

IS PSYCHOANALYSIS

“OUR SCIENCE”?

*Reflections on the  
Scientific Status  
of Psychoanalysis*

SAUL I. HARRISON, M.D.

DESIGNATING PSYCHOANALYSIS “our science” evokes a variety of reactions. One of the more disturbing sensations stems from recollection of the odious comparison made in the Hook (1959) symposium between psychoanalysis and proprietary pseudosciences such as astrology, chiropractic, and phrenology. Obviously, being distressed by such a comparison is not an idiosyncratic reaction. The accumulating evidence indicates that psychoanalysts increasingly prefer the company of the other sciences who constitute the various sections of the American Association for the Advancement of Science and university faculties. Nevertheless, many people question the appropriateness of discussing the *scientific* status of psychoanalysis, to which the philosopher Richfield (1954) replied: “if the meaning of *science* is to be purely a pragmatic convention, determined and exclusively modified by the nature and problems of quantum physics, then the question of psychoanalysis as science might be unworthy of our consideration. But if *science* is to mean, among other things, some distinctive ways in which men actually deal with their experiences, then it is the responsibility of the psychoanalyst to play a prominent role in the creation and development of scien-

---

Professor of Psychiatry, University of Michigan Medical School, Supervising Psychoanalyst, Michigan Psychoanalytic Institute.

These reflections were first drafted in 1966 while the author was an observer at the Hampstead Child-Therapy Clinic in London with the support of a Commonwealth Fund Fellowship and a Grant Foundation Award. They have been presented to the British Psycho-Analytical Society and the Michigan Psychoanalytic Society.

tific principles. Psychoanalysis cannot passively await the outcome of the convulsions that currently beset the philosophy of science."

Indeed, of the several ways in which the word psychoanalysis is employed (therapeutic technique, investigative method, compilation of data, and theories about the data) all except the therapeutic aspect relate *exclusively* to the scientific status of psychoanalysis. This is not to say that psychoanalysis as a treatment does not have vital scientific aspects, but to recognize that therapy necessarily introduces vital "extrascientific" considerations. This was highlighted recently by Bond's (1965) speculation that psychoanalysis as a science probably would be further advanced if psychoanalysis did not possess its therapeutic potential. He raised the tantalizing question whether physiology would have developed as much if simultaneously it was a means of treating patients. Addressing himself to the clinician-investigator split in psychoanalysis, Knapp (1967) perceived it as analogous to the dual research tradition of experimental and naturalistic observations. It was his assumption that without its therapeutic potential, psychoanalysis would not have developed as an investigative method or as a theory because people would have not cooperated without the motivation of pain.

Failure to distinguish between the several meanings of psychoanalysis facilitates an unrealistic insistence that therapeutic cures be demonstrated as substantiation of psychoanalytic technique as a reliable investigative method and as proof of the validity of psychoanalytic theory. It is not my intention to minimize the importance of the ethical and moral concerns that may underlie such demands. It is important, however, to recognize that the vital question of the therapeutic value of psychoanalysis constitutes an unprofitable distraction in the context of a discussion of the status of psychoanalysis vis-à-vis other sciences. The complex issue of the worthwhileness of psychoanalysis as therapy is more appropriately explored in discussions (Harrison and Carek, 1966; Harrison, 1967) focused on treatment. Furthermore, the importance of distinguishing between the various meanings of psychoanalysis is highlighted also by the potential for confusion implicit in the fact that while the techniques of

psychoanalysis are psychological, the theories of psychoanalysis are not entirely psychological.

### *The Scientific Status of Psychoanalytic Theory*

Psychoanalytic theory has varying significance for different people. At one extreme are those who attack it as being unfounded, dangerous, evil, etc. At the other extreme are those who believe it is not only true but the only possible truth. It is my impression that those who profess this latter attitude of complete faith in psychoanalysis frequently employ the phrase "our science." It should be recognized, however, that there may be far more worthy motives for use of this phrase. One of these derives from the contention that only the psychoanalytic method can demonstrate the validity of psychoanalytic theory. Thus, one has to employ psychoanalysis, i.e., be a psychoanalyst, in order to test the theory. This meaning of "our science" is markedly different from that of blind faith. Those who use the phrase "our science" in this second way are vitally aware of the importance of testing the theory. In fact, this second group includes some of the most dedicated psychoanalytic investigators of recognized intelligence, integrity, and sincerity.

Although those who profess unquestioning faith in the validity of psychoanalytic theory occupy a pole of prejudice exactly the opposite from those who consider psychoanalysis to be an enemy, both nevertheless share the common ground of ruling out the need for any research in psychoanalysis. For each, the immutable facts are available already and further evidence would be unconvincing. It is assumed that each of these opposing global views of the totality of psychoanalytic theory are patently inaccurate.

Psychoanalytic theory is a series of plausible propositions, usually logical, though at times contradictory, that have been offered as an explanation for phenomena observed in clinical work. As such, they are a series of hypotheses, all of which should be tested if possible. Some have been exhaustively tested and demonstrated to be established fact. An example is the existence of unconscious mental functioning, which should be distin-

guished from the topographical concept of "*The Unconscious*," which has not been demonstrated as convincingly to be an established fact. Other hypotheses have been subjected to testing without establishing unquestioned validity. An example is the proposition that unconscious mental functioning has certain qualities that are so similar in all people that much as we may differ on the surface, underneath we all resemble one another sufficiently so as to be considered essentially identical. Still other hypotheses have proved difficult if not impossible to test at our current level of knowledge despite possessing considerable explanatory value. An example is the useful theory that there are two types of drives, libidinal and aggressive.

Classification according to testability and validity should be separated because while it is evident that ease of testability does not indicate validity, it appears that it is not always as evident that untestability should not be equated with lack of validity. Unfortunately, there is a tendency to assume a hypothesis is invalid if there is insufficient knowledge with which to devise a test or the means to implement a conceivable test are lacking. Inability to test a proposition does not necessarily reflect on the inherent worth of the proposition. The physicists formulated valid as well as invalid theories about energy and matter before they developed the ability to test their hypotheses. It is of interest that a variety of comparison between the scientific status of psychoanalysis and physics are a popular topic among psychoanalysts.<sup>1</sup>

Such comforting analogies, however, should not relieve us of the responsibility of endeavoring to confirm psychoanalytic hypotheses and classificatory schemes. This temptation is all the more heightened in psychoanalysis because of the fruitful interpenetration of hypothesis construction with fact finding that Hartmann (1958) discusses. The risk is great that the hypothetical character of a proposition may be forgotten when highly abstract concepts such as psychic energy and libidinal cathexes are "described" as if they were the actual data of observation. The distinction between description and explanation is consid-

<sup>1</sup> See for instance Frenkel-Brunswick (1954), Waelder (1960), Guttman (1965), GAP Committee on Research (1966).

ered critical by Kubie (1960) who viewed theoretical formulae in psychoanalysis as usually a "hybrid and confused mixture of the two." Indeed, it is remarkably tempting to assign causal significance to psychoanalytic propositions, even while recognizing that accurate and valuable psychoanalytic formulations which comprehensively include genetic, topographical, structural, economic, dynamic, and adaptive points of view may be of minimal, if any, etiological consequence.

Less controversial than classifying hypotheses according to the degree of testability and the degree of validity is the valuable ordering of the same concepts in a hierarchy reflecting their distance from the data of observation. Waelder (1962) has done this as follows:

1. The *data of observation*.
2. *Clinical interpretation* regarding the interconnections and relationships of the observed data.
3. *Clinical generalizations* such as the impact of certain experiences on a particular psychopathological symptom or syndrome.
4. *Clinical theory*, which is a derivative of the foregoing interpretations. Here, Waelder includes readily demonstrated phenomena such as repression, defense, and regression as well as hypotheses that are not as readily demonstrated such as return of the repressed.
5. *Metapsychology*, which involves theories about the theories resulting in far more abstract concepts such as psychic energy and cathexis.
6. Last, Waelder lists Freud's *philosophy*. The question might be raised, however, whether this should be limited to Freud's personal philosophy or whether there is a philosophical system implicit in psychoanalysis entailing such values as faith in reason and truth which belongs at this level most distant from the observed data.

Waelder observes that nonpsychoanalysts know the most about those more abstract levels which have the least relevance for psychoanalysis such as Freud's personal philosophical writings about religion and civilization. Next, he feels that outsiders

know something about those speculative concepts that Waelder locates "on the fringes of psychoanalytic theory," e.g., psychic energy. He goes on to observe that some of the best analysts that he has known knew next to nothing about metapsychology. Regretfully, the criteria employed in this value judgment about "best analyst" are not articulated. In a personal communication, however, Waelder (1966) named as examples some of the more productive clinical contributors to the psychoanalytic literature.

It is easy to agree with Waelder to the extent that these are the vulnerable fringe areas that certain critics of psychoanalysis elect for the focus of their attack. On the other hand, it appears that there is, in fact, greater awareness as well as acceptance of the more basic concepts such as unconscious mental functioning as represented by the displacements of transference, parapraxes, mind-body interrelationships, etc.

Sophisticated nonpsychoanalytic colleagues are often unrelenting in their demand for evidence that they can understand. Psychoanalysts, particularly those who are uneasy with the proprietary implications in the term "our science," have an uneasy need to respond. Empathy with this uneasiness is common when "our science" reflects the regrettably rigid dogmatism that resembles religious fervor in that it is born of faith rather than observation. There is more controversy about this phrase, however, when it expresses the sober, and thus far apparently accurate, appraisal that nothing demonstrates psychoanalytic theory as convincingly as the psychoanalytic method. Before responding to the demand for evidence, it is vital to recall that these nonpsychoanalytic colleagues' attitudes toward psychoanalysis differ markedly from those with which they consider other sciences outside of their area of expertise. The chemist accepts the reports of the astronomer without examining the astronomer's evidence and the astronomer similarly accepts the reports of the chemist at face value. Yet most scientists are curious about the proof of psychoanalytic propositions. The most obvious reason is that psychoanalysis deals with such personal matters that few people can be neutral about it.

The fact that this demand may come less frequently from physical scientists than it does from other behavioral scientists

should not be surprising in light of the fact that the areas of study overlap. However, the crudeness of some of the demands for evidence that come from colleagues in similar fields may express other attitudes in addition to shared interests. It may take the form of a demanding "show me" attitude about matters that are not demonstrable. It is incumbent on psychoanalysts to realize that such demands may be encouraged in part by the psychoanalytic use of metaphorical language, particularly when it approaches the extreme of concretizing abstract concepts and classificatory systems. It is not hard to imagine a vicious cycle that begins with an analyst talking of "*the ego*" which the behavioral scientist, perhaps tendentiously, asks to see, following which the eager analyst attempts to "show" him. When the behavioral scientist is unable to see "it," the analyst may be tempted to talk about the outsider's resistance toward "our science" and to redouble his conviction that it has to be "*our science*."

It is amazingly easy to forget that the structural hypothesis is nothing more than a means of ordering data in what is really a semiarbitrary though extremely useful fashion. The structural hypothesis encompasses abstract conceptualizations that are derived from classifying related functional components into abstract psychic systems. Underlying this classificatory scheme is the observation that established functions tend to "structuralize" in the sense of forming steady states that resist change and do not have to be created anew repeatedly. Thus, the so-called structures are really systematic groupings of functions forming theoretical constructs. In a sense, they are logical artifacts that are inferred from behavior. As such, neither the ego, id, or superego is observable, nor are they *unobservable*. Their validity cannot be proved any more than the existence of the cardiovascular system can be demonstrated to be more valid or accurate as a system than the cardiovascularrenal system or the cerebrovascular system. Similar examples could be cited from astronomy, chemistry, and a host of other sciences.

An apparently more refined, but equally impossible, external demand for evidence requests that psychoanalysts articulate what data will disprove the existence of a complex constellation like the oedipus complex. Such requests have been cited

repeatedly by critics and defenders of psychoanalysis alike. Unfortunately, however, as tempting as this challenge is, it will not bring testimony to bear about the scientific status of psychoanalysis. In brief, a little boy's aggression to his father or his solicitude toward his father both can be considered behavioral evidence of the oedipus complex. Such apparently irrational "heads I win, tails you lose" self-fulfilling prophecies frustrate logicians intensely, and the efforts of psychoanalysts to clarify the matter may prove even more frustrating (see Hook, 1959). The issue to be considered, however, is whether psychoanalysis should be blamed for being illogical or whether it is human personality functioning that is so filled with paradoxes. In other words, whose logic should prevail?

Clearly, a means of noncircular confirmation of hypotheses would be most helpful. Noncircular methods that *truly* relate to the questions at hand are exceedingly difficult to design. For instance, what would be proved about psychoanalytic theory by study of autistic, retarded, and/or encephalopathic children who do not manifest evidence of an oedipus complex?

What criteria can be applied to assessing the value of a theory? Obviously, a useful and relevant theory should be in accord with the available data. Next, one expects a certain degree of inner consistency. This should be distinguished, however, from fitting in with the rules of logic, for the theory may be about illogical matter such as human behavior. Also, the theory should be comprehensive, which is not meant to imply that other explanations that are judged to be less encompassing should be eliminated. Indeed, it is not an either-or choice between the data of psychoanalysis and the data of learning theory, or social role theory, or neurophysiology. Each enriches the other. Last, the theory should be articulate and have a fruitful capacity for generating additional hypotheses.

Psychoanalytic theory has been taken to task for translating insights derived from study of the so-called abnormal into the foundations of a general psychology. It is probably true that exclusive study of the abnormal would be limiting in the development of a general psychology. It ought to be recognized, however, that exclusive focus on the so-called normal could be as limiting.

In recent years biologists have become increasingly aware of this, and many have been endeavoring to establish a reintegration of pathological and developmental biology.

### *Psychoanalytic Investigative Methods*

It is agreed that the scientific method entails observation, classification, verification, developing the theory, and testing the hypotheses. Although the merit of this generalization is indisputable, one should not lose sight of a potentially tyrannical fallacy in the use of the word "method." Can this particular word implicitly convey the idea that there is a specific scientific technique that is applicable to varying foci of investigation such as bacteriology, astronomy, physics, surgery, history, chemistry, archaeology, anatomy, demography, etc.? Clearly, the word "method" should not convey technical restrictions but rather should indicate that there is a scientific attitude and approach to problems which underlies a variety of different technical procedures.

The nature of astronomy, for instance, dictates that the astronomer observe distinct phenomena without benefit of the rigorous means of verification available to laboratory scientists. The theory of evolution, archaeological explanations, and many of the so-called facts of history cannot be tested in the same way that it is possible to demonstrate by means of applying Koch's postulates that bacteria cause certain infections. Yet, the reconstructions offered by Darwin, archaeologists and historians are generally accepted because available alternate interpretations are not as creditable, and because every conceivable effort has been made to minimize the possibility of coincidence, which, it should be emphasized, is not a guarantee of its absence.

Use of the classical psychoanalytic technique as a method of investigation entails scientific processes akin to the historical and archaeological methods. Yet, there are influences constantly beckoning, if not pressuring and seducing, psychoanalysis in other directions. At one extreme are those elements of the community who view psychoanalysts as omniscient mind readers. If psychoanalysts succumb to behavior which fits this expectation, the

charlatanism label, implicit in the guilt by association with astrology, becomes uncomfortably accurate. Other critics find it hard to trust psychoanalysts who allegedly are too enamored of their theories, and they recommend the adoption of safeguards similar to the legal adversary system. There are others who expect psychoanalytic research to resemble investigation in the more readily quantifiable and loosely coupled physical sciences. The growing efforts to satisfy such expectations can lead to frustration and grief. This does not imply that attempting a rapprochement between the physical and psychological sciences is not worthwhile. Attempts to apply the techniques of some of the physical sciences to psychoanalytic investigations, however, ends up far too often in the application of wonderfully rigorous statistical measures to relatively inconsequential data. Waelder (1962) aptly likened such efforts to insisting that the ugly duckling be a duck instead of a swan.

The contemporary climate emphasizing sponsored research provides an atmosphere in which it may be difficult to enhance the scientific status of psychoanalysis, without destroying its rich potential. The tradition of psychoanalytic research being supported by patient's fees, which raises ethical questions that are beyond the scope of this essay, no longer provides a basis for a promising future. There is uncertainty as to whether such an arrangement could continue to attract the most creative young people. Unrecognized investigators are in a difficult position in that in order to be granted support they often feel obliged to apply a type of systematic quantitative approach that makes their investigation less psychoanalytic. The more psychoanalytic the investigation, the more difficult it is to introduce this more readily rewarded type of methodological rigor. This inverse relationship between psychoanalysis and quantification is largely a consequence of the nature of the data under study. Psychoanalytic reliance on private introspective material results in the fact that even in the highly promising and fruitful dream research, the investigator observes only the behavioral and physiological correlates of the dream but is not able to observe the dream itself. Accordingly, he is forced to rely on the subjective report of the individual under investigation. This is remindful of Kety's (1960) speculation

that "there may someday be a biochemistry or a biophysics of memory—but not of memories."

*Experimental Methodology in the Laboratory and Applied  
to Naturalistic Observations*

Lustman (1963) wisely emphasized that *experimental* sophistication and *scientific* sophistication are not synonymous. He contends that it is the presence of *scientific* sophistication which prevents analysts from distorting and diluting their area of study through the use of available experimental techniques that are more appropriate to other scientific disciplines.

The most satisfying experiments are designed so as to repeatedly and predictably manipulate the relationship between two variables. While psychoanalytic propositions do not lend themselves readily to such translation into two factors because of the nature of the data under study, we should never dismiss the possibility that part of the difficulty may stem from the nature of psychoanalytic conceptualization. It remains to be seen whether clearer definition and delineation of psychoanalytic concepts (see Sandler, 1962) will permit the expression of psychoanalytic theory in a form closer to the classical experimental form.

Indeed, the shortage is not in the quantity of experimental studies but rather in their limited fruitfulness in relation to the data of psychoanalytic investigation. Wolman (1964) considers the "pseudoevidence" of many experiments to be dangerous. The methodological rigor demanded in Ph.D. dissertations makes many of them extremely far removed from what they intend to investigate; they are reminiscent of the old story of the drunk searching for his keys in the well-illuminated area under the street light even though he dropped them in the dark alley. In a similar vein one can recall the statistically oriented observer who claimed that his girlfriend has two of the prettiest legs in the whole world and when his colleague asked him how he knew, he responded by saying "I counted them."

Has it reached the point where the student goes through exercises in methodology just as the student in years past was required to conjugate Latin verbs because it was good for his

self-discipline? Unfortunately, this new influence extends beyond the student years, for there is a tendency for funding agencies to demand methodological rigor, sometimes unrelated to the consequence of the data.

These largely external pressures are not the complete story. On the other side, there is a regrettable tendency among analysts to accept and cite only those experiments which agree with psychoanalytic theory and a temptation to dismiss as irrelevant those that do not. Psychoanalytic thinking lends itself far too readily to this sort of "heads I win, tails you lose" response, which can be avoided, unlike inevitable paradoxes such as the fact that both solicitude and aggression may be considered behavioral evidence of the oedipus complex. In consequence, some interesting observations are rarely pursued in psychoanalytic work. For example, Sears and Wise (1950) reported that children who have been cup-fed from birth do not demonstrate a greater tendency to suck their thumbs or other objects, nor do they manifest a larger number of feeding and speech disturbances than children who sucked at the breast or the bottle. Like those observations of hermaphrodites that led Money, Hampson, and Hampson (1956) to explicitly challenge the cherished psychoanalytic view that sexual behavior and orientation as male or female has an innate instinctive basis, Sears and Wise were quick to conclude that pleasure in sucking is a learned response, in their view thereby invalidating the psychoanalytic concept of orality. Such far-reaching theoretical conclusions are not justified, in my opinion, but it does seem unfortunate that such superficial behavioral observations and correlations are not integrated more in psychoanalytic thinking and investigated in greater depth psychoanalytically.

It would be a monumental task but perhaps a worthwhile one if a committee of analysts undertook to evaluate the methodology of the many experiments that have been performed or are about to be performed. They might clarify the relevance of the experiments to psychoanalytic theory. This would probably entail defining new, and probably less ambitious, hypotheses for many of these experiments. They could re-examine the results in relation to their redefined hypotheses. Such a huge undertaking not only would meaningfully filter experimental results already

available but would also serve to expose the analytic community to more information about methodology in accord with Kris's (1947) call for the need in psychoanalysis of "trained clarifiers" and Hartmann's (1958) and Bellak's (1961) plea that psychoanalysts be trained in methodology. Furthermore, such a project might be helpful to generations of future psychologists who undoubtedly will devote much energy and talent to such experimentation and to the applications of experimental methodology to naturalistic observations.

Psychoanalysts have become increasingly involved in naturalistic observations in the promising and fruitful field of psychoanalytic child observation which Kris (1947) considered to be one of three types of psychoanalytic research (the other two are the psychoanalytic technique itself and experimentation). Noting that the data are always relatively superficial and thereby pose the risk of speculation, Lustman (1963) compares psychoanalytic child observation with utilization of the manifest dream without benefit of the dreamer's associations. Behavior is the only datum of observation as all else is interpretation. A variety of efforts have been made to overcome these shortcomings, notably the formation of teams that seek consensus. Such teams may be mutually enriching and stimulating, but do they invariably enhance the chance of convincing proof? Obviously, popular approval need not correlate with accuracy. Descrying the minimal discrimination in analysis between controlled research and individual opinion, Glover (1952) observed that when an opinion is expressed by a prestigious senior analyst who is enthusiastic, persuasive, or dogmatic, the chances may be great that the opinion will be considered "fact." The degree of privacy ordinarily assumed to be a prerequisite for psychoanalysis further complicates such risks by facilitating the authoritarian misuse of an otherwise valuable concept, such as psychic depth, to obscure the fact that the emperor is uncertain whether he is clothed. Akin to this is meeting skepticism with pontifical words like: "You'll understand it when you are as experienced as I."

Needless to say, such problems are not limited to psychoanalysis. A host of examples of the experimenters' effects on behavior of both humans and the ostensibly more objective labora-

tory animals have been studied and summarized by Rosenthal (1966). One of the most striking examples derived from one of the so-called hard sciences has been described by Kety (1961). This was the experience in physics wherein all the independent measurements of the velocity of light reported in the literature over a twenty-year period clustered in the vicinity of an apparently erroneous determination attributed to Michaelson and published by his associates following his death. If this had been a consequence of pure chance or experimental error one would have anticipated a random scatter of the results. Writing in the *American Journal of Physics*, Bearden and Thomsen (1959) therefore conclude "that the experiments were not really independent but that there was a subconscious psychological factor which tended to make each experimenter look for errors in technique until he could check the then accepted value." In the same journal two years earlier Mulligan and McDonald (1957) had stated: "It does seem clear from the history of physics that occasionally the result of a very high precision measurement of a physical constant by someone eminent in the field has intimidated other workers from publishing results in substantial disagreement with this value. . . . It seems at least possible that other experimenters in the years from 1934 to 1949 may have found higher values of speed by optical methods but refrained from publishing their results because of their disagreement with the determinations . . . in which such great confidence was placed at that time."

The potential for such observer bias is so multi-faceted and omnipresent in psychoanalytic work that the concept of countertransference continues to assume increasing importance for psychoanalysts all the time. Furthermore, simultaneous but independent observation of the same phenomena would seem to hold additional promise for overcoming some of these pitfalls; however, the nature of the behavioral data in child observation, for example, is such that agreement would be extremely difficult to achieve as the independent observers may be attending to different units of observation. In fact, it is the amalgamation of these *different* observations that makes the team approach so potentially rich.

To note some of the shortcomings of psychoanalytic child

observation as an investigative technique is in no way intended to decry this approach. Indeed, it would appear that if these investigations lead to predictions and hypotheses that are subsequently tested by psychoanalytic technique, this constitutes a scientific approach of considerable promise. At our present level of knowledge, however, a great deal more needs to be learned about the complex issue of predicting human behavior.

### *Validation of Reconstructions in Psychoanalysis*

Prior to discussing predictions, it should be noted that the subsequent analysis of children whose development has been carefully observed and meticulously documented opens up a very rich potential for testing the validity of psychoanalytic reconstructions, the interpretations that bring the analyst, in his daily work, closest to intuitive artistic creativity. Many interpretations during the course of analysis "predict the past" and as such have been labeled "postdictions." Only rarely in psychoanalysis do fortuitous circumstances permit convincing verification of these reconstructions of the patient's past. Such instances have been described by Bonaparte (1945) where an old man's confessions confirmed primal scene memories and by Flumerfelt (1962) when hospital records substantiated psychoanalytic reconstruction.

The author had a comparable experience of physical reality confirming psychic reality during the course of an analysis. The interpretation was offered to a woman, who vigorously rejected all surface manifestations of femininity, that the evidence derived from dreams, transference developments, and other associations indicated that she must have been comfortably feminine prior to the birth of her brother when she was four years of age. The patient reacted with the accusation that the analyst was attempting to influence her to be feminine by "brainwashing" her with the idea that there was a dormant part of her that was feminine. Her violent protest suggested that the content of the interpretation was accurate even if the timing may have been off. (Yet, in keeping with the "heads I win, tails you lose" logic so predominant in analysis, had the patient responded with an "aha" reaction followed by relevant associations, that too would

have seemed confirmatory of the accuracy of the content. Obviously such circular validation of "postdictions" constitute scientifically unconvincing self-fulfilling prophecies.) After a subsequent holiday visit to her grandparents' home, the patient reported that the family had happily reviewed many reels of her grandparents' home movies. The films revealed that until the time that her baby brother started appearing in the pictures, the patient wore frilly dresses and seemed light and gay and flirtatious. When her infant brother started to appear, she wore slacks and lumbered like a heavy somber bear.

Without disagreeing with Flumerfelt's (1962) conclusion that "external validations are not apt to be as accurate or informative as the record in the mind of the patient," it should be emphasized that such data have an inherent persuasiveness that can also enable us to refine our methods and add to our knowledge. Therefore, systematization of such verification of reconstructions ranging from Serota's (1964) study of analysts' home movies to the combination of longitudinal child development studies and subsequent psychoanalytic reconstruction holds promise for enhancing the scientific status of psychoanalysis. The possibility of systematically testing "postdictions" and perhaps even the inestimably more complex predictions offers psychoanalytic investigation an advantage that the historical and the archaeological methods, which are so similar to psychoanalytic method, cannot implement as readily.

### *Predictions and Psychoanalysis*

The growing psychoanalytic literature devoted to predictions, which many people consider to be a vital standard of scientific methodology, follows Freud's (1920) advice of caution. M. Kris (1957) describes the complexities of attempting to predict child development in the context of longitudinal studies. Escalona (1952, 1959) urges caution in assessing the verification of predictions as offering support for hypotheses, noting that predictions may be correct for reasons other than those that led to the formulation of the prediction. Waelder (1962) doubts that psychoanalytic predictions will satisfy other scientists. He recommends

distinguishing between diagnostic and prognostic predictions. Diagnostic ones relate to typical groupings of symptoms (i.e., if a patient reports compulsions, one can predict penuriousness), whereas the more complex prognostic predictions attempt to predict the future. He reasoned that if the physicist predicted the future of the individual electron as we attempt with the individual child, the physicist would be equally inexact. The physicist, however, does not limit his predictions to such tiny units but rather makes predictions for aggregates of these units. In this sense, Waelder argues that the physicist's predictions are more like the predictions of the epidemiologist or the demographer. The physicist's predictions are comparable to predicting the average life expectancy of a nation of people in contrast to predicting the life expectancy for an individual.

Bellak's (1961) view of the potential of predictions differs from those opinions cited above. Optimistically, he seems to view each interpretation offered a patient in analysis to be a form of a predictive and/or postdictive hypothesis. Bellak envisions that, with increasing attention to methodology, the supervisor-supervisee relationship could be an investigative team as well as a teaching device. As indicated earlier, he joined Hartmann (1958) in urging increasing attention to methodology in psychoanalytic training.

This optimism about predictions is supported by Knapp (1963), who described his experience with short-term predictions; and by Wallerstein (1964), whose experience has included long-range predictions. Wallerstein vigorously states his agreement with the views of Ezriel (1950, 1951, 1952), Ernst Kris (1947), and Kubie (1956) "that the psychoanalytic situation as it naturally is itself fulfills essentially the requirements of a quasi-experimental research model. According to this view, the psychoanalytic situation is a relatively stabilized, recurring experimental situation in which the experimenter (the analyst) introduces independent variables (interpretations and other specifiable interventions) and can then predict and ascertain their impact on all the dependent variables within the situation, in which after all he has the fullest conditions of access to the subjective data that enter consciousness (no matter how seemingly remote or trivial) ever devised . . . pre-

diction becomes a method for the transformation of the assumptions underlying the clinical judgments of analytic therapists into testable hypotheses."

Rapaport (1960) was of the opinion that the nature of the material available to Freud resulted in Freud's overemphasis of "postdiction" and underemphasis on prediction in the development of psychoanalytic theory. Rapaport questioned whether such an imbalance is not typical for any science in its infancy and went on to state that "the basic necessary condition for predictions and for their confirmation is present in the theory of psychoanalysis . . . the task ahead is to add to the *necessary* conditions of prediction the *sufficient* conditions, by tightening the theory and by developing adequate methods of quantification and confirmation."

In concluding an extensive discussion on prediction in which he noted that much can be learned from inaccurate predictions, Benjamin (1959) wryly observed that some of those "who take a pessimistic point of view about the possibility of systematic research in our field, on the ground that things are too complex ever to permit the necessary simplification . . . have no hesitation in 'explaining' these complex phenomena with the utmost simplicity and authority."

### *Psychoanalytic Evidence*

If it is agreed that inaccuracy of predictions does not disprove psychoanalytic theory, should it be emphasized also that the adverse of this holds true? In other words, can accurate predictions be cited as evidence of the validity of the theory if inaccuracy does not point to lack of validity? As Escalona and Heider (1959) have emphasized, the prediction may be correct for reasons that are entirely different from those that led to the formulation of the prediction.

It cannot be reiterated too often that psychoanalysis needs to guard against the appearance of citing only the evidence that supports that theory. Even those who are convinced that psychoanalytic theory can be tested only in the psychoanalytic situation cite data from anthropological and experimental sources that sup-

port the theory. While it is reasonable to do so, study must be made of data from similar sources that appear to contradict the theory.

Demonstration of statistically significant correlations between certain childhood events and subsequent adult behavior is not necessarily evidence of a causal relationship. As Hartmann and Kris (1945) point out in their discussion of the genetic point of view in psychoanalytic metapsychology, the crucial psychoanalytic question is why a particular adult outcome was chosen out of the multitude of possibilities available. Answers to such questions are best derived from the psychoanalytic technique with its recapitulation of the childhood event in transference. Only then can the causal link be established with any degree of conviction. For example, the fact that children who have been subjected to restriction of motility might frequently (even if statistically significant) develop certain types of adult personality constellations merely suggests a cause-and-effect relationship. During the course of psychoanalysis, however, the reappearance of the splinting experience as a transference manifestation linked with the particular personality traits is far more convincing evidence of a causal relationship.

Utilization of such data requires exploration of some of the differences between psychological investigation and most other research. The investigator of psychological data has a capacity that is not similarly utilized by investigators in most other fields. This capacity is introspection, through which one is able to know about psychological propositions from the inside. In a sense, the average man knows more about this aspect of psychology than the physicist knows about gases and the bacteriologist knows about bacteria. The capacity for empathy obviously has a potential for both sharpening and blurring observations. In multidisciplinary scientific circles, the latter potential for obscuring the accuracy of observations has been stressed. Typically, much effort is expended on isolating the observer from the phenomena under study because the degree of precision in science generally correlates with the distance between the observer and the phenomena. In consequence, psychoanalysts may feel apologetic about the techniques with which they are most expert. This results in the growing

phenomenon of the psychoanalyst employing research techniques alien to his greatest area of expertise as he neglects the rich data available in psychoanalytic sessions.

Participant observation is a demanding occupation and the helpful assistance of personal analysis does not guarantee objectivity. It is commonly recognized that a research or scholarly interest may influence the behavior of patients. It is unlikely that an analyst who develops an interest in the Isakower phenomenon should suddenly observe the phenomenon in several of his patients *only* because of his heightened awareness of this particular phenomenon. It is also probable that his interest has been communicated to the patient in some fashion, thereby influencing the patient's productions.

Such experiences underscore the value of team work in psychoanalytic investigation. We have passed the day when the psychoanalytic investigator need work *only* in isolation, using only the data that he himself has observed. Employment of data accumulated by others raises the important and complex issue of the storage and retrieval of psychoanalytic data. The considerable talents of the Hampstead Clinic have been devoted to this problem as evidenced by the formulation of the Developmental Profile (A. Freud, 1962) and Index (Sandler, 1962). Hopefully such devices not only offer the investigator a more extensive source of data than his own cases but also minimize (though they do not eliminate) the potential for the participant observing analyst and the patient observee entering into a form of unconscious collusion about confirming hypotheses.

Although the Developmental Profile and Psychoanalytic Index do not appear to have been designed with an eye to storing data for specific investigations, the predilections of their innovators and modifiers are naturally evident. In fact, the principal harvest derived from the Index thus far has been the result of refining these predilections in the form of meticulous definition and delineation of certain psychoanalytic concepts, a not inconsiderable contribution to furthering consistency in psychoanalytic theory. Sandler and his co-workers have considered such redefinition of certain concepts vital because of the lack of universal specificity in application of some psychoanalytic terminology.

The metaphorical language of psychoanalysis, which could have been produced only by a creative genius, runs the risk in everyday usage of degenerating to a readily misunderstood and imprecise jargon. Many examples could be cited, but one common one that deals with a basic issue will suffice as illustration. Cathexis, a highly abstract hypothetical concept, can easily become equated with actual behavioral manifestations of caring for or neglecting another person. Such usage neglects the possibility that a mother could cathect the mental representation of her baby at the same time that she actually neglects the baby because she is incapacitated in a hospital.

Akin to the fluidity of our metaphorical terminology is the disadvantage that accrues from the fact that the object of study is highly unstable. Not only is no individual ever the same on two separate occasions, but even the prevalence of certain pathological conditions seems to change over the course of time (see GAP Committee on Preventive Psychiatry, 1961). In the light of the frequency of the major hysterias of the nineteenth century, it is easy to understand Freud's (1894) equivalence of the concepts of defense and repression until 1926 (see Freud, 1926). Today, however, in our urbanized society, hysteria is rarely observed and defenses either have become increasingly complex or the ability to describe them has increased.

Coupling this instability of phenomena with the fact that psychoanalysts must work with concepts of overdetermination and multiple functioning makes it easy to understand why the classical experimental method is so difficult to apply meaningfully. Isolation of two variables invariably runs the risk of negating the basic premises that multiple determinants contribute to a phenomenon and that the phenomenon can serve a variety of functions.

Furthermore, the psychoanalyst not only deals with fantasy but takes it seriously. In contrast, most other scientists prefer to deny its existence or, if they do recognize it, attempt to eliminate it (although, on occasion, it is labeled serendipity). It is of interest, however, that great scientists who have made brilliant discoveries in the so-called hard sciences do at times credit their inspiration to a form of intuition that borders on fantasy. A recent prominent example was afforded by Glaser (1955), the physicist, who said

that he first got the inspiration for his Nobel prize winning bubble chamber while observing the behavior of the foam in the glass of beer that he was drinking in a student tavern. In psychoanalysis, however, one does not have to achieve greatness before finding the courage to describe fantasy, for both the patient's and the analyst's fantasy are everyday grist for the investigative and therapeutic mill.

### *Summary*

Referring to psychoanalysis proprietarily as "*our science*" represents at least two widely divergent attitudes about the scientific status of psychoanalysis: (1) a sober view that nothing tests the validity of psychoanalytic theory as convincingly as the psychoanalytic method itself, and (2) a regrettably rigid dogmatism about the validity of psychoanalytic theory that resembles religious fervor in that it is born of faith rather than observation.

Beginning with a discussion of the appropriateness of discussing psychoanalysis as a science and the inappropriateness of citing therapeutic results as evidence of its scientific status, it is still necessary to distinguish between the techniques and theories of psychoanalysis. There is a vital distinction between those aspects of psychoanalytic theory which are established fact, those which are hypotheses, and those which are classificatory schemata. It is important to establish hierarchies of theory according to validity, testability of hypotheses, and distance from the data of observation. Difficulties inherent in devising noncircular means of testing psychoanalytic propositions lead to exploration of efforts to minimize observer bias by employing multiple observers, utilizing external means of validating psychoanalytic reconstructions, the role of predictions in psychoanalysis, and the vital issue of the storage and retrieval of psychoanalytic data.

Although neutrality or indifference about psychoanalysis is rare, there is a widespread tendency to view the psychoanalyst as an omniscient, omnipotent, mind-reading magician despite the obvious fact that most laymen, in a sense, know more about this area of study as a consequence of introspection than the physicist knows about the "inside" of gases. Thus, everyone is, to an extent,

an expert in this field, bringing us perilously close to those areas such as politics and religion where opinions and beliefs hold sway almost exclusively.

This unavoidable position requires methodological scrutiny, which should be distinguished from a reaction formation in which only "hard" data that are statistically significant are considered valid. Yet, such data should not be ignored even though assessment of the true relevance to psychoanalysis of such data is a task of considerable magnitude. Such judgments need to consider more than the extent to which the conclusions from experimental studies fit clinical observations and are in accord with psychoanalytic theory. Recognizing that a deeper examination would be exceedingly difficult for individual analysts to accomplish, I suggest that committees of psychoanalysts assess the hypotheses, methodology, findings, and conclusions of experimental studies in an effort to clarify the relevance of the evidence for psychoanalytic theory. When required, these committees would rephrase the experiment's hypotheses and conclusions in terms that are meaningful to psychoanalysis.

A discussion of the scientific status of psychoanalysis would be incomplete without mentioning psychoanalytic training. Admittedly, much depends on the instructor's style, but if there were increasing emphasis on critical evaluation of data and method in the course of psychoanalytic training, then, even if psychoanalysis must remain "our science," its chances for growth and vitality will be maximized.

#### BIBLIOGRAPHY

- Bearden, J. A. & Thomsen, J. S. (1959), Résumé of atomic constants. *Amer. J. Physics*, 27:569-576.
- Bellak, L. (1961), Research in psychoanalysis. *Psychoanal. Quart.*, 30:519-548.
- Benjamin, J. B. (1959), Prediction and psychopathological theory. In: *Dynamic Psychopathology in Childhood*, ed. L. Jessner & E. Pavenstedt. New York: Grune & Stratton, pp. 6-77.
- Bonaparte, M. (1945), Notes on the analytic discovery of a primal scene. *The Psychoanalytic Study of the Child*, 1:119-125. New York: International Universities Press.
- Bond, D. D. (1965), Some perspectives in psychoanalysis. *Amer. J. Psychiat.*, 122:481-484.
- Escalona, S. K. (1952), Problems in psychoanalytic research. *Int. J. Psycho-Anal.*, 33:1-11.

- & Heider, G. M. (1959), *Prediction and Outcome*. New York: Basic Books.
- Ezriel, H. (1950), The scientific testing of psycho-analytic findings and theory. *Brit. J. Med. Psychol.*, 23:59-74.
- (1951), The scientific testing of psycho-analytic findings and theory. *Brit. J. Med. Psychol.*, 24:30-34.
- (1952), Notes on psycho-analytic group therapy: interpretation and research. *Psychiatry*, 15:119-126.
- Flumerfelt, J. M. (1962), On reconstruction. *Bull. Philadelphia Assn. Psychoanal.*, 12:53-68.
- Freud, A. (1962), Assessment of childhood disturbances. *The Psychoanalytic Study of the Child*, 27:149-158. New York: International Universities Press.
- Freud, S. (1894), The neuro-psychoses of defence. *Standard Edition*, 3:45-61. London: Hogarth Press, 1962.
- (1920), The psychogenesis of a case of homosexuality in a woman. *Standard Edition*, 18:147-172. London: Hogarth Press, 1955.
- (1926), Inhibitions, symptoms and anxiety. *Standard Edition*, 20:77-174. London: Hogarth Press, 1959.
- Frenkel-Brunswick, E. (1954), Meaning of psychoanalytic concepts and confirmation of psychoanalytic theories. *Sci. Mon.*, 79:293-300.
- GAP Committee on Preventive Psychiatry (1961), *Problems of Estimating Changes in Frequency of Mental Disorders*. GAP report #50.
- GAP Committee on Research (1966), *Psychiatric Research and the Assessment of Change*. GAP report #63.
- Glaser, D. A. (1955), The bubble chamber. *Sci. American*, 192(Feb.):46-50.
- Glover, E. (1952), Research methods in psychoanalysis. *Int. J. Psycho-Anal.*, 33:403-409.
- Guttman, S. (1965), Some aspects of scientific theory construction and psycho-analysis. *Int. J. Psycho-Anal.*, 46:129-136.
- Harrison, S. I. (1967), Individual psychotherapy. In: *Comprehensive Textbook of Psychiatry*, ed. A. M. Friedinan & H. I. Kaplan. Baltimore: Williams & Wilkins, pp. 1453-1463.
- & Carek, D. J. (1966), *A Guide to Psychotherapy*. Boston: Little, Brown.
- Hartmann, H. (1958), Comments on the scientific aspects of psychoanalysis. *The Psychoanalytic Study of the Child*, 13:127-146. New York: International Universities Press.
- & Kris, E. (1945), The genetic approach in psychoanalysis. *The Psychoanalytic Study of the Child*, 1:11-20. New York: International Universities Press.
- Hook, S., Ed. (1959), *Psychoanalysis, Scientific Method, and Philosophy*. New York: New York University Press.
- Kety, S. S. (1960), The true nature of a book: an allegory. *N.I.H. Record*, June 7.
- (1961), The heuristic aspect of psychiatry. *Amer. J. Psychiat.*, 118:385-387.
- Knapp, P. H. (1963), Short-term psychoanalytic and psychosomatic predictions. *This Journal*, 11:245-280.
- (1967), Discussion of an earlier draft of this paper.
- Kris, E. (1947), The nature of psychoanalytic propositions and their validation. In: *Freedom and Experience*, ed. S. Hook & M. R. Konvitz. Ithaca: Cornell University Press, pp. 239-259.
- Kris, M. (1957), The use of prediction in a longitudinal study. *The Psychoanalytic Study of the Child*, 12:175-189. New York: International Universities Press.
- Kubie, L. S. (1956), The use of psychoanalysis as a research tool. *Psychiat. Res. Rep.*, 6:112-136.
- (1960), Psychoanalysis and scientific method. *J. Nerv. Ment. Dis.*, 131:495-512.
- Lustman, S. L. (1963), Issues in psychoanalytic research. *The Psychoanalytic Study of the Child*, 18:51-74. New York: International Universities Press.

- Money, J., Hampson, J. G., & Hampson, J. L. (1956), An examination of some basic sexual concepts: the evidence of human hermaphroditism. *Bull. Johns Hopkins Hosp.*, 98:43-57.
- Mulligan, J. F. & McDonald D. F. (1957), Some recent determinations of the velocity of light II. *Amer. J. Physics*, 25:180-192.
- Rapaport, D. (1960), *The Structure of Psychoanalytic Theory: A Systematizing Attempt* [Psychological Issues, Monogr. 6]. New York: International Universities Press.
- Richfield, J. (1954), On the scientific status of psychoanalysis. *Sci. Mon.*, 79:306-309.
- Rosenthal, R. (1966), *Experimenter Effects in Behavioral Research*. New York: Appleton-Century-Crofts.
- Sandler, J. (1962), Research in psychoanalysis. *Int. J. Psycho-Anal.*, 43:278-291.
- Sears, R. R. & Wise, G. W. (1950), Relation of cup feeding in infancy to thumb sucking and the oral drive. *Amer. J. Orthopsychiat.*, 20:123-139.
- Serota, H. M. (1964), Home movies of early childhood: correlative development data in the psychoanalysis of adults. *Science*, 143(March 13):1195.
- Waelder, R. (1960), *Basic Theory of Psychoanalysis*. New York: International Universities Press.
- (1962), Psychoanalysis, scientific method, and philosophy. *This Journal*, 1:617-637.
- (1966), Personal communication.
- Wallerstein, R. S. (1964), The role of prediction in theory building in psychoanalysis. *This Journal*, 12:675-691.
- Wolman, B. B. (1964), Evidence in psychoanalytic research. *This Journal*, 12:717-733.

*Submitted June 5, 1968*  
*Children's Psychiatric Hospital*  
*University of Michigan Medical Center*  
*Ann Arbor, Mich. 48104*