, purging them of obvious fallacies and errors—a job essentially completed as far as metapsychology goes. The clinical theory, on the other hand, needs a great deal more work of the kind Rubinstein has so admirably begun. We must *restate the theory* in such a way as to make it as testable as possible, giving the relation between its theoretical and observational language a clear and unambiguous formulation.

Thirty years ago, at the Research Center for Mental Health, George Klein and I decided to make the attempt. With the help of several younger colleagues, we tried to reformulate the psychoanalytic theory of thinking so that we could test it in our laboratory and more generally in empirical research. I focused on making the theory of the primary and secondary process operational in the form of a manual for scoring manifestations of primary process thinking and its control in Rorschach responses (and, later, other kinds of data like dreams, TAT stories, and free associations). That gave rise to a good deal of research, and indirectly to some efforts to reconsider the theory in light of the findings. I was surprised, at first, by the fact that we never succeeded in finding any Freudian proposition about primary process that was directly testable. We did formulate and test a number of theoretical propositions, but they have not been much noticed by psychoanalysts—partly because I have not yet pulled it all together, asking what the theoretical yield has been (see, however, Holt, 1976b).

Its theory of thinking is a relatively unrepresentative facet of psychoanalysis, however. What is distinctive about psychoanalysis as a psychology, what gives it a special claim to our attention aside from our personal involvement, is its concern with what is most important in human lives. Long before the birth of lifespan developmental psychology, Freud was virtually alone in attempting to make a theory about how human lives grow, are malformed and straightened out, and what determines their major features. Just because of this macroscopic approach, this orientation to the largest issues and the most perplexing dilemmas of human lives, psychoanalytic theory is especially interesting but extraordinarily difficult to test. The clinical psychoanalyst has an unrivalled opportunity to make observations of potentially broad importance, to formulate observed or intuited regularities, and to frame hypotheses. The treatment situation has many grave deficiencies as a setting in which to test and validate hypotheses, however; but how else are the necessary data to be obtained? The main trouble is that one has to expend enormous amounts of time, effort, and money to get such data on a single case, while the probabilistic nature of the theory demands statistical studies with *many* cases.

Too many analysts treat it as obvious that only the data of the psychoanalytic hour are relevant to answering questions about the clinical theory. Actually, a good deal can be done with the techniques of multiform personality assessment, particularly when applied in such longitudinal research as that of Jack and Jeanne H. Block (1980). Indeed, I am convinced that it is easy to exaggerate the comprehensiveness of psychoanalytic clinical data. Often enough, an analyst knows very little about several departments of a patient's life. Those can be probed by systematic inquiry of a kind the therapeutic enterprise does not need and in fact countermands. And such techniques can yield surprising amounts of confidential, highly personal and conflict-ridden confessions to a skillful interviewer in a research setting (see, e.g., the work of Vaillant, 1977) and direct observations of familial interactions in place of one participant's reports (see, e.g., Hauser, Powers, Nom, Jacobson, & Follansbee, 1984).

Therefore, systematic empirical research on the clinical theory must be done, using data other than those of the treatment. Let's have all of such research we can get; but do not expect to see much of it, especially not from the psychoanalytic institutes. It requires teams of highly trained clinical researchers, a favorable setting in which they can establish good rapport and gather data over long periods of time, and money—lots of it, providing secure support over long times. For many problems, that means decades. To some extent, such protracted work can be carried out with changing personnel if the attrition and replacement is gradual and if the main leadership remains constant.

Another needed and feasible kind of research that can feed back to the clinical theory uses tape recordings of complete psychoanalyses, or at least of multihour segments of them (e.g., the work of Luborsky, 1967; Silberschatz, 1978; Gill & Hoffman, 1982). Dahl has proposed and has begun assembling an eclectic library of such data, not limited to classical Freudian treatments; the data are available to any qualified researcher. Obviously, the value of each case grows as the recorded hours are accompanied by accessory data, and as various kinds of indexing, summarizing, compilation, and coding are performed and their results stored as a permanent addition to the library. Again, however, the work is very expensive, requires many talents not likely to be found in single individuals, and virtually none of it can be done without the aid of psychoanalytically trained judges to perform various kinds of blind analysis of the raw data. This complex, demanding, expensive work has the great merit of solving the problems of public access (Wallerstein & Sampson, 1971).

It would be greatly helped along if training institutes would require all candidates to submit one recorded case of no less than 100 consecutive hours before graduation, along with their notes and a diagnostic workup. Supervising analysts could be required to keep some kind of record of their observations also. To become even more fanciful, a condition of a senior psychoanalyst's attaining the status of training analyst could be the submission of the complete recording of a reasonably successful case. Those would be rather substantial contributions to research, which would cost something but not nearly as much as would be necessary otherwise to build up quickly enough a data base of substantial size.

If training institutes had active research branches where competent teams were working with the cooperative data base just described, it would contribute greatly to the training of future generations of more scientifically oriented psychoanalysts. The senior investigators could offer required courses in the institutes on the problems, rationale, and techniques of controlled clinical research. Candidates would naturally be drawn into the work, serving first as judges and learning first hand what rigorous investigation is like. Obviously, only a minority would go on to take more important roles and to become investigators themselves, but the numbers would doubtless be considerably greater than at present, when the supply is alarmingly small.

In a thoughtful recent paper, Holzman (1985) attributes the neglect of research and the scientific stunting of psychoanalysis to its exclusive focus on therapy. He overlooks, however, the excellent economic reasons for this focus. The situation remains now as it has been since the beginning: The one way to make a secure living in psychoanalysis is private practice. No career lines exist for anyone who might have the fantasy of becoming a psychoanalytic scientist, no obvious job openings and no prospect that society will ever find enough of a need for such people to establish the necessary institutional base. We probably can't expect working psychoanalysts literally to tithe in order to support research centers, though many are already voluntarily giving substantial amounts of their incomes to the American Psychoanalytic Association's Fund for Psychoanalytic Research, which has funded some much needed work. Without a basic change in our crisis-ridden, absurdly expensive and dilapidated system of providing medical care, I see no prospect that the necessary economic base can be provided for the heroic effort needed to reorganize, redirect, and reform psychoanalytic theory. Let's hope I am wrong! For it would be an enormous loss to psychology if the great insights of psychoanalysis faded away without ever having had a chance to be converted into a real science, and a huge loss to society if it were deprived of the unique contribution to human welfare that clinical psychoanalysis at its best can make.

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