CHAPTER FIVE

CHALLENGES TO PSYCHOANALYTIC THEORY: RECENT DEVELOPMENTS

Psychoanalysis has always been challenged, or even held in a state of siege, by its commentators and co-workers in other psychological disciplines. Up to now, however, most of the challenges have missed the point. The charge that the theory was a kind of pan-sexualism bothered Freud, but not as much as it has later theorists; Freud sometimes courted this charge, as I have suggested earlier, while his descendants have successfully negated it. By the time of Freud's visit to the U.S. in 1909, he had emerged from a period of intense theoretical and clinical work during which he had still not credited the reality of sexual instinct in the infant and child (Sulloway, 1979, pp. 111-112, 210-213); the fact that he had felt forced to change his mind on this, and to expand his definition of the sexual body to include the whole of infantile and adult life must have made Freud leary of any temptation to play down the role of the sexual. But of course he did not mean to endorse the later Reichian attitude in which healthy sexuality and health itself were closely correlated. As Sulloway has shown, Freud in fact was gathering heavy opposition in professional circles in Europe at about the time of his visit to the U.S. in 1909, more in fact than in the earlier years when he published his *Three Essays on the Theory of Sexuality* (Freud, 1905b) or his *Interpretation of Dreams* (1900). An awareness was growing that Freud was not merely endorsing the importance of the sexual body. He was showing its connections with too many other areas. There were others in Europe who courageously emphasized the need for attention to sex, but none who both dared to write about it in plain language

(instead of Latin euphemisms, such as Kraft-Ebbing used), and who at the same time connected sexuality with neurotic symptom formation (Sulloway, 1979, pp. 205, 457).

The other standard accusation, in addition to that of pan-sexualism, and one which is a veritable stock in trade for objectors to psychoanalysis, concerns a lack of empirical evidence. But this has failed continually, partly because there appears to be considerable empirical support for pschoanalytic theory (Kline, 1981), but also because such criticism has consistently ignored the specifications of the theory and underestimated its complexity (Masling and Schwartz, 1979). To demand, as did Sartre, that there be some direct empirical confirmation of such complicated constructs as sublimation, for example, is a tactic that descends into anti-intellectualism (Efron, 1973). Criticisms of psychoanalytic theory based on contrary assumptions about human nature are not directly refutational of the clinical reports in great abundance which "confirm" again and again that the endopsychic forces postulated by the theory are very much present in the human psyche. Like other theories making use of clinical evidence, confirmation can be found, but no one knows if the evidence is an artifact of the clinical situation. In an atmosphere of hostility and rejection within which psychoanalytic therapy has had to exist, it is only to be expected that the clinical "proof" would be honored and even made sacrosanct by practitioners. Moreover, the origin of criticism among a populace of researchers and other commentators who had not themselves undergone psychoanalysis presented a problem of disparate worldviews. A few years ago, the psychoanalytic theorist Norman N. Holland, then a colleague of mine, declined to consider an article I showed him, which contained a sharp criticism of Freud's famous case-history of a child's neurosis, the case of little Hans (Freud, 1909). Holland remarked that it is perfectly feasible but quite inconclusive to tear the old cases apart with nonanalytic logic; valid criticism could only come from within the field.

Such defense is almost foolproof, and it was reinforced until recently with the firm conviction that the early Freud had indeed set the stage for the later theory through his discovery that nearly all the patients who had led him to believe they had been sexually molested or "seduced" as children, by parents or other older relatives, had actually undergone these seductions only in their own, endopsychically produced fantasy. Ultimately such fantasy is a part of the infantile psyche, and is not susceptible to laboratory experiments; it can only be inferred retroactively through privileged evidence that emerges during adult and child therapy itself.

One theorist, Heinz Kohut, has formally raised the analytic experience itself to the status of a special empirical ground from which psychoanalysis gets its evidence (Kohut, 1977). Kohut was far from intending, by this move, to evade unsettling evidence; on the contrary, he wanted to open the way fully to any data, however distressing these might be to practitioners, but no piece of

empirical data can be permitted an automatic veto over evidence that emerges in the only area or "laboratory" that is central to the discipline of psychoanalysis: the analytic process itself. By defining the essence of psychoanalysis to be the prolonged empathetic introspective immersion in the inner life of the human being, as this immersion occurs during an analytic situation and with that situation as the prime source of psychoanalytic evidence, Kohut hoped that he had cleared the way for the reconsideration and if need be the rejection of any specific psychoanalytic notion, no matter how sacred. Thus, while he admitted that he could not imagine getting along without such basic concepts as transference and resistance, he could in theory see how these concepts might some day be superseded, and psychoanalysis still retain its essence. Kohut would have liked his open definition of psychoanalysis to "do away with the *ex cathedra* rejection of findings and thoughts which are at variance with established doctrine . . ." (Kohut, 1977, pp. 308-309). It was Kohut's hope that psychoanalysts would accept his innovation and thus become able to give themselves over to the task of recognizing newly described configurations and processes.

The openness to evidence displayed by Kohut is of dubious merit, however, if it should serve as a further reason for the refusal to consider other *kinds* of evidence, such as the findings of infant research. Through most of psychoanalytic theorizing until recently this problem seemed not to be critical. Not only was it believed that no one would be able to prove what an infant was fantasizing, feeling, dreaming, or thinking; the psychoanalytic community could also point to a certain amount of empirically grounded theory on the infantile psyche, notably that by René Spitz and especially by Margaret S. Mahler, which supported the major theory that had emerged after 1920. This research claimed to show that the infant was poorly equipped during the first months of life, in almost all dimensions—mental, physiological, perceptual and emotional. Spitz found in his psychoanalytic study of the infant that the newborn has no faculty of perception, representation or volition (Spitz, 1965). This amounts to Melanie Klein's assumption again, that the infant's "earliest reality is wholly phantastic" (Klein, 1948b, p. 238), though without supposing the fantasies are destructive. Such an infant could not be expected to differentiate, at first, between its own body and the body of its mother, much less between two different selves. This denial of the capacity for differentiation in the neonate has become axiom, supported by the highly respected controlled observations of Margaret S. Mahler (Mahler, Pine, and Bergman, 1975, p. 44). Mahler tells us that during the second to fifth months, "The infant behaves and functions as though he and his mother were an omnipotent system—a dual unity within one common boundary." This period of "symbiosis" is described in a way that fits well with my characterization of much psychoanalytic theory of the infant as an expression of hatred toward the infant: "The essential feature of symbiosis is hallucinatory or

delusional, somatopsychic omnipotent fusion with the representation of the mother" (Mahler, 1968, p. 9, quoted by Milton Klein, 1981b, p. 91). The earlier developmental period, prior to the "symbiotic," is an even more dramatic expression in this light: it is called the "autistic" phase, which is best understood "in physiological terms" (Mahler, Pine, and Bergman, 1975, p. 41, quoted by Klein, 1981b, p. 76). In other words—and it will not do to dodge this conclusion—the neonate is reduced, for all intents and purposes, to a kind of amorphous mass. Eventually, however, the infant learns differentiation, but it does this only by means of a painful process of psychological separation which damages the infant's delusory feeling of omnipotence. Development is premised on the infant's creation of dual images of the mother as part-object: a good mother who obeys the infant's needs totally, and a bad one who hatefully and inexplicably denies these needs. At this juncture, Mahler's theory of infant development and Melanie Klein's were closely joined, with Klein emphasizing the "splitting" of good and bad object-representations and Mahler the "autistic" and "symbiotic" phases.

Criticism of Theory: Three Issues that Cannot Be Dodged

Recent developments have disturbed the complacency, and aroused the defensiveness of psychoanalytic epistemology. These developments have come from several sides. One is the scholarship on Freud, where, for example, Frank Sulloway (1979) has shown decisively that Freud was seriously committed to giving his theory a biological base. Freud, contrary to legend that had grown up around him, never gave this up, even in his old age. He did not consider biology a dimension of theory that could be dispensed with. Sulloway chose to portray Freud as a "biologist of the mind," an act of naming which lost touch with the multidisciplinary force of his "sexuality"—but this simplification cannot erase the sound research in Sulloway's work. Sulloway's proof (and the term may be used here with assurance) that Freud never left biology has the effect of putting the body back into the psychoanalytic world view.

Secondly, Freud's so-called "seduction" theory has been resurrected, partly through increased recognition of the widespread problem of child abuse, and partly through the very recent publication of the complete text of Freud's own letters at the time that he rejected the seduction theory in favor of his own conclusion that the root cause was fantasy, not bodily interaction in the family. This publication came after several researchers had become increasingly dissatisfied with the decision of the Freud Archives to refuse publication of the material (Efron, 1977, p. 254; Sulloway, 1979, p. 188). The psychoanalyst who edited the material for the Archives, Dr. Jeffrey Moussiaeff Masson, was dismissed from his post because of his outspoken insistence that the revelations are sufficient for a reversal to be made in psychoanalytic assump-

tions. If actual sexual abuse and not fantasies unaided by such abuse were the key factor, analysts, said Masson to the press, "would have to recall every patient since 1901. It would be like the Pinto" (quoted in Blumenthal, 1981a; see also Blumenthal, 1981b and 1984). With this flamboyant declaration, Masson not only got himself dismissed; he also signalled his own tendency to simplify the issues. As he presented it, Freud's decision to de-emphasize the seduction theory became an issue in moral action, in "honesty" and "courage," rather than a matter of science and psychology. To be sure, Masson occasionally qualified his attack on the image of Freud by saying he did not think Freud "ever made a conscious decision to ignore his earlier experiences . . ." (Masson, 1984a, p. 189); but Masson's unexplained anger at Freud regularly overwhelms his writing about the issue. The issue is reduced to one of personalities—Freud's versus Masson's. With Janet Malcolm's somewhat gossipy character portrait of Masson (Malcolm, 1983, 1984)—presented largely in Masson's own words during an extended interview or series of interviews—the stage was set for his discrediting. There has been speculation in fact that Malcolm chose deliberately to concentrate on the personal, journalistic aspects of the controversy; according to Milton Klein (Klein, 1984) who was present (along with others including Masson) at Malcolm's home during some of the discussions leading to Malcolm's book, important theoretical issues were brought up only to be given very little play in the book as it emerged. Instead a portrait of Masson dominated Malcolm's text; he is portrayed, according to Robert Coles, as "a grandiose egotist—mean-spirited, self-serving, full of braggadocio, impossibly arrogant, and, in the end, a self-destructive fool" (Coles, from a review in the *Boston Globe* of May 27, 1984, as quoted by Masson, 1984c, p. 46). Walter Kendrick, reviewing Masson's book, advised readers not to trouble to read Masson at all: anything of value that his book might contain could be found in more readable and reliable form in Malcolm's debunking version of Masson's research (Kendrick, 1984, p. 12). In addition to the obvious problem of urging that interested readers in the disciplines rely on a secondary source and ignore the original, this advice shows an excessive willingness to credit Malcolm's interviewing as an objective account of Masson's sexual body; one of the most florid details she offered was Masson's alleged statement that he had had sex with some 1000 women (Malcolm, 1983, p. 101). Masson has since denied the accuracy of Malcolm's interview reports, and another journal has offered to give an independent, impartial report on the contents of Malcolm's 50 hours of cassette tape interview material with Masson (English, 1984; Masson, 1984c). But the object of diverting discussion of the issues to Masson's personality seems to have been temporarily accomplished.

Masson's book has coincided with a recent public interest in "child abuse," which is often accompanied by a reversion to a pre-Freudian concept of the

infant and child as "innocent"—innocent, that is, of having a sexual body at all. There are already glimmerings of a general anti-sexual streak in the concern about the problem of child-abuse. Katz, a psychologist working in this area, reports receiving complaints from frightened people about virtually any sign of adult-child affectionate touching; he warns that excessive concentration on sexual abuse not only serves to take attention from other forms of abuse such as emotional bullying, but that it will interfere seriously with the newly emergent child-rearing patterns in which affectionate touch is an accepted and valued practice (Katz, 1984). The Reichian theory of orgastic gratification at the adult level provides a distinction between touch that is an expression of affection by an adult toward a child, and touch which expresses adult impotence and is thus potentially a form of foreplay with sexual intent. But this is a difficult concept to get across to a public not generally well versed in the differences between touching with affection and touching with sexual aim.

Nonetheless, intelligent discussion of child-abuse is increasing. The recent flyer I received from the National Committee for the Prevention of Child Abuse features emotional abuse where touch is not involved at all as one of the important forms of child abuse, in addition to sexual abuse (Cohn, 1984). Research into "emotional deprivation" as a form of child abuse is well under way (Brody, 1983d). And in psychoanalytic circles, the seduction theory controversy does not seem to be dying out. Nor, as Milton Klein has explained (Klein, 1984), is the seduction theory a simple one involving purely external (nonpsychological) assaults on the child. In fact, the theory emerged within the context of Freud's development of his theory of repression, and it hinged on a subtle hypothesis of the igniting of repressed memories of early childhood seduction during a later phase of development (Klein, 1984). The theory is critically explosive, however, in the way that Freud's investigations of the sexual body tend to be, because it connects traditional family practices in child-rearing with sexual practices which are to all intents violations of the sexual body of the child, as well as being expressions (in Reichian terms) of the orgastic impotence of adults whose own sexual bodies are in a pregenital, that is, psychobiologically infantile, stage of development. And in some of the cases, it would appear that these adults are the parents themselves. The possibility of parental involvement had already been raised by a number of reseachers prior to Masson's highly-publicized comments, although earlier inquiry had to proceed without benefit of the unpublished materials to which he had access. Milton Klein and David Tribich, both of whom are psychoanalytic therapists, have published sharply critical articles on Freud's virtual blindness to the damage caused by parents to children, which is the overall issue of the seduction theory as they see it (Klein and Tribich, 1979; Klein and Tribich, 1982). Klein and Tribich in fact have reasons for seriously questioning all five of Freud's major case histories on precisely the grounds that he was

blind to the ways in which these patients had been damaged by parental and other adult hostility, always in relation to their sexual bodies. Unlike Masson, who has tended to make flamboyant statements which play into the hands of those who would like to forget about the seduction theory and its problems, Klein and Tribich have presented a formidable critique which will be difficult to refute in detail. An additional powerful critique of Freud's dismissal of the seduction theory has been launched in three books by the Swiss-trained psychoanalyst, Alice Miller (Miller, 1981a, 1981b, 1983). Miller has developed her argument beyond the level of merely criticizing Freud; she connects the seduction theory issue with larger problems of child-rearing in society, and takes a position regarding the value of somatosensory affection that is similar to that taken by Prescott (Prescott, 1979; see Loewenstein's review of Miller: Loewenstein, 1984b, p. 325). Like Klein and Tribich, Miller reinterprets all of Freud's case histories to show how he mistakenly de-emphasized the factor of adult hostility and aggression toward the child, often in matters of sexuality (Miller, 1981b).

A very late addition to the research on one of these five case histories tends to add weight to the criticisms now being raised. Freud's famous patient, the so-called Wolf-Man, came to the conclusion that Freud had been quite wrong in finding a primal scene fantasy through interpretation of a dream. The Wolf-Man did not deny that Freud had been insightful, but in this crucial point, the very one which has been used for 70 years to support the doctrine that images of sexual violation are endopsychic productions of children's thinking about sex, regardless of life-situation or environment, the Wolf-Man has to firmly disagree with the analyst (Obholzer, 1982). The Wolf-Man points out that unlike other Freudian dream interpretation, the key elements in Freud's derivation of the primal scene from his dream remain pure interpretation, related in no discernable way to the dream material (Obholzer, 1982, pp. 35-36). The parts of the dream which should eventually have made emotional sense to the patient never came to do so, even after 60 years. As Loewenstein (1984a) has argued, Obholzer's evidence of Freud's longterm ineffectiveness with the Wolf-Man must be taken as a serious indication of an over-emphasis on the highly speculative primal scene material at the expense of much stronger evidence that the Wolf-Man had been "sexually and emotionally exploited as a child" (Loewenstein, 1984a, p. 9).

The sociologist and legal expert Sheleff (1981, pp. 70-87) has also provided a devastating critique of Freud's handling of the "seduction" and child abuse issues. David Finkelhor (1979), in one of the most widely respected recent empirical studies of sexually victimized children, has found evidence to show that the Freudian theory is wrong, not only with regard to the rate of occurrences of such victimization, but in its concept of the resultant trauma. Freud was led to believe that the trauma comes from the child's guilt feelings, because the sexual contact with the adult would have amounted to an acting-

out of prohibited incest wishes on the part of the child. The child would have been in complicity with its own sexual victimization. Finkelhor found instead that the short-term, coerced violations of the child were far more productive of trauma than the long-term ones involving consent and complicity (Finkelhor, 1979, pp. 104-115). Finkelhor's conclusions are supported by Constantine's comprehensive review-analysis of some 30 studies of the impact of childhood incest and sexual encounters with adults, comprising some 2500 subjects (Constantine, 1980).

Once again, however, there is the problem of how the new insights into child-adult sexual contact, and the issues they raise, can be integrated into a theory that seems to have no place for certain propositions. By now, the position that psychopathology is primarily a matter of endopsychic processes has become entrenched in psychoanalytic theory. Even where it is modified by an emphasis on the need for good mothering—or the "good enough" mother as in Winnicott—the cognitively helpless infant lacking a capacity to differentiate itself from the mother, and overwhelmed with imperatives of its own emotional and physical constitution, leaves little expectation that it will become a credible witness of its own child abuse. Yet it would seem that the impact must be felt; perhaps the change in theory will first go on underneath the denial that it is being made. That is, the supposition that the patient has fantasized his or her account of being sexually abused as a child will now become the *last* rather than the *first* assumption to operate within the therapist's mind. I suspect that some shift in the direction of giving credence to the patient's reports already is taking place in therapy, despite Bruno Bettleheim's dismissive prediction that the dispute over the seduction theory "won't change the way anyone does anything" (quoted in Goleman, 1984a).

The question of how an infant or young child could have the cognitive competence to carry such a memory of early sexual abuse, and have that memory be an accurate narrative of something that actually occurred, may no longer be pushed out of sight. It is here that a third and probably most serious challenge to psychoanalytic epistemology has been brought to bear, because there is now an immense amount of research on the neonate, the implications of which are a reversal of psychoanalytic theory of the infant in early development. The "explosion" of knowledge in infant studies over the past two decades has produced a complicated series of findings and new questions, but there is little doubt that these findings call into very serious question what psychoanalytic theorists have taken to be true. Infant "competence" is now a well-supported notion, and it goes ill with psychoanalytic assumptions. The fact that infants under two months old can "habituate to a novelty stimulus" has only become clear since 1972 (Haith, 1980, p. 7); it implies that doubts about a memory capacity of infants under two months, based on a belief that they could *not* so habituate, lose most of their force with this finding. Similarly, it becomes increasingly implausible to suppose (as psychoanalytic

theory now does) that a young infant cannot discriminate the boundary between its own body and that of its mother when new research shows, for example, that the newborn infant has three-dimensional vision and displays some hand coordination with this vision, apparently for purposes of orientation (von Hofsten, 1982).

In psychoanalysis, the impact of neonate findings has been felt especially with regard to the widely accepted psychoanalytic theories of Margaret Mahler. Both Emmanual Peterfreund (1978) and Milton Klein (1981b) have argued in detail, and within the pages of psychoanalytic journals, that Mahler's findings on the infantile psyche are thoroughly, radically wrong, in the light of new research and with a re-examination of Mahler's own methodology. Klein refutes her work on its merits in light of the evidence now available, and also finds that she has been a great deal less than responsive to this evidence than a responsible investigator might have been (Klein, 1981b, pp. 82-83; see also Bowlby, 1984, p. 43). Inasmuch as Mahler's theory offers the most prominent and the most detailed synthesis of research on infancy that psychoanalysts have been able to point to, this is a serious confrontation. Peterfreund and Klein are both active within the field of psychoanalytic training, a fact that will make them difficult to ignore.

The threat posed by new evidence is in fact greater than even Peterfreund and Milton Klein indicate; so fast is the field of neonate research developing that they did not take into account one of the most directly challenging developments to date, namely the discovery of very early capacities on the parts of infants to imitate expressions of adults. These have been proposed mainly by Meltzoff and co-workers, in several presentations from 1977 and continuing. Although most accounts in the press concerning Meltzoff's work have concentrated solely on his finding of facial imitation (e.g., "Newborns Found," 1982), it should be noted that one of his original experimental findings was that infants as young as 12 days old were able to imitate "sequential finger movement (opening and closing the hand by serially moving the fingers)" (Meltzoff and Moore, 1977, p. 76); moreover, other research has now been reported which shows "early imitation of additional non-oral behaviors" (Meltzoff, 1981, p. 101). Potentially, this is a finding concerning bodily functions in the process of imitation; it involves fine-tuned self-corrective behavior on the part of the infant as it attempts to approximate the expression it is imitating, in a sequence *not* at all like that of the animal "releaser mechanism" (1981, p. 101). The "major claim" Meltzoff makes about early imitation is that it "involves active intermodal matching" (e.g., visual perception and tactile imitation)

> in which infants recognize an equivalence between the act seen and their own act which is done at a later time . . . . Our corollary hypothesis is that this imitation is mediated by a representational system that unifies different modalities. (1981, p. 102)

The significance of these findings for psychoanalytic theory is even greater than for Piagetian psychology. As Meltzoff argues, Piaget would have been forced to admit that infants have a capacity for forming accurate representations of stimuli which they imitate, at a much younger age than Piaget had supposed. The empirical findings lead Meltzoff to formulate a conclusion which jars not only the Piagetian edifice of psychology, but rocks the psychoanalytic theory of the infant right off its stand:

> The ability to act on the basis of abstract representations or descriptions of perceptually absent events needs to be considered as the starting point of infant development, not its culmination. (Meltzoff, 1981, p. 109)

Precisely where this starting point would be located in developmental time is still open to question. Many of the findings apply to infants 12 to 30 days old, but newer experimentation has applied to increasingly younger subjects. By 1977, some of the subjects were newborns, one of them only an hour old, and quite able to engage in facial imitations (Meltzoff and Moore, 1977, p. 78). Meltzoff and Moore's findings will require careful interpretation; their meaning is too far-reaching to be clear at this point. Nonetheless, such findings can not be read as a favorable indication for a psychoanalytic theory of infantile cognitive fusion—and thus confusion—of representations of the infant's own body with that of its caretaker, mother, or general environment.

So far, most of the response to these challenges on the part of those who wish to defend psychoanalytic theory in its present forms, has been by way of avoidance. An influential training analyst and theoretician in England, Dr. M. Masud R. Khan, reviewed Sulloway's *Freud, Biologist of the Mind* contemptuously. Khan's reasoning was at this level: "*Mind* is a concept, hence there can be no biology of it" (Khan, 1981, p. 125). Khan similarly denied the value of new research into the circumstances of Freud's early psychoanalytic theorizing, the period in which the seduction theory was devalued. Instead of welcoming the new data, as Kohut might have hoped, Khan has bemoaned the release of certain of Freud's letters written to Dr. Wilhelm Fliess, even on the limited basis that had been taking place prior to their recent publication in full. He has let it be known that any attempts to analyze Freud's psychological processes at the time of the rejection of the seduction theory, even if the methods of analysis are psychoanalytical, must be considered foolish and impertinent (Khan, 1982). Khan's response to the recent challenges to psychoanalytic theory thus is one of dogmatic defensiveness. His objections to the publications of new material pertaining to Freud's famous dream of "Irma's injection" (Freud, 1900, pp. 106-121, 292-295), which were brought out as early as 1966 by Freud's personal physician, the psychoanalytic theorist Max Schur (Schur, 1966), shows a desire to turn back the clock, a wish to erase almost twenty years of research. It was exactly Schur's new evidence which I stressed in my own reopening of the seduction theory controversy

(Efron, 1977, pp. 258-260), and which Masson rightly takes up again. But the very fact that Khan has been led to make such bald, hostile statements can be a sign of recognition that the threats are serious. Khan usually writes with sophistication.

The Seduction Theory: Denials and Issues

More authoritative is the denial by the late Anna Freud, in one of her last letters, that the seduction theory could be incorporated into psychoanalytic thought in any way. Writing to Masson, Anna Freud says:

> Keeping up the seduction theory would mean to abandon the Oedipus complex, and with it the whole importance of fantasy life, conscious or unconscious fantasy. In fact, I think there would have been no psychoanalysis afterwards. (quoted in Malcolm, 1983, p. 125)

Anna Freud of course is defending her father's decision to reject the seduction theory, insofar as she means to say that Freud could not have gone on to his own greatest discoveries without taking this step. I am inclined to agree with her, and in fact have taken that very position (Efron, 1977). However, to say that Freud's abandonment of the seduction theory is warranted by "the whole of the analytic theory altogether" (as she does, in the same letter), and to link this issue to an all-or-nothing position regarding the Oedipus complex and "the whole importance of fantasy life" is merely a revelation of her own immersion in the theory as it exists today rather than of any necessity in psychological theory. It would seem obvious enough that any psychoanalytic theory or for that matter any general psychological theory of the sexual body in infants and children must come to grips *both* with fantasy life and with the actual occurrences of childhood sexual victimization (Finkelhor, 1979). Anna Freud thought that was too much to ask, but I doubt that future psychoanalytic theorists will be able to hold the line, as she urges in her letter to Masson. Some may not even want to.

Masson's accusations in *The Assault on Truth* (Masson, 1984a) have met with a virtual stonewalling, as in the prominent review of his book by Anthony Storr (Storr, 1984). Certainly Storr brought out the weaknesses of Masson's book, but his efforts to serve these up as a pretext for rejecting the seduction theory all over again, seem to have had the effects of overkill. Knight and Herik (1984) rightly label Storr's review one of these "nonresponsive condemnations" that do no one any credit. And even Storr had to acknowledge, in the midst of his attack on Masson's book, that Freud indeed may have badly underestimated the frequency of childhood seduction (Storr, 1984, p. 3). In another effort to stonewall the issue, Norman Holland claimed that, contrary to Masson's account of the matter, Freud points out cases of sexual abuse of children "in most of his later case histories" (Holland, 1984).

But to this Masson was able to reply that after 1905, the *significance* of such abuse of the child's sexual body is not discussed in the remaining 35 years of Freud's writing career (Masson, 1984b). Holland augmented his charges during a session on the seduction theory controversy at the annual conference of the Center for the Psychological Study of the Arts, SUNY-Buffalo, May 12, 1984: Freud's position was not that "all hysterics' recollections of sexual abuse are lies," but neither are all adult hysterias the result of childhood sexual events. Holland's evidence, however, was a Freud passage (Freud, 1925a, p. 34) which contains a bit too much finesse; it sounds suspiciously like rationalization:

> When . . . I was at last obliged to recognize that these scenes of seduction had never taken place, and that they were only phantasies which my patients had made up or which I myself had perhaps forced upon them, I was for some time completely at a loss . . . . When I had pulled myself together, I was able to draw the right conclusions from my discovery: namely, that the neurotic symptoms were not directly related to actual events but to wishful phantasies, and that as far as the neurosis was concerned psychical reality was of more importance than material reality. I do not believe even now that I forced the seduction-phantasies on my patients, that I "suggested" them. I had in fact stumbled for the first time upon the Oedipus complex . . . but which I did not recognize as yet in its disguise of phantasy. Moreover, seduction during childhood retained a certain share, though a humbler one, in the aetiology of neuroses. But the seducers turned out as a rule to have been older children. (Freud, 1925a, pp. 34-35)

For Holland, this quotation from Freud was proof positive that Masson had no case, that the issue was "simply hype." But I cannot agree with Holland's reading of the passage. It contains a repetition of an unequivocal denial that the seductions had ever taken place at all, combined with an admission that some of them did, but that even if they did, they were not very important psychologically, and besides, only non-adults took part in them "as a rule." The whole passage resembles the old story of the man who explained why he had not returned the pot he had borrowed: first, I did so return it, second, I never borrowed it, and third, it had a hole in it anyway. The restriction of the seducers to the actions of other (older) children is especially suspect, in light of Masson's report that Freud had declined to reveal that in several of his cases, the violator was actually the father (Masson, 1984a, p. 93). As Masson pointed out in August, 1984, none of the hostile reviewers of his book have quoted the relevant passages in Freud, previously unpublished, in which Freud showed that he knew of a two-year-old girl who had been "brutally deflowered" by her father (Masson, 1984d; see Masson, 1984a, p. 116).

There is also some question concerning the way we are to read Freud's qualified and incomplete denials of the value of the seduction theory. In another note, this one written in 1924, and attached to a new edition of his essay "Further Remarks on the Psycho-Neuroses of Defence" (Freud, 1896), Freud explained why he no longer accepted his early theory of childhood seductions, but noted that "we need not reject everything written in the text

above," and went on to say that he still thought that the seduction theory "retains a certain aetiological importance . . ." (1896, p. 168). These guarded remarks, however, are probably as close as Freud could have come, within the terms of his own theory of mental functioning, to issue a denial. After all, Freud is the psychologist who alerted us to the manner in which outright denials or "negations" tend to mask the unconscious affirmation of that which is denied (Freud, 1925b). The essay "Negation" (Freud, 1925b) in fact came out one year after the two belated passages concerning the seduction theory were published. Then, one year later, Freud issued the text which would dispose of the seduction theory for good, to all intents and purposes, insofar as psychoanalytic thinking was to follow Freud. In *Inhibitions, Symptoms, and Anxiety* (Freud, 1926a), he made the theoretically fateful leap of proposing that it really made no difference whether a patient's anxiety had an environmental cause or not; anxiety, and by implication sexual hysteria as well, now became a result of endopsychic factors (Efron, 1977, p. 267).

Nor is the problem of the seduction theory important primarily within the framework of Freud's original theory of sexual hysteria. Hunter has shown that some women patients Freud found to be suffering from hysteria might be better regarded as valuable reformers whose hysteria served a successful social purpose (Hunter, 1983, pp. 485-486; following Israël, 1980). In the problem of the seduction theory, we are talking about the tendency or bias of a theory of the sexual body: will it face clearly the facts of family sexual violence, and of incestuous relations in fact as well as fantasy, or will it place all its emphasis on what, as Freud put it, "my patients had made up . . ." (Efron, 1977)? For Judith Herman, whose powerful study of father-daughter incest should be considered part of the evidence in dealing with this problem of theory, it is more than plain that the psychoanalytic tradition has lent itself to denying the sexual violence of the family and reports by victims of actual incest, through its determination to deny the seduction theory. She points out, for example, that "Helene Deutsch's massive *Psychology of Women*, published in 1944, makes no mention of it whatsoever" (Herman, 1981, pp. 10-11).

In the seduction theory controversy lies an indication of Freud's personal allegiance to traditional patriarchal values, that same social order which his emphasis on the sexual body threatened to undermine. The new scholarship on Freud's early decision to drastically de-emphasize this theory will have an impact for a long time to come on how psychoanalysis is understood in therapy and even, thanks to Masson's simplifications, at the popular level. The seduction theory controversy seems to have struck a nerve. When I pointed out that Holland's dismissal of the whole issue leaves psychoanalysis in the position of expressing a theoretical indifference to the extent of child abuse—the theory would remain the same whether there were 2% or 80% of children involved in sexual victimization—and that such a theory could not be good for children's health, Holland replied that he was inclined to agree:

psychoanalysis *is* primarily an endopsychic theory, and it probably is *not* the best theory to use if you are concerned with children's health. Holland then went on to reassert the integrity of the theory as an entity in itself which gives us our best understanding of the human mind (Holland, remarks during session of annual conference, Center for the Psychological Study of the Arts, SUNY-Buffalo, May 12, 1984). But it is exactly the adequacy of the present day psychoanalytic psychology which is at issue.

Where the Challenges Can Lead

The last of the three current challenges, however, is perhaps even more interesting for its threat to the psychoanalytic sense of the sexual body. Again, the infant research findings have often met with denial or with dismissal. One analyst writing on this topic in the *Psychoanalytic Quarterly* (Dowling, 1982) first noted further evidence of the surprising perceptual and learning capacity of neonates, only to go on to declare hastily that this can make no difference to the theory. Dowling found that infants as young as 16 to 21 days can observe and imitate tongue and mouth movements in an adult, and that 29-day-olds can recognize which of two perceived shapes (pacifiers) they had already tactually explored (by sucking). There is nothing in these or other findings to make Dowling even consider that the infant might be able to produce mental representations of self and of the object, nor does he see any relevance to the psychoanalytic assumption that the infant's ego is nondifferentiated, that is, the baby makes no distinction between itself and its mother because it does not yet have a self. Therefore, no change in psychoanalytic theory is called for. Similar dismissive reasoning on infant research findings by Kaplan (1978, p. 256) has been noted by Klein (1981b, p. 84), who refutes Kaplan's denial convincingly. It would appear that denials and dismissals of this kind can only be a form of whistling in the dark; eventually the findings will have to be faced, or the credibility of psychoanalytic theory will be damaged for all but the most willing true believers.

The three challenges to psychoanalytic theory discussed in this chapter all bear upon the sexual body. The first, the biological underpinning recovered by Sulloway, in what has been called the most influential book on psychoanalysis in the past decade (Prawer, 1983), makes it more difficult for future psychoanalytic theory to attempt to bypass the body and sexuality; the second, the seduction theory controversy, will encourage a greater interest in psychoanalysis as a radical critique of traditional family structure, with all of that structure's powers over the sexual body of the child; and the third, the research findings on infancy, will open more minds to the possibility that the newborn infant is a living sexual body rather than a still-to-be-born helpless dependency lacking a self, and teeming with aggressive desire. All three of these challenges coming at once may bring an overall change, or willingness to change, or recognition that change is necessary.

Because psychoanalytic theory has built so grandly on its own image of the infant, the infant research material may be the most serious of the new challenges. I will now examine it in some detail, not only to permit a better grasp of what it involves, but to show why psychoanalytic theory—because it is the only theory that takes infantile sexuality seriously—is still necessary for understanding the human infant; indeed, such understanding is more necessary now than it was during the years 1920-1930, when a cruder but not necessarily inaccurate Freudianism was still drawing attention to the multiple meanings of the sexual body in human life.