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Toward the Resolution of Controversial Issues in Psychoanalytic Treatment

Curative Factors in Dynamic Psychotherapy

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Our aim in this chapter is to consider four controversies that are either explicit or implicit in the previous chapters and to outline research strategies that might be used to help resolve them.[1] The four issues are the relative therapeutic efficacy of a focus on: (1) problems of the "self' versus conflicts over libidinal and aggressive wishes, (2) Oedipal versus pre-Oedipal conflicts, (3) transference versus nontransference interpretations, and (4) the therapeutic atmosphere versus insight

These issues are encountered frequently, whether explicitly or implicitly, in current writings on the theory and technique of psychoanalysis and psychoanalytic psychotherapy. They are issues that, as we shall argue below, cannot be resolved adequately through further case studies based on psychoanalytic treatment as it is typically conducted.

I

Psychoanalysis probably has been more preoccupied than any other scientific discipline with its status as a science. Its self-consciousness in this

regard can be inferred from a couple of simple observations. First, a steady flow of articles through the years (e.g., Brenner, 1968; Joseph, 1975; Gaskill, 1979; Kaplan, 1979) has proclaimed that psychoanalysis *is* a science. And second, the "paper sessions" listed in the programs of psychoanalytic conventions and meetings of local psychoanalytic societies typically contain phrases such as "scientific papers" or "scientific sessions"—designations that undoubtedly would be unnecessary at a convention of physicists. We suggest that assertions of this kind simultaneously reflect the intense desire of psychoanalysts to obtain greater scientific status for psychoanalysis (in our opinion an admirable goal) and a kind of illusion based on wishful thinking that this status already has been achieved (hardly admirable).

We think it likely that these "demonstrations" by pronouncement have been resorted to because, as analysts, we have underlying doubts about "our science" (as Freud called it). Recent years have seen a number of cogent attacks on the seemingly scientific concepts of Freudian metapsychology (e.g., Gill, 1976; Holt, 1976; Klein, 1976; Schafer, 1976). These developments, in the context of the proliferation and increasing popularity of other schools of therapy (especially behavior therapy) over the past two decades, have made many analysts sensitive to the question whether psychoanalysis can properly be called "scientific."

We believe, however, that it *is* possible to adopt an effective scientific

approach to the validation of psychoanalytic hypotheses. We share the view of such writers as Klein (1976) and Gill (1976) that metapsychology is not the essence of psychoanalytic thinking. We can therefore grant credence to the criticisms of metapsychology, note that its assumptions are untestable, and concentrate on developing and testing the data-generated "clinical theory" (Klein, 1976) of psychoanalysis. And whatever the complex factors underlying the increasing popularity of other systems of therapy, there is no evidence that their approaches are more efficacious, particularly with regard to the kinds of emotional problems typically dealt with in psychoanalytic treatment.

In our judgment, where psychoanalysis can be legitimately faulted is in its failure to develop theory and practice in accord with existing scientific principles and procedures that it is possible to follow. The minimal requirements for any discipline that aspires to be a science include: (1) making the "raw data" of observation accessible to all interested observers; (2) stating clear and falsifiable hypotheses; and (3) establishing rigorous methods of testing these hypotheses as the means of resolving disagreement. Unfortunately, these standards have been ignored by most psychoanalysts.

The first two standards are easier to meet than the third, and in recent years a few psychoanalytic investigators actually have taken steps toward meeting them (see Gill et al., 1968; Wallerstein and Sampson, 1971; Sampson

et al., 1972; Rubinstein, 1975). The third requirement poses many thornier difficulties, so it is not surprising that it has received relatively little attention. What has made such avoidance possible is a deeply ingrained attitude among many analysts that, in conducting a psychoanalysis, one is concurrently carrying out research. Since Freud spoke of psychoanalysis as simultaneously a method for investigating the mind, a theory, and a treatment technique, it became easy for psychoanalysts to avoid the distinction between "search" and "research." Stated otherwise, they failed to make the distinction between the "context of discovery" and the "context of justification" (Reichenbach, 1938), and have maintained that in their clinical practice they are not simply generating hypotheses about patients but also testing these hypotheses.

While the main thrust of Freud's writings suggest that he believed that the clinical hypotheses of psychoanalytic theory could be tested within the analytic situation, on at least one occasion he acknowledged the limitations of the psychoanalytic method as a scientific procedure. In his introductory remarks to his discussion of Little Hans (1909) he wrote:

> It is true that during the analysis Hans had to be told many things that he could not say himself, that he had to be presented with thoughts which he had so far shown no signs of possessing, and that his attention had to be turned in the direction from which his father was expecting something to come. This detracts from the evidential value of the analysis; but the procedure is the same in every case. For a psychoanalysis is not an impartial scientific investigation, but a therapeutic measure. Its essence is not to prove anything, but merely to alter something. In a psychoanalysis the physician always gives his patient (sometimes to a greater and

> sometimes to a lesser extent) the conscious anticipatory ideas by the help of which he is put in a position to recognize and to grasp the unconscious material. For there are some patients who need more of such assistance and some who need less; but there are none who get through without some of it [p. 104].

Freud's comment can be viewed as casting doubt on the assumption that a patient's productions are a reliable means of judging the correctness of an analyst's interpretations and understanding.

Brenner (1976), in elaborating the opposite position, introduces distinctions between "conjecture," "interpretation," and "understanding." The term conjecture refers to "an analyst's formulation in his own mind about a patient's psychic conflicts," whereas interpretation is "what an analyst tells his patient about his psychic conflicts" (p. 3). Brenner reserves the term understanding for conjectures that are "strongly enough supported to seem quite certainly correct" (p. 3).

But how does an analyst know when he has moved from a conjecture to an understanding? Brenner states, "Psychoanalysts, like other scientists, must have some way of putting their conjectures to test" (p. 41), and he properly rejects the view that awareness of a conjecture is equivalent to proving it. But to what, then, can the analyst turn to validate or support a conjecture? Brenner suggests four types of evidence: (1) the patient repeats the same behavior on which the initial conjecture was based; (2) the patient confirms a

prediction the analyst makes based on an earlier conjecture; (3) the patient convincingly acknowledges the analyst's interpretation; (4) a heuristic reconstruction from a source outside the analysis confirms the interpretation —this last, according to Brenner, a relatively rare event. In addition to the four main criteria listed above, Brenner also refers to other indices of validity: the emergence of new analytic material (e.g., memories); expressions of surprise and other affective reactions; parapraxes; "confirmatory associations"; and "confirmatory actions."

If Brenner had presented these guidelines in the spirit of a *proposal*, i.e., suggesting criteria that could be used in the treatment situation to validate conjectures that psychoanalysts make about patients, we would view his paper as a significant step forward. For such a spirit would imply that one should develop operational definitions for the various criteria and propose ways in which the reliability and validity of analysts' judgments could be tested. However, Brenner seems to be saying that his criteria have already proved to be reliable and valid, and that a method is now available that allows psychoanalysts to operate scientifically in the clinical situation.

By taking such a view, Brenner seems to be glossing over the complex issue of validating analytic hypotheses, as is clear in his inclusion of "confirmatory" associations and actions in his criteria of validity. The very use of the word "confirmatory" begs the question. Even if we set aside for the

moment such thorny issues as suggestion and patient compliance, it is not at all clear how associations come to be regarded as confirmatory. For example, how does one decide whether to accept at face value the emergence of pre-Oedipal content following an interpretation of a defense, or whether to view such content as a further defensive reaction to underlying Oedipal issues? It is too easy to "find" supporting evidence for a conjecture, particularly if an analyst is invested in a particular hypothesis—a not uncommon occurrence (see Spence, 1976).

To consider a more topical example, would it really be a cut-and-dried matter, as Brenner's thesis implies, to decide whether a reported pattern of masturbatory behavior expresses a conflict over particular wishes or an effort to experience greater cohesion of the self? In short, one has to ask, "What is an observed fact during psychoanalytic treatment?" It is not the behaviors per se (and we include here the verbal productions of patients), but rather, the *meaning* the analyst assigns to the behavior, that validates a conjecture or interpretation.

Brenner's position that the treatment situation has proved itself as a vehicle for testing and validating psychoanalytic clinical propositions is contradicted by the following evidence. First, the many longstanding controversies among psychoanalytic clinicians strongly suggest that psychoanalytic clinical observers have great difficulty in agreeing on how the

productions of patients are to be "read." Second, in the few formal studies (e.g., Seitz, 1966) of this "consensus issue," the results have been most discouraging. Thus, in Seitz's study, when a group of analysts (trained at the same institute, which, if anything, should have increased the chances of their arriving at a consensus) were presented with the same material from a patient's analysis, the degree of reliability in their judgments of what unconscious conflicts were being expressed was disappointingly low.

The available data seem instead to support the view of Kubie (1952), who maintained that the data generated by typical psychoanalytic practice "give rise to controversies, but they are hardly the stuff out of which fundamental scientific advances can be fashioned" (p. 118). It is important to note that Kubie did not regard the validation of psychoanalytic propositions as impossible in principle, but only as impossible within the usual treatment situation. He thus advocated setting up a research institute for the study of psychoanalysis that could devote itself to correcting deficiencies in gathering and assessing clinical data (e.g., establishing a better data base than notes taken after sessions, and improving clinical follow-up studies) and devise other research methods to test clinical psychoanalytic propositions. The present chapter has been written in the spirit of Kubie's proposal.

II

What are the treatment issues that divide psychoanalytic clinicians? Many could be listed, but we will limit ourselves to the four stated at the outset, about which differences are particularly sharp and clear.

(1) In interpretation, what weight should be given to "self problems" in contrast to conflict over unconscious wishes? This question has been a major divisive issue among psychoanalytic clinicians since the publication of Kohut's first book (1971), and controversy has greatly intensified following the publication of his second book (1977). The substantive point in question is the following. Traditionally, psychoanalytic clinicians have viewed conflict over libidinal and aggressive wishes as the central problem in all nonpsychotic psychopathology. Kohut has challenged this view, at least for one (substantial) group of patients—those he refers to as "narcissistic personality disorders." For this group, according to Kohut, the pathogenic agent is not conflict over unconscious wishes, but rather, deficiencies in the sense of self (or what Kohut terms "self structures") resulting from early experiences of unempathic parenting. (Problems in the sense of self have been recognized before by psychoanalytic clinicians, but they have been viewed as the result of conflict over unconscious impulses.)

The response to Kohut in the psychoanalytic community has been very mixed. On the one hand, there are those (e.g., Stein, 1979) who reject his central thesis entirely and maintain that conflict over impulses is no less

central in the psychopathology of narcissistic personalities than it is in the psychology of other (nonpsychotic) persons. Others (e.g., Wallerstein, 1979; Stolorow and Lachmann, 1980) accept Kohut's formulation but view its applicability as limited. Here, the criticism of Kohut is that, whereas his thesis legitimately applies to one group of patients, he has overextended it and sees too many patients as suffering from a deficient sense of self. At the other end of the continuum are some followers of Kohut (Goldberg, 1978) who seem to have extended the applicability of Kohut's formulation to an even larger group of patients than Kohut has applied it to.[2]

(2) What weight should be given to Oedipal versus pre-Oedipal conflict in the analyst's interpretations? Putting aside the question whether, or to what degree, self problems should be viewed in Kohutian fashion, where it is agreed that interpretation of libidinal and aggressive wishes should be the focus of treatment, there is considerable divergence about whether Oedipal or pre-Oedipal conflicts are more deserving of attention. At one end of the continuum are clinicians like Fairbairn (1952) and Guntrip (1961), who view virtually all behavior from a pre-Oedipal perspective. Thus Fairbairn has written:

> I have departed from Freud in my evaluation of the oedipus situation as an explanatory concept. For Freud, the oedipus situation is, so to speak, an ultimate cause; but this is a view with which I no longer find it possible to agree ... I now consider that the role of ultimate cause, which Freud allotted to the oedipus situation, should properly be allotted to the

phenomenon of infantile dependence [p. 120].

Note that Fairbairn is not speaking here of particular patients or particular conditions but is completely rejecting the view that psychopathology can be rooted in Oedipal problems.

There are some clinicians who accept Fairbairn's characterization of infantile dependence (or other pre-Oedipal wishes) as the predominant pathogenic agent, with the amendment that Oedipal wishes act as an occasional agent. Others believe that there are substantial numbers of both Oedipal and pre-Oedipal patients, and still others claim that in many, if not most, patients, *both* Oedipal and pre-Oedipal conflicts are centrally involved. Finally, at the other end of the continuum are clinicians like Brenner (1974), who view Oedipal conflict as the crucial issue for the vast majority of patients.

Some of the Oedipal versus pre-Oedipal controversy is focused on particular types of patients. For example, among those who reject the Kohutian understanding of "narcissistic personality disorders" and view conflict over unconscious wishes as the pathogenic agent, there is a further split between those who implicate Oedipal conflict and those who view pre-Oedipal conflict as causative. Representing the former position is Wangh (1974), whereas Kernberg (1975) describes these same kinds of patients as struggling with pre-Oedipal "oral envy" and "oral rage." Similarly, while most analysts view most depressions as of pre-Oedipal origin, Brenner (1974)

writes: "in my experience, the clinical facts contradict the prevalent view that unconscious conflict associated with depressive affects in later life must be pre-Oedipal. For most [depressed] individuals it is the Oedipal phase that is crucial" (p. 30).

(3) What weight should be given to nontransference as opposed to transference interpretations? This issue has been explored in detail by Leites (1979). He notes that in recent years many analysts have been tending to limit themselves to "transference interpretations," a term that has come to refer "not so much to the genetic interpretation of the current transference attitude as … of an attitude toward the analyst which is at the moment active but unconscious or … preconscious" (Stone, 1967, p. 48), referred to by some as interpretation of the "here and now" transference. The position of the most extreme segment of this group (with Merton Gill [see Chapter 6] their most articulate spokesman) is well-captured by Leites in two sections of his book entitled, "Is All Transference?" and "Is Transference All?"

With regard to the first question, the group of analysts just referred to assume that virtually all patient productions are dominated by, if not exclusively the expression of, veiled references to the analyst. Their second assumption, which follows from the first, is that the *only* effective (i.e., mutative) interpretations in psychoanalytic treatment are (here and now) transference interpretations—i.e., those exposing the hidden meanings

behind the veiled references to the analyst. Other interpretations are, according to this school of thought, at best ineffective and at worst damaging to treatment. In the words of Gitelson (1962), "the analyst plays into … resistance by directing interpretations [to other things] rather than to the [here and now] transference" (p. 266).

On the other hand, Leites cites other clinicians who believe there is considerable value in nontransference interpretations (i.e., those referring to the patient's past or present life outside of treatment). Leites cites papers by Rosen (1955), Neiderland (1965), Heimann (1977), and Schafer (1977), in which nontransference interpretations appeared to elicit important material from patients. Whereas this latter (more inclusive) position probably characterizes the practice of most psychoanalytic clinicians, there is wide variation in the *degree* to which nontransference interpretations are made. For some they are clearly the exception, for others they are the rule, with all points in between represented by different segments of the "psychoanalytic community."

(4) What weight should be given to fostering a therapeutic atmosphere in psychoanalytic therapy in addition to offering interpretations? Let us spell out this issue in some detail.

There is no disagreement among psychoanalytic clinicians that the chief

role of the therapist is to offer interpretations and make whatever other interventions are necessary (e.g., clarifications, confrontations, and questioning) to pave the way for interpretations.[3] Moreover, there is no dissent from the view that in offering interpretations, the psychoanalytic clinician should be objective and nonjudgmental, an attitude that is part of the "interpretive stance." But there is disagreement about whether maintaining this interpretive stance is enough, or whether something more has to be done either to make interpretations more effective or to supplement them.

Again, differences among psychoanalytic clinicians can be viewed on a continuum. At one end are those who clearly believe that for all patients something more is needed—the "something" most often having been conceptualized as a "working alliance" (Greenson, 1967), a "therapeutic alliance" (Zetzel, 1956), or a "holding environment" (Winnicott, 1965; Modell, 1976). These conceptualizations are not identical but they share the view that something additional must be created in the therapeutic atmosphere if the analyst's interpretations are to have maximum effect and if patients are optimally to "work through" their conflicts.

Other psychoanalytic clinicians believe that special attentiveness to the therapeutic atmosphere is important only for certain types of patients. Fleming (1975), for example, notes that with patients who have experienced "early object deprivation" it is important to provide some symbiotic

gratification and, toward that end, "how useful wordless sounds of response from the analyst can be" (p. 754).

Nacht (1964) also suggests that the analyst help such deprived patients experience a degree of symbiotic satisfaction. He writes that "It seems necessary to me when this [symbiotic] need is too strong … that the [analysand] should be enabled to experience it at least fleetingly in analysis…. [If gratified] the patient will find ... a new peace and strength which will prove valuable for achieving normal relationships" (p. 301).

While changes achieved in this way might be viewed by some analysts as resulting from a "corrective emotional experience" rather than from the psychoanalytic process as it is usually conceived, Nacht makes clear his belief that the symbiotic experience can stimulate the analytic process as well.

> From [then] on, the explanations and the interpretations of the analyst will be accepted and experienced altogether differently … verbal interventions will ... be received in a different manner ... the words will form roots in [the patient's] deepest being and will bear fruit, whereas before they were virtually lost, almost as soon as they were heard [p. 302].

At the other end of the continuum are clinicians (Arlow, 1975; Kanzer, 1975; Brenner, 1979) who view any behavior by the analyst that goes beyond the adoption of an interpretive stance as not only unnecessary but as likely to interfere with the analytic work. Brenner, for example, after reviewing Zetzel's (1956) and Greenson's (1967) concepts of the therapeutic and

working alliances, concludes: "I am convinced by all the available evidence that the concepts of therapeutic and working alliance that have been current in the psychoanalytic literature since 1956 are neither valid nor useful" (1979, p. 149). On the basis of his reading of Zetzel's and Greenson's cases as well as Leo Stone's (1961) widely cited book, *The Psychoanalytic Situation*, Brenner believes that any departure from a strictly interpretive stance is likely to provide gratification to patients that will interfere with the analysis.

III

The issues we have outlined not only bear on various aspects of psychoanalytic theory but are crucially involved in determining the fate of psychoanalytic treatment. A psychoanalyst's position on the first two questions—self problems versus conflict over impulses, and Oedipal versus pre-Oedipal conflict—will obviously influence the kind of interpretations he or she makes. If we accept the psychoanalytic assumption that treatment outcome depends in large measure on the insights a patient develops into the specific psychodynamic and genetic roots of his pathology, and if we agree that such insights are based on analytic interpretations, the accuracy of these interpretations is obviously important.

Similarly, with regard to transference versus nontransference interpretations, Rangell (1978), Gill (Chapter 6), and Leites (1979) make it

clear that in their minds the degree to which each type of interpretation is made (a point on which they disagree) plays an important role in determining the effectiveness of treatment. And clinicians such as Zetzel (1956), Stone (1961), Greenson (1967), Arlow (1975), Kanzer (1975), and Brenner (1979) believe that one's conception of the proper atmosphere for psychoanalytic treatment (about which they disagree) plays an equally important role in outcome.

Since these issues are important ones for psychoanalytic clinicians, it is appropriate to ask what systematic investigations have been brought to bear on them. In a word, extremely few. With but a few significant exceptions (see Luborsky and Spence, 1978), psychoanalytic clinicians operate as if their theoretical and clinical differences will resolve themselves in time without any special effort beyond carrying out more analyses. The fact of the matter is, however, that three of the four issues under consideration (all but the first) have been dividing psychoanalytic clinicians for six decades.

It should not be surprising that the continued use of the conventional case study method has not brought these issues any closer to resolution than they were sixty years ago. For this method, as productive as it has been in generating meaningful hypotheses about the causes and treatment of psychopathology, does not allow for the controls necessary to test these hypotheses so that one psychoanalytic clinician can convince another of a

clinical proposition about which the latter is skeptical. (See Silverman [1975, 1978] for an elaboration of this point.) Gill (Chapter 6), in reflecting on why psychoanalytic findings have failed to "become solid and secure knowledge instead of being subject to erosion again and again by waves of fashion" attributes such failure to "the almost total absence of systematic and controlled research in the psychoanalytic situation."

IV

We will now suggest some research approaches that could yield reliable knowledge relevant to the controversial treatment issues outlined above.[4] In presenting these approaches we will outline them in a somewhat schematic, idealized fashion, neglecting for now the fine points of method and issues of feasibility.

We shall present five "research paradigms," ranging from most to least "naturalistic" on a continuum that reflects the degree of departure from the typical psychoanalytic treatment situation. The dilemma that investigators in this area must confront is that, the greater the methodological rigor of a study, the more the situation will depart from the typical treatment situation, making generalizations about typical treatment situations more hazardous. On the other hand, the closer the researched situation is to treatment as it is typically conducted, the fewer the controls that can be instituted and the

more tentative the inferences that can be drawn. This is one reason why data generated from different approaches are useful in providing converging lines of evidence.

Paradigm 1: Naturalistic Design with Interclinician Comparisons

In this paradigm, the data come from psychoanalytic treatment as it is ordinarily conducted, by groups of analysts representing two contrasting approaches. In terms of the issues that have been outlined, treatment results could be compared for clinicians as follows: (1) those who approach self problems in a Kohutian fashion versus those who do not; (2) those who focus on Oedipal issues versus those who emphasize pre-Oedipal issues; (3) those who largely limit themselves to transference interpretations versus those who do not; (4)those who make a special attempt to foster a therapeutic atmosphere versus those who do not.

For this paradigm to advance knowledge substantively, the following steps should be taken: (1) Each of the positions being compared should be represented by a sizable number of clinicians (twenty or more). (2) The clinicians representing the positions being compared should be equated for years of experience, sex, and whatever other variables are judged pertinent to treatment outcome. It would be desirable if in each group there were clinicians at different levels of experience and of both sexes. (3) In selecting

cases of the participating clinicians, an attempt should be made to match the groups being compared for relevant patient characteristics. At the very least, such matching should be done for degree of pathology, character type, and the presence of personality characteristics that are generally viewed as conducive to successful outcome in psychoanalytic treatment. It would also be desirable if, in each group, patients were represented at different levels of pathology, with different character types, and with varying resources available. (4) Evaluations should cover the fate of the presenting problem, the status of various ego functions (object relationships, adequacy of defenses, sublimatory capacity, etc.), and other important considerations such as the degree to which transferences—particularly the transference neurosis—have been resolved. (5) The evaluations should be carried out by independent clinicians who do not have knowledge of the characteristics of the psychoanalytic treatment that each patient received.

Paradigm 2: Naturalistic Design with Intraclinician Comparisons

This paradigm proceeds in the same way as the first except that, instead of comparisons being made between two groups of clinicians, they are made between pairs of cases from one group of clinicians, each clinician conducting treatment from the two vantage points being contrasted. This has an important research advantage over the first paradigm, but it poses a practical problem. The advantage is that it holds constant (or at least more constant)

many aspects of the clinician's behavior that could influence outcome, other than the treatment variable that is being evaluated. Put simply, it is much more likely that two cases will be handled in a similar way with regard to such extraneous variables if they are treated by the same clinician than if they are treated by two different ones. The practical problem is that the clinicians involved have to be both willing and able to conduct treatment from the two vantage points. For this to be feasible, the participants could either be neophyte clinicians, not set in their ways, or seasoned clinicians who are receptive to the two approaches being compared. In addition to the evaluation "instruments" needed in paradigm 1, this paradigm also would require the development of a questionnaire or a structured interview that could assess the potential clinicians' openness to the two approaches being contrasted so that the above criterion could be fulfilled.

Paradigm 3: Modified Naturalistic Design with Interclinician Comparisons, and Paradigm 4: Modified Naturalistic Design with Intraclinician Comparisons

In these paradigms things proceed in the same way as in the first two paradigms except that the psychoanalytic treatment sessions are taped. The taping is the "modification." Whereas some psychoanalytic clinicians have voiced discomfort at the idea of taping treatment sessions, a number of those who have done so (e.g., Gill et al., 1968; Dahl, 1972) have reported that neither the treatment process nor the outcome need be adversely affected.

Without taking sides on this issue (only systematic investigation will provide data that will allow a substantive resolution), our point is only that this paradigm has important research advantages. For one thing, characterizations of how clinicians conduct treatment would no longer be dependent on prospective and retrospective self-reports, but could be judged directly by noting the actual content and delivery of interventions. Thus, this information would serve as a way of verifying that the clinicians are actually representing their selected positions.

Such information is also relevant to the first two issues under consideration, which deal with the clinician's understanding of what underlies particular forms of psychopathology. Is a "narcissistic personality" struggling with a "self problem" in Kohut's sense, or with conflict over impulses? Is a depressive beset by primarily Oedipal or pre-Oedipal conflict? A question could be raised about whether the differences of opinion on these issues are due to the fact that clinicians are exposed to the same clinical material but view it differently; or whether by virtue of their particular personalities and interventions they elicit different kinds of material. For example, there may be a personality difference between clinicians who focus heavily on Oedipal problems and those who focus on pre-Oedipal problems. A reasonable hypothesis might be that the former come across as more authoritative, which in turn leads to their more often being experienced as the same-sex Oedipal parent in the transference. This transference experience

could lead in turn to the frequent activation of Oedipal conflicts in patients, with the result that they "produce" more Oedipal material than the patients of less authoritative clinicians.

All of the above, of course, presupposes that the psychoanalytic clinician can play a significant, if often unwitting, role in determining the kind of material that emerges in treatment. This supposition will no doubt be challenged by many, but it is precisely this issue that could be put to the test. Clinicians' interventions could be evaluated not only for the degree to which they represent a particular approach, but for the way in which they are conveyed.

Another important research advantage of recording sessions is that it allows for the objective observation of the immediate reactions of patients to particular kinds of interventions (see Gill et al., 1968; Sampson, Horowitz, and Weiss, 1972). Such observations would nicely complement the observations of the more distal effects that are observed in posttreatment and follow-up evaluations. Whereas these latter observations reflect on the important question of how a particular therapeutic approach influences the way a person emerges from treatment, it leaves uncertain just which aspects of the approach are having which effects. Viewing the patient's behavior immediately after a treatment intervention allows the observer to be much more certain of the intervention's specific short-term consequences. One

could address questions such as the one just alluded to, i.e., Is an intervention that is conveyed with an air of authority more likely to stimulate Oedipal rather than pre-Oedipal material?

Other questions one might address are: (1) Do transference interpretations elicit more intense emotional reactions than other kinds of interpretations? (2) Does a comforting tone of voice (as a concrete manifestation of a "holding environment") allow a patient to address anxiety-arousing material that he or she might otherwise avoid? (3) Under what conditions does focusing on conflict about impulses in a narcissistic personality stimulate nonproductive rage and a further narcissistic withdrawal? Obviously, one would have to look at a number of instances from the treatment of any one patient before arriving at a judgment of the effect of a particular intervention on that patient. Similarly, one would have to evaluate the reactions of many patients (in a diagnostic grouping) before one could generalize about the value of a particular therapeutic intervention for that type of patient.

Paradigm 5: Experimental

We use the word "experimental" in its strict sense here, referring to research in which there is an experimental manipulation designed to affect behavior in a particular way, the effect of which is compared with a "control"

manipulation, with all other variables held constant. This paradigm is viewed as alien by many psychoanalytic clinicians, yet it is as necessary in investigating clinical psychoanalytic issues as it is in medical research. The obvious advantage of the experimental method is that it provides controls that cannot be exercised in the clinical situation and thus can complement the clinical paradigms that have been outlined. (See Silverman, 1975, for elaboration).

Is there an experimental method available that can effectively address the controversial issues under discussion? We think that an affirmative answer can be given for at least some of these issues. The method has been termed "subliminal psychodynamic activation" and is described in Chapter 10 of this volume.

Over fifteen years ago, an interesting discovery was made that paved the way for the development of this method and for the study of psychodynamic processes in the laboratory. The discovery was built on earlier work on subliminal perception by Fisher (e.g., 1954) and others stimulated by Fisher's research (summarized in Wolitzky and Wachtel, 1973). In this earlier work, it was demonstrated that stimuli exposed tachistoscopically at a speed so great that nothing more than a flicker of light could be consciously perceived would nevertheless register in the brain and affect behavior. Thus, when subjects were asked to free-associate or "free-

image" (i.e., draw whatever comes to mind) immediately after such subliminal exposures, aspects or derivatives of the stimuli would often appear in their productions.

The new discovery (Silverman, 1967) was that if the stimulus has "psychodynamic content" (i.e., content related to unconscious wishes, anxiety, or fantasies), in addition to its content becoming retrievable, the person's level of psychopathology would be affected. That is, the subliminal input would silently stir up psychodynamic motives congruent with the particular stimulus, and symptoms rooted in these motives that the person was vulnerable to would emerge or become intensified. (See Silverman, Lachmann, and Milich [in press, Chapter 4] for a detailed account of this discovery.) This then made possible the systematic experimental study of the effects of psychodynamic processes on psychopathology. We have detailed the procedure that has been used in these experiments in Chapter 10, but we repeat it here to refresh the reader's memory.

Subjects are seen individually for an experimental session on one day and a control session on another, in counterbalanced order. The first session begins when the experimenter briefly explains to the subject the purpose of the study and seeks his or her cooperation. Then subjects are told about the tasks that will be administered to assess aspects of their behavior and are informed that several times during these tasks they will be asked to look

through the eyepiece of a machine (a tachistoscope) at flickers of light which contain extremely brief exposures of verbal and pictorial stimuli. Subjects are promised that at the end of the experiment they will be told the purpose and content of these stimuli.

The session proper begins with a "baseline" assessment of the subject's propensity for whatever pathological manifestations are being investigated. Then the subject is asked to look into the tachistoscope and to view and describe the flickers of light. There follow four exposures of either a psychodynamically relevant stimulus (the experimental session) or a (relatively) neutral stimulus (the control session). Each exposure lasts 4 msec. The specific pathology is then reassessed to determine the effect of whatever stimulus was exposed.

The procedure for the other session is identical to that just described except that a different stimulus is exposed between the baseline and reassessment task series. Subjects who are exposed to the psychodynamic stimulus in the first session are shown a neutral stimulus in the second, and vice versa. In each session the experimenter who works the tachistoscope and administers the assessment procedures is "blind" to which of the stimuli is being exposed. Since the subject is also unaware of the stimulus (as it is subliminal) the procedure qualifies as "double blind" in the same sense as in drug studies where neither the person administering the capsule nor the

person ingesting it knows whether the capsule is a drug or a placebo. The evaluation of pathological manifestations is also carried out blind.

In almost fifty studies that have been completed to date (summarized in Silverman, 1976, 1980), the psychodynamically relevant stimulus effected behavior changes not brought about by the neutral control stimulus. For example: (1) in twelve groups of schizophrenics (detailed in Silverman, 1971), indicators of ego disturbance (particularly thought disorder) significantly intensified after a stimulus with oral-aggressive content was exposed; (2) this same kind of stimulus content also was found to intensify dysphoric feelings of depressive persons (Miller, 1973; Rutstein and Goldberger, 1973; Varga, (1973); (3) in three groups of stutterers (Silverman et al., 1972; Silverman, Bronstein, and Mendelsohn, 1976) speech disturbance intensified after the subliminal presentation of anal content; and (4) in three groups of male homosexuals (Silverman et al., 1973; Silverman, Bronstein, and Mendelsohn, 1976) indices of homosexual interest intensified after the subliminal introduction of content suggesting incest. (It is of interest to note that, in a number of these studies, when the same stimuli were presented supraliminally—i.e., in the subject's awareness—the level of pathology was unaffected.)

Can this type of study yield data that have bearing on the treatment issues under consideration? We think that for the first two issues discussed

(self problems versus conflict over wishes, and Oedipal versus pre-Oedipal conflict), the answer is "yes"; for the third issue (transference versus nontransference interpretations), "probably not"; and for the last issue (the importance of the therapeutic atmosphere), "to some extent." Let us detail each of these answers.

Issues one and two relate to the psychodynamic content of interpretations. To what degree should interpretations address "self problems" in Kohut's sense and to what extent should they address conflict over Oedipal and pre-Oedipal impulses? This question can be recast as "What kind of psychodynamics underlie particular types of psychopathology?" The results from the studies just cited bear on this question, but for such studies properly to address these issues, a modification of the experimental design is called for. What is needed are experiments in which patients with a particular kind of pathology are given *three* experimental conditions: one in which a neutral Control stimulus is subliminally introduced, and two in which the stimuli have been designed to tap each of the two positions that are the subject of debate.

A series of experiments has already been carried out in which the experimental design approached the one just described, and which yielded data that have some bearing on one of the issues under discussion. In these experiments, four groups of subjects were seen: hospitalized male

schizophrenics, hospitalized female depressives, stutterers of both sexes from an outpatient speech clinic, and male homosexual nonpatient volunteers from the community. The question that was addressed was one of "specificity"—i.e., whether the identifying behavior of each of the groups (thought disorder in the schizophrenics, dysphoric reactions in the depressives, stuttering in the stutterers, and the homoerotic interests of the homosexuals) was tied only to conflicts about which psychoanalytic clinicians have written or to other types of unconscious conflict as well.

Each group received three (counterbalanced) conditions in which the following stimuli were exposed subliminally: (1) a "relevant" conflictual stimulus that had been implicated for the behavior at issue in the psychoanalytic clinical literature and which had intensified the relevant behavior in previous research using the subliminal psychodynamic activation method (i.e., an oral-aggressive stimulus for the schizophrenics and depressives, an incestuous stimulus for the homosexuals, and an anal stimulus for the stutterers); (2) a conflictual stimulus that was "irrelevant" for the group in question but had been shown to intensify the symptoms of one of the other groups (i.e., an incestuous stimulus for the schizophrenics and stutterers, an oral-aggressive stimulus for the homosexuals, and an anal stimulus for the depressives); and (3) a neutral control stimulus. The findings for these different groups were consistent (Silverman, Bronstein, and Mendelsohn, 1976). Although further support was obtained for the original

psychodynamic relationships studied, in no instance did the irrelevant conflictual condition influence the symptom under consideration.

We have cited these experiments not only because they exemplify the kind of design that we are suggesting for the first two issues under discussion, but because for three of the groups (all but the depressives), both an Oedipal and a pre-Oedipal stimulus were used. Interestingly, for two of the groups—the schizophrenics and the stutterers—the pre-Oedipal stimulus affected the behavior studied while the Oedipal stimulus did not. On the other hand, for the third group—the homosexuals—the reverse was the case, with only the Oedipal stimulus affecting behavior.

These findings suggest that pre-Oedipal and Oedipal conflicts are pathogenic for different kinds of pathology, a conclusion that contradicts the exclusionary view that only one or the other kind of conflict can play a pathogenic role. These results, however, amount to only a drop in the bucket. Before such findings could be viewed as substantially bearing on the point of controversy, several additional steps would have to be taken. First, whereas in each of the experiments cited the comparisons were between but one type of Oedipal and pre-Oedipal stimulus, several types would have to be compared.

Second, in addition to sampling the effects of different Oedipal and pre-

Oedipal stimuli, various groups of persons manifesting the psychopathology under investigation would have to be studied. For example, it is possible that in the experiments cited above, the homosexuals' responsiveness to the Oedipal stimulus and their nonresponsiveness to the pre-Oedipal stimulus, with the reverse pattern found for the stutterers, were a function of a sampling artifact. The homosexual sample happened to be composed of nonpatients whereas the stutterers were patients from a speech clinic. It is thus conceivable that those in the former group had available greater personality resources, which could account for their differential responsiveness to the Oedipal and pre-Oedipal stimuli. Only if varied groups of homosexuals and stutterers were studied could this possibility be ruled out.

Finally, more than one research laboratory should be involved in experimentation on each issue. This arrangement would not only provide more facilities to ensure that all the necessary experiments are carried out, but it would also enable the reliability of one laboratory's findings to be checked by another. The model would be that of laboratory experimentation in medicine, in which no single type of experiment and no single laboratory is viewed as providing enough data for drawing meaningful conclusions.

V

Could the subliminal psychodynamic activation method be of help in addressing the other two issues under consideration? We think this unlikely with regard to the issue of transference versus nontransference interpretations because, in contrast to the first two issues, this issue is entirely "treatment bound." That is, whereas we were able to translate the technique controversies over "self problems" and Oedipal versus pre-Oedipal interpretations into broader questions about the unconscious motivations for particular forms of psychopathology, no such translation seems possible with regard to the third issue. Whether or not patients in psychoanalytic treatment could benefit more from a singular focus on transference interpretation, or from a variety of interpretations of which transference interpretations are only one part, does not lend itself in any way that we can think of to a meaningful translation.

With regard to the fourth issue—the importance of creating a therapeutic atmosphere—there is a good possibility that the experimental method described above would yield relevant data. In fact, data are already available that we believe have some bearing on the issue.

Several years ago, the subliminal psychodynamic activation method was put to a new use. Whereas previously the stimuli were designed to stir up unconscious wishes and thus (temporarily) exacerbate psychopathology, interest now focused on providing a fantasied wish gratification that might be

expected temporarily to reduce pathology. As has been detailed in Chapter 10, the main stimulus chosen for this purpose was one intended to activate a fantasy of symbiotic gratification. It consisted of the verbal message MOMMY AND I ARE ONE, sometimes used alone and sometimes accompanied by a picture of a man and a woman merged at the shoulders like Siamese twins.

When the effects of this stimulus were compared with the effects of a control stimulus, such as MEN THINKING or PEOPLE ARE WALKING (accompanied by a congruent picture in those studies where a picture accompanied the symbiotic message), the following was found: (1) in ten studies carried out with "relatively differentiated" schizophrenics (summarized in Silverman, 1980), the symbiotic stimulus led to reduced thought disorder and otherwise more adaptive ego functioning; [5] (2) in twelve studies carried out with various types of nonschizophrenic groups (including “normal” college students, phobics, alcoholics, overweight persons, depressives, and character disorders), there also was increased adaptive behavior after the symbiotic condition.

In several of these studies, the subliminal stimulation accompanied a therapeutic intervention and was found to increase the effectiveness of the intervention. For example, in studies by Silverman, Frank, and Dachinger (1974) involving phobic women, by Martin (1975) involving overweight women, and by Palmatier (1980) involving cigarette smokers of both sexes,

various non-analytic treatment modalities were used to deal with the problem behavior (phobic symptoms, overeating, and cigarette smoking, respectively). In each study, subliminal stimulation accompanied the treatment intervention, with the participants randomly assigned to an experimental or a control group—the former receiving the symbiotic stimulus and the latter a neutral control stimulus. In each instance, there was significantly greater symptom reduction for the former group.[6]

What bearing do these findings have on the issue under consideration—the importance of establishing an optimal therapeutic atmosphere? It may be remembered that Fleming (1975) and Nacht (1964) have characterized this atmosphere (at least for certain kinds of patients) as one that provides a modicum of symbiotic gratification, which they maintain can improve adaptive functioning. (See also Marmor, Chapter 3, this volume; and Winnicott, 1965.) Thus, the research findings that have been cited on the pathology-reducing effects of the MOMMY AND I ARE ONE stimulus can be seen as supporting Fleming's and Nacht's position.

Those who disagree with this position might argue, however, that the therapeutic effects of activating symbiotic fantasies may well be at a price, especially when they are activated during psychoanalytic treatment. According to this argument, though such fantasies may produce symptomatic improvement, they can impede the analytic process and interfere with the

attainment of the more ambitious goal of "structural personality change." Although we can hardly discount such a possibility at this point, some studies already carried out have yielded data that are consistent with the contrary view (explicitly stated by Nacht) that symbiotic gratification can *further* the analytic process as well as reduce symptoms.

Specifically, in three such studies, the focus was on "treatment facilitating behavior" rather than on symptom reduction; in each instance, more of the facilitating behavior appeared after the symbiotic condition. The findings were as follows: (1) in one study (Silverman and C. Wolitzky, 1972), subliminal symbiotic stimulation, when contrasted with subliminal neutral stimulation, led to an increased willingness in the research participants to own up to wishes, feelings, and other personal motives; (2) in a study by Schurtman (1978), a group of alcoholics who were receiving subliminal symbiotic stimulation became more involved in their AA counseling sessions than did a control group receiving subliminal neutral stimulation; and (3) in a study by Linehan (1979), the same symbiotic condition, when compared with a control condition, was found to increase the degree to which college students were willing to disclose things about themselves in group counseling sessions.

If symbiotic gratifications could lead to the behaviors just described, they might well have a facilitating effect on psychoanalytic treatment. Of

course, many more data are called for—data from studies that attempt to replicate the findings just cited, and data from related experiments. For example, new studies similar to those described above should be carried out, but using research subjects with various kinds of personality structure. Then we could determine whether the adaptive behaviors that follow the activation of symbiotic fantasies characterize people generally, or only those with particular personality types. (It might turn out, for example, that for certain kinds of people, the activation of such fantasies leads to *less* acceptance of responsibility, involvement, and willingness to disclose things about oneself.)

It might also be possible to carry out an experimental study in which a method is first devised for tapping a person's potential for developing insight into his or her motivations; then the effects of subliminal symbiotic stimulation on this potential would be investigated—again, optimally for persons of different personality types.

Finally, one could study the behavioral effects of other fantasies that bear on the "therapeutic atmosphere." For example, a message such as MOMMY HOLDS ME CALMLY could be viewed as creating (in fantasy) the kind of holding environment that Winnicott (1965) and Modell (1976) view as facilitating psychoanalytic treatment.[7]

Of course, data from studies such as those just described would have

only indirect and circumstantial bearing on the "therapeutic atmosphere" issue in psychoanalytic treatment. When considered together with data from our clinical research paradigms, however, the experimental data could be of considerable value. In the next section, we will attempt to demonstrate the complementary roles that clinical and experimental paradigms can play in addressing a psychoanalytic treatment question.

VI

Let us suppose that the question to be researched is: What are the merits of each of the major psychoanalytic approaches to the treatment of narcissistic character disorders? If we delineate the major approaches as those of Kohut (1971, 1977), Kernberg (1975), and the classical school of thought (for example, as described by Rothstein, 1979), this question can be seen as touching on both the first and second treatment issues outlined earlier—i.e., self problems versus intrapsychic conflict, and Oedipal versus pre-Oedipal pathogenesis.

Before undertaking such a study, one would have to deal with a series of preliminary matters relating to definitional and measurement problems. First, the "representatives" of the three positions would have to agree on how to define the term "narcissistic character disorders" and on which specific behaviors to include in an operational definition. Second, the representatives

of the three positions would have to spell out in concrete detail the defining characteristics of their approaches. And third, a consensus would have to be reached about which behaviors would be targeted—that is, which changes in treatment sessions and at termination would be viewed as bearing on the merits of the different approaches.

Addressing these preliminary matters would be important for two reasons. The obvious one is that the research procedures would require that these matters first be addressed. (For example, if one implements the paradigm in which each therapist uses the three different approaches on a trio of matched patients, one must be able to specify to the therapist, in concrete detail, the defining characteristics of each approach.)

But equally important is the fact that addressing these preliminary matters would allow for a determination of the extent of *substantive* disagreement among the adherents of the three approaches. Thus, when the question of defining "narcissistic character disorders" is addressed, it should become clear how much of the disagreement among these adherents is based on the fact that their clinical experience has been with different kinds of patients. As Stolorow and Lachmann (1980) have suggested, it is possible that the proponents of the different approaches have been treating very different kinds of patients even though all have been given the same diagnostic label.

Similarly, with regard to the second preliminary matter—eliciting from the representatives of the different positions a concrete detailing of the defining characteristics of their respective approaches—we might find that the representatives do not differ as much in practice as one might suppose from reading their papers. (One could get an even better answer to this question by studying the transcripts of treatment sessions from a paradigm 3 study.)

And finally, in addressing the question of what changes one would look for as a result of treatment, it might turn out that the adherents of the three different approaches have very different things in mind when they assert or imply that their approaches have been successful with narcissistic patients.

VII

Assuming that a consideration of the results of these preliminary discussions leaves one convinced that substantive differences do exist among the proponents of the different positions (a likely possibility in our opinion), and assuming also that the proponents can achieve a working consensus on an operational definition of "narcissistic character disorder" and on the kind of patient changes that are to be viewed as germane to evaluating the three treatment approaches, we can now return to the question of the place that experimental data could play in addressing the research question.

In order for us to make our point here, we will consider two hypothetical sets of findings that might emerge from the use of the four clinical paradigms. First, let us suppose that these findings consistently indicate that one of the three treatment approaches is superior to the other two. More specifically, let us suppose that a study using paradigm 1 reveals that, in examining the pre-, post- and follow-up evaluation material of patients who have been treated by clinicians representing each of the three approaches, the narcissistic patients in one of the groups have shown a greater degree of positive change than those in the other two groups. Let us further suppose that when the same evaluation material was examined using the second paradigm we outlined, parallel findings emerged. That is, when the same clinicians treated matched patients with the three approaches, the treatment approach that was found to be superior in the first study emerged as superior in the second study as well. And finally, let us suppose that similar studies using paradigms 3 and 4 revealed that recorded sequences in sessions produced results consistent with those found using the previous paradigms.

Despite their consistency, such findings would still leave unclear which aspect of the "most effective" approach was responsible for its greater effectiveness. Since the treatment approaches of Kohut, Kernberg, and those who are more traditionally oriented are multidimensional, it would be hard to determine which aspect of each approach was responsible for its particular effects. For example, Kohut not only advocates addressing a particular kind of

psychic content—the underlying deficiencies in "self structures"—but maintains that this content should be addressed in particular ways with regard to the timing and ordering of interpretations, the relative weight given to transference and nontransference interpretations, and most important, the manner in which interpretations are given. Thus, if one wishes to know whether the superior approach succeeded because the relevant content area was dealt with, some other kinds of data would be needed.

The "other kinds of data," in our view, could be obtained from the subliminal psychodynamic activation method. More specifically, the following type of experiment could be undertaken.

Nonpatients who met the criteria for "narcissistic character disorders" could serve as research participants and could be evaluated for changes in the degree of narcissistic pathology that they manifested after being exposed to different subliminal conditions. Extrapolating from the writings on each of the three approaches, stimuli could be devised that would be expected either to intensify or to reduce narcissistic pathology if the particular approach is correct in its understanding of such pathology. For example, it would follow from the classical approach that a stimulus message that intensifies Oedipal conflict—e.g., DEFEATING DAD IS WRONG (for male subjects)—should exacerbate narcissistic pathology, whereas a message reducing such conflict —e.g., DEFEATING DAD IS OK[8]—should have the opposite effect.

Analogously, from Kernberg's theory, it might be expected that the message MOTFIER'S BREASTS ARE EMPTY would exacerbate the pathology, whereas the message MOTFIER'S BREASTS ARE FULL would have a diminishing effect. And from Kohut's theory, it might be predicted that the message I AM NOBODY would intensify narcissistic pathology, whereas the message I AM STRONG AND ABLE would have the opposite effect.

Following the research strategy described earlier, in later experiments other stimuli that tapped the same psychodynamics with different messages could be introduced. If the messages related to one approach consistently had a greater effect on the research participants than those related to the other approaches, and if the former approach was the same one that the clinical studies found to be superior, the following conclusion could be drawn. Since the experimental data would have been collected under tightly controlled conditions, one could reasonably infer that the superior experimental approach also produced the best therapeutic outcome because the content of its interpretations was most on the mark in addressing the psychodynamic issues at work in the type of patient studied.

Let us now consider the role that experimental findings could play in different hypothetical circumstances. Let us suppose this time that the clinical paradigms have produced discrepant results: that the findings from the first two paradigms (where treatment outcome is evaluated) indicate that one

approach is superior, whereas the results from the paradigms that evaluate changes within a session indicate no difference among the three approaches.

There would be at least two ways of understanding such discrepant results. It could be that the approach fared better only when posttreatment results were the point of focus because the working through of insights required time. Thus, when short-term changes were looked at, it erroneously seemed as if the interpretations had not had an effect.

But another possibility is that the absence of changes within sessions mirrored the fact that the interpretations were incorrect, and that the superiority of the approach in the outcome studies was due to the therapist's manner or other aspects of what we have referred to as the therapeutic atmosphere.

What could be helpful in deciding between these possibilities would be the results from the type of experiment described above. If the psychodynamic messages related to the approach that produced the best treatment results were found to have a greater effect on subjects than the messages related to the other two approaches, this finding would be a strong argument for the validity of the first explanation. But if one of the other sets of psychodynamic messages turned out to have as much or greater effect, the second explanation would be supported.

It is the complementary use of clinical and experimental data that we are stressing here. Experimental data by themselves could be justifiably viewed as too artificial and removed from clinical reality to be given heavy weight in their own right. Conversely, clinical data could be legitimately criticized as too poorly controlled to be taken seriously by themselves. But when both kinds of data are considered together, the weaknesses of each are compensated for by the strengths of the other, so that conclusions can be drawn with greater confidence.[9]

VIII

Concluding Comments

In his article "The Future of Psychoanalysis and Its Institutes," Holzman (1976), commenting on the use of clinical reports in the psychoanalytic literature, writes:

> It is noteworthy that our 80-year-old discipline never developed further canons for research or for judging the worth of contributions.... New ideas in psychoanalysis provoke some essays for and against, but these are not sufficient. Unlike ... literary criticism, we require more than such essays. We need proposals to test ideas systematically, and unfortunately there are too few calls for such tests [p. 269],

We are sounding one of these calls for the development of reliable empirical evidence relevant to the clinical theory of psychoanalysis,

particularly regarding controversial treatment issues such as the four that have been the focus of this paper. In so doing, we emphasize our agreement with George S. Klein (1976), who stated, "Among the sorriest clichés I have heard in psychoanalytic circles are the views that doing therapy *is* research and … that treatment *is* experimentation" (p. 64).

One of the major difficulties for clinicians who maintain the view that Klein criticizes is that when they disagree among themselves, the citation of clinical evidence rarely changes the minds of those on the other side. In this regard, one of Rapaport's (1960) conclusions in his systematic evaluation of psychoanalysis twenty years ago is equally valid today: "The extensive clinical evidence which would seem conclusive in terms of the system's internal consistency, fails to be conclusive in terms of the usual criteria of science, because there is no established canon for the interpretation of clinical observations" (p. 113).

Given the absence of clearly specifiable rules of evidence and the necessity of heavy reliance on clinical inference in attributing meanings, motives, and intentions to the patient's behavior and utterances, it is not surprising to observe the persistence of controversies such as those we have discussed. How clinicians understand pathology generally—as well as for particular patients—and how they conduct treatment can be too readily influenced by subjective factors, as the following report bears out.

For several years, one of us (D. L. W.) was engaged in a collaborative study with a group of colleagues from a psychoanalytic institute.[10] A major aim of the study was to explicate the underlying logic and implicit assumptions of clinical inferences made by trained analysts. The guiding hypothesis was that this task could be accomplished most readily when analysts agreed that a clinical segment constituted strong evidence for a given clinical hypothesis.

After considerable trial and error, the following procedure was adopted. Hypotheses about a patient were generated by reading aloud the typed transcripts of the first six sessions of a tape-recorded psychoanalysis. Any member of the group was free to interrupt to offer a hypothesis and state the observation on which it was based. In this way, about two dozen hypotheses were formulated. Then nine subsequent sessions were selected at random, with the restriction that they cover a sizable time span in the analysis. The transcripts of these sessions were read for evidence that would support or refute any of the initial hypotheses. When a member of the group came upon material that he regarded as having evidential value, the others paused and made independent ratings of the evidence item. Ratings were done on a scale of -4 (a judgment that the evidence went against the hypothesis) to +4 (when the evidence supported the hypothesis). The higher the rating, the stronger the evidence, pro or con.

A number of interesting findings emerged from this study, but we will focus only on the following one. The judge who first called attention to an evidence item consistently rated it higher than the other judges. That is, whereas at other times any given judge may have seen evidence as more or less positive than the other judges, when introducing a piece of evidence, he typically rated the evidence as more compelling.

This finding strongly suggests that clinicians overvalue evidence that they themselves find. A reasonable extrapolation to the clinical situation would be that there is a danger that therapists will have a vested interest in "confirming" their favorite hypotheses. Their threshold for "finding" supporting evidence will be lower, and conversely, their threshold for finding disconfirming evidence or evidence for another hypothesis will be higher. We therefore need additional methods of accumulating a body of responsible clinical knowledge. In this paper we have outlined some of the methods that might be used.

REFERENCES

Arlow, J. A. (1975), Discussion of Kanzer's paper. *Internat. J. Psychoanal. Psychother.*. 4:69-73.

Brenner, C. (1968), Psychoanalysis and science. *J. Amer. Psychoanal. Assn.*. 16:675-676.

____ (1974), Depression, anxiety and affect theory. *Internat. J. Psycho-Anal.*, 55:25-32.

____ (1976), *Psychoanalytic Technique and Psychic Conflict.* New York: International Universities

Press.

____ (1979), Working alliance, therapeutic alliance and transference. *J. Amer. Psychoanal. Assn*. (Suppl.), 27:137 157.

Dahl, H. (1972), A quantitative study of a psychoanalysis. In: *Psychoanal. & Contemp. Sci*., 1:237-257. New York: Macmillan.

Fairbairn, W. R. D. (1952), *Psychoanalytic Studies of the Personality*. London: Tavistock.

Fisher, C. (1954), Dreams and perceptions. *J. Amer. Psychoanal. Assn*.. 2:389-445.

Fleming, J. (1975), Some observations on object constancy in the psychoanalysis of adults. *J. Amer. Psychoanal. Assn*., 23:743-760.

Freud, S. (1909), Analysis of a phobia in a five-year-old boy. *Standard Edition*, 10:5-149. London: Hogarth Press, 1955.

Garfield, S. L., & Bergin, A. E. (1978), Handbook of Psychotherapy and Behavior Change. New York: Wiley.

Gaskill, H. S. (1979), Bridges to the future. *J. Amer. Psychoanal. Assn*., 27:3-26.

Gill, M. M. (1976), Metapsychology is not psychology. In: *Psychology versus Metapsychology* (*Psychol. Issues*, Monogr. 36). New York: International Universities Press.

____ , Simon, J., Fink, G., Endicott, N. A., & Paul, I. H. (1968), Studies in audio-recorded psychoanalysis. I. General considerations. *J. Amer. Psychoanal. Assn*., 16:230-244.

Gitelson, M. (1962), The curative factors in psychoanalysis. I. The first phase of psychoanalysis. *Internat. J. Psycho-Anal*., 43:194-205.

Goldberg, A. (1978), *The Psychology of the Self*. New York: International Universities Press.

Greenson, R. R. (1967), *The Technique and Practice of Psychoanalysis*. New York: International Universities Press.

Guntrip, H. (1961), *Personality Structure and Human Interaction*. New York: International Universities Press.

Heimann, P. (1977), Further observations on the analyst's cognitive process. *J. Amer. Psychoanal. Assn*., 25:313-333.

Holt, R. R. (1976), Drive or wish? A reconsideration of the psychoanalytic theory of motivation. In: *Psychology versus Metapsychology* (*Psychol. Issues*. Monogr. 36). New York: International Universities Press.

Holzzman, P. S. (1976), The future of psychoanalysis and its institutes. *Psychoanal. Quart*., 45:2

Joseph, E. D. (1975), Psychoanalysis—science and research: Twin studies as a paradigm. *J. Amer. Psychoanal. Assn*., 23:3-31.

Kanzer, M. (1975), The therapeutic and working alliances. *Internat. J. Psycho-Anal. Psychother*., 4:48-68.

Kaplan, A. H. (1979), From discovery to validation: A basic challenge to psychoanalysis. Presidential address to the Plenary Session, American Psychoanalytic Association, New York City, December.

Kernberg, O. F. (1975), *Borderline Conditions and Pathological Narcissism*. New York: Jason Aronson.

Klein, G. S. (1976), *Psychoanalytic Theory: An Exploration of Essentials*. New York: International Universities Press.

Kohut, H. (1971), *The Analysis of the Self*. New York: International Universities Press.

____ (1977), *The Restoration of the Self*. New York: International Universities Press.

Kubie, L. S. (1952), Problems and techniques of validation and progress. In: *Psychoanalysis as Science*, ed. E. Pumpian-Mindlin. Stanford, Ca.: Stanford University Press, pp. 46-124.

Leites, N. (1979), *Interpreting Transference*. New York: Norton.

Linehan, E. (1979), A study of the effects of subliminal symbiotic stimulation on self-disclosure during counseling. Unpublished doctoral dissertation, St. John's University.

Luborsky, L., & Spence, D. P. (1978), Quantitative research on psychoanalytic therapy. In: *Handbook of Psychotherapy and Behavior Change*, ed. S. L. Garfield & A. E. Bergin. New York: Wiley, pp. 331-368.

Martin, A. (1975), The effect of subliminal stimulation of symbiotic fantasies on weight loss in obese women receiving behavioral treatment. Unpublished doctoral dissertation, New York University.

Miller, J. (1973), The effects of aggressive stimulation upon adults who have experienced the death of a parent during childhood and adolescence. Unpublished doctoral dissertation, New York University.

Modell, A. (1976), The "holding environment" and the therapeutic action of psychoanalysis. *J. Amer. Psychoanal. Assn*., 24:285-307.

Nacht, S. (1964), Silence as an integrative factor. *Internat. J. Psycho-Anal*., 45:299-308.

Neiderland, W. G. (1965), The role of the ego in the recovery of early memories. *Psychoanal. Quart*., 34:564-571.

Palmatier, J. (1980), The effects of subliminal symbiotic stimulation in the behavioral treatment of smoking. Unpublished doctoral dissertation. University of Montana.

Rangell, L. (1978), Discussant on panel on Psychoanalytic Classics Revisited. American Psychoanalytic Association, New York City, December.

Rapaport, D. (1960), The Structure of Psychoanalytic Theory: A Systematizing Attempt (*Psychol Issues*, Monogr. 6). New York: International Universities Press.

Reichenbach, H. (1938), Experience and Prediction. Chicago: University of Chicago Press.

Rosen, V. H. (1955), The reconstruction of a traumatic childhood event in a case of derealization. *J. Amer. Psychoanal. Assn*., 3:209-221.

Rothstein, A. (1979), Oedipal conflicts in narcissistic personality disorders. *Internat. J. Psycho-Anal*, 60:189-200.

Rubinstein, B. R. (1975), On the clinical psychoanalytic theory and its role in the inference and confirmation of clinical hypotheses. *Psychoanal. & Contemp. Sci*., 4:3-58. New York: International Universities Press.

Rutstein, E. H., & Goldberger, L. (1973), The effects of aggressive stimulation on suicidal patients: An experimental study of the psychoanalytic theory of suicide. *Psychoanal. & Contemp. Sci*., 2:157 174. New York: Macmillan.

Sampson, H., Horowitz, L., & Weiss, J. (1972), Defense analysis and the emergence of warded-off mental contents: An empirical study. *Arch. Gen. Psychiat*., 26:524-532.

Schafer, R. (1976), A New Language for Psychoanalysis. New Haven, Ct.: Yale University Press.

____ (1977), The interpretation of transference and the conditions of loving. *J. Amer. Psychoanal. Assn*., 25:335-362.

Schurtman, R. (1978), The effect of psychodynamic activation of symbiotic gratification fantasies on involvement in a treatment program for alcoholics. Unpublished doctoral dissertation, New York University.

Seitz, F. D. (1966), The consensus problem in psychoanalytic research. In: *Methods of Research in Psychotherapy*, ed. L. A. Gottschalk & A. H. Auerbach. New York: Appleton-Century-Crofts.

Silverman, L. H. (1967), An experimental approach to the study of dynamic propositions in psychoanalysis: The relationship between the aggressive drive and ego regression —initial studies. *J. Amer. Psychoanal. Assn*., 15:376-403.

____ (1971), An experimental technique for the study of unconscious conflict. *Brit. J. Med. Psychol*., 44:17-25.

_____ (1975), On the role of laboratory experiments in the development of the clinical theory of psychoanalysis: Data on the subliminal activation of aggressive and merging wishes in schizophrenics. *Internat. Rev. Psycho-Anal*., 2:43-64.

_____ (1976), Psychoanalytic theory: "The reports of my death are greatly exaggerated." *Amer. Psychol*., 31:621-637.

_____ (1978), Reply to Theodore Shapiro's discussion of "Unconscious symbiotic fantasy: Ubiquitous therapeutic agent." *Internat. J. Psychoanal. Psychother*., 7:594-601.

_____ (1980), A comprehensive report of studies using the subliminal psychodynamic activation method. Research Center for Mental Health, New York University, New York City. (Unpublished.)

_____ , Bronstein, A., & Mendelsohn, E. (1976), The further use of the subliminal psychodynamic activation method for experimental study of the clinical theory of psychoanalysis: On the specificity of relationships between manifest psychopathology and unconscious conflict. *Psychotherapy: Theory, Res., & Prac*., 13:2-16.

_____ , Frank, S., & Dachinger, P. (1974), Psychoanalytic reinterpretation of the effectiveness of systematic desensitization: Experimental data bearing on the role of merging fantasies. *J. Abnorm. Psychol*., 83:313-318.

_____ , Klinger, H., Lustbader, L., Farrell, J., & Martin, A. (1972), The effect of subliminal drive stimulation on the speech of stutterers. *J. Nerv. Mem. Dis*., 155:14-21.

_____ , Kwawer, J. S., Wolitzky, C., & Coron, M. (1973), An experimental study of aspects of the psychoanalytic theory of male homosexuality. *J. Abnorm. Psychol*., 82:178-188.

_____ , Lachmann, F. M., & Milich, R. (in press), *The Search for Oneself*. New York: International Universities Press.

_____ , & Wolitzky, C. (1972), The effects of the subliminal stimulation of symbiotic fantasies of the defensiveness of "normal" subjects in telling TAT stories. Research Center for Mental Health, New York University, New York. (Unpublished.)

Spence, D. P. (1976), Clinical interpretation: Some comments on the nature of the evidence. *Psychoanal. & Contemp. Sci*., 5:367-388. New York: International Universities Press.

Stein, M. H. (1979), Review of The Restoration of the Self, by Heinz Kohut. *J. Amer. Psychoanal. Assn*., 23:665-680.

Stolorow, R. D., & Lachmann, F. M. (1980), *Psychoanalysis of Developmental Arrests*. New York: International Universities Press.

Stone, L. (1961), *The Psychoanalytic Situation*. New York: International Universities Press.

(1967), The psychoanalytic situation and transference: Postscript to an earlier communication. *J. Amer. Psychoanal. Assn*., 15:3-58.

Varga, M. (1973), An experimental study of aspects of the psychoanalytic theory of elation. Unpublished doctoral dissertation, New York University.

Wallerstein, R. S. (1979), Discussant on panel on The Bipolar Self. American Psychoanalytic Association, New York City, December.

____ & Sampson, H. (1971). Issues in research in the psychoanalytic process. *Internat. J. Psycho-Anal*., 52:11-50.

Wangh, M. (1974), Concluding remarks on technique and prognosis in the treatment of narcissism. *J. Amer. Psychoanal. Assn*., 22:307-309.

Winnicott, D. W. (1965), *The Maturational Processes and the Facilitating Environment*. New York: International Universities Press.

Wolitzky, D. L., & Wachtel, P. L. (1973), Perception and personality. In: *Handbook of General Psychology*, ed. B. Wolman. Englewood Cliffs, N.J.: Prentice-Hall.

Zetzel, E. R. (1956). Current concepts of transference. *Internat. J. Psycho-Anal*., 37:369-378.

Notes

1 Since dealing with all the issues raised in the preceding chapters would be an impossible task, we have selected those that, in our judgment, are most central to psychoanalytic treatment and are sufficient to illustrate the research strategies discussed later.

2 Kohut himself appears somewhat ambiguous concerning the explanatory scope of his self psychology. On the one hand, he advances the view that self psychology is better suited to explain certain phenomena (e.g., varieties of narcissistic disturbance) while traditional Freudian theory offers a better explanation of other phenomena (e.g., Oedipal conflicts). At the same time, he suggests that concepts from self psychology offer superior explanations even of phenomena (e.g., masturbation) explained by traditional Freudian theory.

3 See Levenson, Chapter 5, for an exception to this statement.

4 For a presentation of various research approaches to psychotherapy in general, rather than psychoanalysis in particular, see Garfield and Bergin (1978).

5 See Chapter 10, for a discussion of the relevance of the "differentiation level" on schizophrenics' responsiveness to the activation of symbiotic fantasies.

6 See Silverman (1980) for a listing of all studies—with both positive and negative results—that have used the subliminal psychodynamic activation method

7 Dr. Susan Farber, our colleague at New York University, suggested this experimental possibility.

8 For a discussion of studies that have used this particular message and their bearing on psychoanalytic treatment, see Chapter 10.

9 Our discussion of the use of the subliminal psychodynamic activation method to investigate controversial issues is not meant to imply that experimental research always yields results that are replicable and clear in their implications. Often the road to obtaining reliable data that have (relatively) unambiguous meaning is a rocky one. (See Silverman, Lachmann, and Milich, in press. Chapters 4 and 7, for some examples.) What can be said for experimental research is that when findings are inconsistent or unclear, there are accepted ways of resolving these ambiguities.

10 The group was led by Drs. H. Dahl and B. Rubinstein. The study is currently being prepared for

publication.