CHAPTER SIX

REINVENTING THE ASEXUAL INFANT: ON THE RECENT "EXPLOSION" IN INFANT RESEARCH

The recent revolution in the study of the infant involves a massive theoretical shift from considering the infant as "a passive organism who was the object of forces which determined development," a view taken in very different ways by Freud, Watson, and Gesell, to the mapping out of the competencies that infants have and of the limits to those capabilities. In the years after 1960, "there has been an explosion of infant research of all kinds, and our knowledge continues to expand at a rapid rate" (Appleton, Clifton, and Goldberg, 1975, pp. 102-103). The term "explosion" has occurred repeatedly in the research literature (Klein, 1981b, p. 7; Stern, 1977, p. 144; Stone, Smith and Murphy, 1973, p. vii; Stratton, 1982a, p. 1), and by now things have simmered down. Some have urged that the revolution is over and it is time to get on with other, more important things. But it would be hard to deny that there has been a great access of new and surprising findings in the study of the infant, with results that are confusing.

Research results are still flying in every direction, from numerous disciplines and with few attempts to bring the fragments together. A great deal of confusion is inevitable so long as the underlying theoretical problem of conflicting world hypotheses (Pepper, 1942) is not faced. Stone, Smith and Murphy preferred to edit some 202 articles within their compendium on "the competent infant" (1973), but apologized for slighting the purely theoretical level (p. 5). From the perspective of the sexual body, the theory of infant development remains undeveloped still.

One basic issue lacking resolution is whether early traumatic experience, such as separation from the mother during key periods (e.g., shortly after birth) has any long-term deleterious effects on a child, whether these effects are seen at the time, somewhat later, or even in old age. Psychoanalytic theory is committed to a hearty "yes" in reply to this question, and the arguments of Leboyer and the whole "natural childbirth" school concur, but there are good researchers who deny that any such thing is known at all (Chess, 1978; Chess and Thomas, 1981; Clarke and Clarke, 1976; Goleman, 1984b; Kagan, 1976, 1980; Vaillant, 1977). The controversies, problems, and disputed issues put before us basic questions about infant life which are still unanswered, although they perhaps are not quite as unanswerable as they once were thought to be. One huge, persistently confusing factor underlying much of the difficulty in all such research is the evident bias in favor of cognitive rather than emotional knowledge, showing little consideration of the body or sexuality.

In their survey of psychological research on the emotional development of infants, Cicchetti and Pogge-Hesse (1981) report that the topic had been sadly neglected; in fact, the psychoanalytic literature on "attachment" was practically alone in the field (pp. 215-216). This neglect goes hand in hand with a 40-year lack of interest in human emotions by psychology under the influence of behaviorism (p. 218). Piaget's developmental model virtually excluded sexual development, as Goldman and Goldman argue (1982, pp. 10-12). Nor did Piaget's followers do any better. Goldman and Goldman report on a survey of 1,500 Piagetian studies, none of which have much to say about sexuality (Modgil, 1974). To their credit, Goldman and Goldman do try to right the balance; they are Piagetians themselves in large part. They find that Piaget's stages of cognitive development apply, but only with considerable adjustment, to the development of children's sexual thinking (pp. 375-377).

As for research into childhood sexuality by academic psychologists in general, I offer this comment by Goldman and Goldman (1982):

> . . . Recently the *American Psychologist* published a special issue (1979) entitled "Psychology and Children—Current Research and Practice" to commemorate the International Year of the Child. Although intended as a survey of Child Psychology and Child Development, a thorough reading reveals nothing on the normal sexual development of children and no mention of sexual thinking, despite several informative sections on children's cognition. One would at least expect in the section headed "Identifying the Problems and Needs of our Children" there would be some recognition of the area of sexuality. The nearest reference is in the article on "Child Abuse" which mentions the sexual abuse of children in defining the terms used, but concentrates the discussion entirely on physical violence. An examination of the bibliography confirms this tunnel vision. The only other allusion to children's sexual development is in the article on "Divorce" . . . . (pp. 2-3)

Thus it appears that Freud's early challenge to psychology, the challenge of childhood sexuality, has never been confronted by "respectable" scientific psychology. There is no rival in this field to contest the status of psychoanalysis as the key discipline in the study of the sexual body.

Even behaviorism, when it occasionally did have something to suggest regarding the sexual body, was ignored during the long period of its own dominance. In 1928, J.B. Watson, no less, reported "rage" as a reaction to the prevention of movement of the child's limbs, but there was very little research interest in his observation (Watson, 1928, cited in Jackson and Jackson, 1978, p. 178). Yet the linkage between restriction and rage is culturally significant in a civilization which has depended on rage as motivation for its wars of righteousness (Efron, 1981).

One reason behaviorism would not study the development of affect in the infant is given by Campos and Stenberg (1981, pp. 306-307): how could we find the primary reinforcers in a neonate? Besides, as they also point out, it would be a bit silly to suppose that direct reinforcement is the major mode of learning for the infant, since rather early in a young child's life, it becomes important to understand warnings about various dangers (such as electrical sockets in the home) which cannot be confronted through reality testing. But long before the current research "explosion," some psychologists had proposed theory and pointed to evidence which would have allowed for the investigation of infantile emotional development. Charlotte Bühler suggested in 1930 some of the findings of today's research concerning neonate responsiveness to face and voice during the fifth through ninth months of life; she based these suggestions on exploratory but careful experiments (Bühler, 1930, cited in Campos and Stenberg, 1981, p. 305). K.M. Bridges, in several articles on infantile emotional development published in the early 1930's, maintained that in the first three weeks of life an infant learns through experience to differentiate what was initially a state of generalized "excitement" into the discrete emotions of distress and delight (Bridges, 1930, 1932, cited by Ciccheti and Pogge-Hesse, 1981, pp. 240-241). The precise theories and assumptions of Bridges need not be described here for the point to be underscored: as early as 1932, we had observations pointing strongly to the development of different emotions in the infant. In fact, Charles Darwin, in his "Biographical Sketch of an Infant," offered a whole set of interesting, subtle observations on emotional expression, especially facial, in babies (Darwin, 1877). But little was done with this line of research until quite recently. If the infant's emotional life could not interest researchers, how much less would the sexual life, with which emotion is intertwined, attract their interest.

The common explanation for the long delay in learning that the neonate is competent, active, and equipped with very considerable sensory-motor capacities has been the sheer difficulty of studying a human being prior to the development of most motor abilities and all language. This explanation rings hollow, however. There is no reason why it should have taken until 1966 to learn that a "quiet, alert state" is what the infant demonstrates during most, if not all, of the first hour after its birth (Desmond et al., 1966, cited in Klaus and Kennell, 1976, p. 66). During this state, the eyes are wide open and the newborn is able to respond to the surrounding environment. All one would have had to do would have been to look at the infants and notice, although that kind of looking might not have shown the alertness if obstetric drugs had been used in the birth. Actually, just learning to *look* at the infant was what enabled the most startling breakthroughs in the field of infant study to occur. In the study of infant visual behavior, says M.M. Haith, one of today's leading research workers in that area, "an adequate methodology was lacking until Fantz (1958, 1961) popularized an easily implemented technique which opened new vistas in infant visual research" (Haith, 1980, p. 1). The number of studies since 1960 that have followed Fantz's easily implemented method has numbered into the hundreds. The technique, as Haith puts it, was "very straight-forward . . ." (*ibid.*). "A person can determine what a baby attends by watching the baby's eyes! The reflection of the stimulus the baby fixates appears on the cornea near the center of the black pupillary opening of the eye" (Haith, 1980, pp. 1-2). You give an infant a chance to look at two different visual stimuli while you watch its eyes to see which one it looks at most. Or, you present stimuli in sequence and see which one gets the most attention from the infant. This methodology is so simple that it could hardly have been sought for within the programmatic scientific ambitions of academic psychology, but once found, it "has been unmatched for utility and applicability to a wide range of issues and problems in infancy including: attention, habituation, learning, cognition, preference, detection, discrimination, recognition, identification, intermodal and space perception, motivation and affect" (Haith, p. 2).

What has been learned about all these issues and problems is that the infant is far more competent than had been believed. But the issues and problems have been studied singly, or in small clusters of groups, with the highly complementary results brought together periodically in surveys. Theories are either lacking or one-sidedly cognitive. Peter Stratton, editor of the recent Wiley text, *Psychobiology of the Human Newborn* (1982b), repeatedly urges the most circumspect doubt and caution concerning the meaning of neonatal findings. The series editor, K. Connolly, seems to suggest that now, after thousands of careful experiments over the past two decades, it is time to begin thinking: ". . . we now have in outline form a framework within which to analyse and consider the newborn's behavior" (Stratton, 1982b, p. xiv). An example of a one-sided cognitive approach is found in G. Butterworth's, *Infancy and Epistemology: An Evaluation of Piaget's Theory* (1981). The book makes no mention of such matters as feeling or emotion, much less sexuality; it concentrates on memory, imitation, and representation, overlaid on a concept of the infant body as a desexualized sensorimotor apparatus. Nor would one gather that the infants described in the Butterworth collection ever went through the process of childbirth. Yet the type of childbirth—whether "natural" or, as Leboyer might put it, "assaultive"—would have to be taken into account if we are really to "analyse and consider" the infant. As one of the writers in Stratton's collection acknowledges, we now have excellent evidence that obstetric drugs do have harmful effects on the newborn:

> Medication during the labour is associated with degradations of a variety of behaviors in the newborn, including sleep, arousal attention, motor competence, and sucking and feeding. (Barrett, 1982, p. 275)

As the work of Brackbill (1975) and Muller et al. (1971) has shown, some of the most common drugs in obstetric use have been shown to affect IQ at age nine! But while Barrett cites all this information and Stratton includes the comments in his anthology, no one seems to realize that (with very rare exceptions) the findings surveyed throughout the same anthology are not considered with regard to the simple distinction: drugged or not. Stratton's own attitude may be inferred from his demonstration that neonate individuality shows up strongly even in a baby born under difficult conditions, with induced labor and 22 hours administration to the mother of the drug synoctin (Stratton, 1982a, p. 4; see also his Fig. 1 , showing photos of this infant on the first successive 28 days of his life). It may be argued that the demonstration of individuality here occurs *in spite of* the drugging; without drugging, the quality would have been otherwise, as the Brackbill and Muller studies indicate.

The sexual body somehow got left out of the research explosion, and now the infant is moving toward being understood once again as asexual. Jerome Bruner, in a major review article on the study of the infant (1983a), currently supports the idea that infants are not as intelligent as their parents—or their researchers—have come to assume. He opts for a purely social theory of the formation of the self, in the tradition of George Herbert Mead. But the emphasis is not even on Mead's great theory of the child's active acquisiton of a self through "taking the role of the other," but on the infant's being guided toward the development of a social self through the influence of parental teaching. For this theory, Bruner relies on the work of Kenneth Kaye (1982), which is aptly subtitled, *How Parents Create Persons*. Now the theory of the self, the "person," is not what I would wish to focus upon in this study of the sexual body, but somewhere it is unavoidable as part of my topic. It is enough to say at this point that in Kaye's theory of how a baby should progress to becoming a person, there is very little room for the sexual life of the infant. You would never know, for example, that an infant has genitals and touches them. There is practically nothing on sex nor the sexual body anywhere in Kaye's book. Given that absence, and given Kaye's belief that the parent—not the infant—must very carefully engage in "the management of the infant's level of arousal" (Kaye, 1982, p. 230), the possibility for sexual self-regulation on the part of the child is nil.

Reviewed along with Kaye's book in Bruner's article is Howard Gardner's book on the theory of "multiple intelligences" (Gardner, 1983). The "frames of mind," which appear to be related to the differential faculty psychologies of the past, go well with the parental "frames" in which Kaye sees the infant benignly contained. They also are asexual. Even the chapter on "Bodily-Kinesthetic Intelligence" (Gardner, 1983, pp. 205-236) is asexual. There is no sexual body to be investigated in these powerful, sophisticated new theories. The slate has been wiped pure. True, the fact that both Kaye and Gardner are former star graduate students of Bruner at Harvard (as Bruner mentions in his autobiography, 1983b), may have something to do with the way the review is written. But that would not explain the paucity of sexual focus in Michael Lewis' new edition *Origins of Intelligence: Infancy and Early Childhood* (Lewis, 1983). The 15 large survey essays in this book contain very scant mention of sex, the body, infantile penile erection, vaginal lubrication, or even REM dreaming. A small amount of data on emotions is tucked under "affect." That is as close as these all-intellect infant artifacts are allowed to get to the sexual body. There is now a veritable army of infant researchers; neglect of the sexual body in this research can only be due to the aggregate of choices made by this group. Bruner reports:

> . . . infant psychology and even infant psychiatry are now both flourishing professions, their respective world conferences in the last couple of years each drawing more than a thousand participants. The study of development, and particularly of development during infancy, has become a major growth industry. (1983a, p. 84)

Only a few of these workers in the professions of infant development and a few of their predecessors have fought for the issues of the sexual body. Among the few are those who have raised the question of what happens to the infant when it goes through the process of birth.

The Leboyer Challenge to the Disciplines and to Cultural "Assaultive" Birth

In the background of the dispute over childbirth and evidence lies some 40 years of "natural childbirth" advocacy, by Grantley Dick-Read, Reich, Ritter and Ritter, Lamaze, Bradley (see Sheleff, 1981, p. 238), and finally Leboyer and his associate, Dr. Michel Odent. In the literature on natural childbirth, the effects of drugs have been repeatedly noted. There is good evidence that the "home birth" movement in the United States, Canada, and Europe, has led to birth practices in which there is significantly less use of drugs, and with no loss (in fact probably with some gain) in overall health safety for mother and baby (Hahn and Paige, 1980). There is thus both a cultural option (people may choose natural childbirth or conventional obstetric delivery), and an interdisciplinary research issue (effects of drugs used in childbirth procedures). The dual option of research and cultural change is often the case with the sexual body.

Both possibilities might have been in view for Ann Oakley, who makes a nervous comment on Leboyer in the Stratton volume—virtually the only one in all of the articles, except for a gibe by the editor (Stratton, 1982b, p. 7). After surveying obstetric practices cross-culturally, Oakley (1982) writes:

> The pleas made by Frederick Leboyer (1975) on behalf of the newborn hark back to some of the neonatal practices I have described in this chapter, but the only randomized controlled trial of Leboyer delivery so far conducted came up with the interesting conclusion that the only difference between the Leboyer delivery group and the control group of women undergoing normal "humane" childbirth was that the first group had a shorter first stage—presumably in anticipation of the delights of the Leboyer delivery in store for them . . . . (Oakley, 1982, p. 311)

Oakley is bemused, I suppose, and perhaps also is a bit smug or enthnocentric: we in the West, she implies, can not be expected to "hark back" to preliterate societies (Oakley, 1982, p. 311). Yet the anthropologist Stanley Diamond has given a reasoned theoretical case for the crucial need of the West to "hark back," which does not mean to copy or simply imitate, the so-called primitive world before it disappears entirely (Diamond, 1974).[1] Oakley is partly scoffing at the subjective judgment of the mothers, whose concerns are reduced to the immaturity of anticipatory "delights." She is also reflecting her own world hypothesis, which is grounded in class struggle and in feminism. As she argues in her book, *The Captured Womb* (Oakley, 1984), the modern hospital has not come to grips with the actual problems of poverty and class inequality which millions of women have, and variations in childbirth care such as the Leboyer method do not fundamentally change this inequity. Nor does Leboyer directly attack the domination of childbirth procedures by the male medical doctor, a problem which also concerns Oakley very deeply.

The randomized study (Nelson et al., 1980) to which Oakley refers is not any real test of Leboyer's theory. Anyone who takes note of how *Birth Without Violence* is written will realize that Leboyer was quite sincere when he said, in an address I heard at SUNY-Buffalo (November 10, 1978), that he does not actually have a "technique," a series of steps to be followed in childbirth, but an attitude of love and of living respect for what the infant must be going through at birth. It is with reason that Dr. Leboyer has refused all his life to publish his work on childbirth in any medical journals: his own sense of the sociology of medicine makes him hold that change would not occur through

[1]Morris (1980) has explained Diamond's "project of understanding the primitive . . . as one primary means of disclosing the 'nature of human nature' or 'human possibilites' denied in civilization. Diamond posits a radical transformation of civilized life informed by a critical knowledge of the primitive" (Morris, 1980, pp. 99-100).

those journals in any case; moreover, how could he talk about "love" in a medical journal? Yet love, and as I will show, love in a way that is quite bound up with the sexual body, is the basis for what Leboyer has to say. The *New England Journal of Medicine* study is not investigating love, nor does it mention it; instead the investigators wanted to find out if Leboyer delivery is "better" than conventional, humane birth, in these terms:

> The outcomes that we examined were the safety of the mother and the infant, infant irritability and responsiveness in the neonatal period, maternal experience and perception of her labor and delivery, and maternal perception of the infant. (Nelson et al., 1980, p. 655)

The first of these "outcomes," on the safety of the Leboyer delivery, did in fact show that it was safe, contrary to the experimenters' expectations (p. 659). It is doubtful, however, that anything is accomplished by having the fathers give their newborn babies a warm bath, under the conditions of this experiment, since this ignores Leboyer's idea that the newborn is somehow sensitive to hostility anywhere in the room (including the research assistants engaged in observing reactions). In a hospital in which the Leboyer delivery was being put on trial, it is not surprising that the 19 infant baths worked in just the opposite way from what Leboyer had found in the thousands he and his co-workers had supervised: the newborns either stayed "alert," unrelaxed, or they "reacted with irritable crying" (p. 657). The experimenters present this finding as if it were definitive, not pausing to wonder at the implication that Leboyer would have had to have been extraordinarily lucky in his hundreds of deliveries using the same bath "technique" to have found that almost always the result was profound relaxation for the newborn. They do not have a word to say about the one statistically significant positive result they did obtain: "at eight months, the mothers in the Leboyer group were more likely to say that the delivery experience had influenced the child's behavior ($p = 0.05$)" (Nelson et al., p. 659).

This study, which ends with the relieved assurance that prospective parents need have no qualms about the wisdom of well-conducted conventional childbirth procedures, strikes me as an artifact created by the experimental design and the experimenters' biases (Rosenthal, 1966). In any case, Oakley's reference to this study as "the only randomized controlled trial of Leboyer deliveries so far conducted" is misleading even if true to the canons of research. There exists some follow-up research in France which supports Leboyer's claims. Some 120 "Leboyer" babies, at various ages up to three years old, show better than average psychomotor functioning, are very largely free of digestive, sleeping, toilet training, and self-feeding problems, and also "from the paroxysmic crying associated with the neonate." Important too, considering the recent interest in right and left brain hemisphere functioning, is that these babies commonly remain ambidextrous (Kliot and Silverstein, 1980; Rapaport, 1976). The *New England Journal of Medicine* study dismisses the French follow-up as "uncontrolled," and does not inform its readers of this result, nor in fact of any of the specifics of the follow-up. The infants were given the Brunet and Lezine test for psychomotor functioning, and scored a good deal higher than the average comparable French baby; a control group is hardly necessary for this to attract notice. But it got none in New England, nor from Oakley, who also omits any reference to the writings of Dr. Leboyer's co-worker, Dr. Michel Odent (Odent, 1976, 1979; see also his books in English, Odent, 1984a, 1984b). Odent's work at the childbirth clinic in Pithviers, France, had drawn the attention of *The Lancet* (Gillett, 1979), well before the publication of Oakley's article in 1982. Oakley also chose not to comment on (or simply did not know of) the favorable reports on Leboyer's "gentle birth" by Oliver and Oliver (1978), or of Salter (1978). A review of the research on Leboyer by two authors who support his work, the obstetrician David Kliot and the psychiatrist Louise Silverstein, provides controlled data showing a lesser degree of internal physiological tension and a greater alertness in the Leboyer newborn than in the controls:

> Salter observed the state of six "Leboyer" and six control group infants during three 15-minute observation periods within the first 24 hours postpartum. Although she did not analyze her data statistically, she reports that the "Leboyer" infants spend more time in the quiet-alert state than did control group infants in all three observation periods. Oliver and Oliver, observing spontaneous behaviors occurring in the delivery room during the first 15 minutes postpartum in "Leboyer" and control group infants ($N=17$ in each group), found that the "Leboyer" babies spend significantly ($p < .01$) more time with eyes open and hand muscles relaxed than did control group babies. (Kliot and Silverstein, 1980, pp. 283-284)

But I realize that such "significant" results could easily appear unimportant: after all, what are 15 minutes? Without an appreciation of what Leboyer was trying to achieve and the assumptions behind that effort, such research can be written off.

Let me turn to Leboyer's own descriptions of his project. These must have provided a shock for any respectable, conventional scientific psychologist who happened to read them. The love that Leboyer wants in the delivery room is not only a term that is simply awkward for the experimenter; it is also sexual. Arguing that since we can use neither language nor, very well, gesture, to communicate with the newborn, we must use "love," Leboyer imagines himself asked if he really means that we must "speak of love" to a newborn baby.

> Yes, speak of love. Speak the language of lovers.
> And what is the essential language of lovers?
> Not speech. Touch.
> Lovers are shy, modest. When they want to embrace, they seek the darkness; they turn

> out the light. Or simply close their eyes.
> And in this darkness, they quiver, caress each other, lightly stroke each other.
> Put their arms around each other. (Leboyer, 1975, p. 36)

It is not a matter, as the conventional experimenters thought, of having the baby receive a "massage" from its mother and a warm bath from its father. Without the involvement of the adult sexual body in this contact, the "techniques" are meaningless. Lest there be any doubt that Leboyer intends a close link between loving the newborn and adult sexual love, note the following:

> But, people will say, you're making love to the child:
> Yes, almost.
> To make love is to return to paradise, it is to plunge again into the world before birth, before the great separation. It is to find again the primordial slowness, the blind and all-powerful rhythm of the internal world, of the great ocean. Making love is the great regression. (Leboyer, 1975, p. 62)

These statements would have been worse than incomprehensible to the research worker unfamiliar with a positive sense of the sexual body. They would have produced hatred, even, the same hatred of the sexual that I have shown as a broad cultural substrate in many of the writings of psychoanalysts such as Melanie Klein and D.W. Winnicott. Leboyer, as did Freud before him, strikes something deep in any reader, when he makes the sexual body his underlying focus. His disciple, Odent (1984a) continues to deepen the sexual body focus of natural childbirth; photographs from Odent's *Birth Reborn* (1984b) of women giving birth in an upright, supported position, show strong indications of orgasmic gratification on the part of the woman at the moment of birth (Kahn, 1984). Sheila Kitzinger has discussed this phenomenon under the term "birth passion" (Kitzinger, 1983, p. 109).

Leboyer's reason for allowing the umbilical cord to stay after birth until it stops pulsating is not only that it is "gentle" to allow the infant a slower leave-taking from the womb, but that the early minutes of life allow for immediate practice of self-regulation on the infant's part: the newborn is not forced to start breathing at once, but can, and in Leboyer's observation, does, experiment at her or his own pace with letting go of the oxygen supply from the cord and changing over to that from the air in the room. This issue of self-regulation (a Reichian consideration, as I have shown in discussion of Ritter and Ritter, above), is matched by the issue of aggression: the sudden clamping of the cord confronts the infant with aggression, hitting the infant at the vital level of its breathing, almost at the instant of birth. This is the beginning of a habituation to fear which will be very hard to overcome later on in development (Leboyer, 1975, p. 51). Leboyer is making a value judgment: he prefers a world where not aggression, but the sexual body, is the center of value.

The Natural: A Category or Not for World Hypotheses?

From another discipline, psychohistory, evidence seems to be emerging that would connect mass social outbursts of violent aggression in war with fantasies of painful, strangulating birth experiences. Lloyd DeMause, editor of the *Journal of Psychohistory*, has collected examples from political rhetoric and press commentary during periods preceding the outbreak of war and found them loaded with

> figures of speech and images related to biological birth. These involve strangulation, choking, being crushed, dark caves, craving for the light on the other side of the tunnel, descent into the abyss, life and death struggle for breathing space, drowning, hanging, and feeling small or helpless. (Grof, 1977, p. 303)

The very fact that DeMause is working in a controversial new discipline (and making upsetting generalizations as well as daring speculations) makes it unlikely that respectable psychological research workers will take up his hypothesis for experimental investigation. This would be all right if the issues he raises did not matter, but unfortunately they matter all too much for social survival.

The problem thus is not merely methodological (can we trust Leboyer's account, based on the 1,000 births he has worked on, and those of the follow-up studies?), but world-hypothetical: can anything having to do with the natural connection of infant body and mother's body be taken seriously in scientific research today? And, if the answer should be *yes*, can sexuality be included and considered seriously in theory, to be followed by appropriate social practices? If your world hypothesis contains no major category for the natural, then you will be tempted to exert your efforts or lack of effort toward a negative reply to the question I have posed.

Thus Hahn and Paige, psychologists who have an excellent understanding of the success and the limitations of the "home birth" movement, qualify the term "natural childbirth" by saying—correctly—that in many respects it is still a "medical birth." Their interest, much like that of Oakley's cited earlier, finally lies in the power relations between the medical profession and women of all social classes who give birth. The natural childbirth movement is not of prime importance in this struggle, they believe, partly because it is a middle-class movement (Hahn and Paige, 1980, p. 169). They are missing something: although it is true that a contemporary natural childbirth cannot be natural in the same sense the word would have were it applied to birth practices prior to the rise of modern medicine, there is still far less interference with the biological childbirth process in "natural childbirth" than there is in alternative obstetric practice. There seems little chance of avoiding the term "natural" here, however much we may attempt to avoid its implications in order to think instead in terms of such social ideals as egalitarianism.

Three major controversies in infant research which demonstrate the shadow-boxing of alternative world-hypotheses behind the portrait of psychologists engaged in a collaborative scientific enterprise are the problem of neonate visual preference for the human face, neonate synchrony of body movements with the human voice (especially female), and the theory of maternal-infant bonding. In the context of an examination of these issues, I will also comment on a number of additional research breakthroughs in which the sexual body does not exist: it has been erased by the investigators. In the background of these problems hovers the issue of the category, "natural."

Faces, Body Conversations, Vision and The Sexual Body, Mothering without Touching

Despite decades of research, pioneered again by Fantz, which has led many observers to conclude that "there is an innate bias in human newborns to attend visually to human faces," the highly respected Hanus Papoušek and Mechtild Papoušek calmly report that the latest surveys of all the research tell us "that there is only very little evidence" for this finding, "and that the perception of faces seems to follow the general course of form and pattern perception" (Papoušek and Papoušek, 1982, p. 375). That the Papoušeks can write this in the context of their comprehensive survey of research on the infant's "Integration into the Social World," and that this essay makes no reference to infantile sexuality of any kind (psychoanalytic or other) is ominous. In fact, the only discussion the Papoušeks offer of emotional factors in the social integration of the infant occurs in a preliminary paragraph of a section aptly entitled "Early Integrative Capacities: To Know Means Almost Everything" (pp. 369-374). The experiments discrediting the theory that infants visually prefer the face consist primarily of a methodology of confusing the neonates with two dimensional patterns, some of them schematically facial and some with various facial elements skewed or omitted. These experiments lead to the conclusion that it is not until the age of 5 months that "facial communication patterns" are "available to the infant," and that up until that time "the use of patterned visual information involving the whole face is not likely to occur . . ." (Campos and Stenberg, 1981, pp. 296-297). With such language we are now referring to information processing, in which indeed the slogan may be applied: "To Know Means Almost Everything" (see discussion below, Chapter Eight). But let me speculate that a live face may emit affect, in some ways, which the infant can receive (and may be especially equipped to receive), and hence that the infant's inability to know whether the cardboard pattern before it looks like a face or not is irrelevant to the understanding of infancy.

What is at stake in this de-emphasis on the possibility of the newborn's preference for the human face is not merely that set of findings, but a whole view of the human being: does the baby need warm human contact for its "integration," as Prescott's arguments strongly suggest, or is development mainly a cognitive matter having to do with such abstract matters as "form and pattern perception"? By presenting the findings only within discrete fields, such as form and pattern perception in the infant, the researcher can avoid the larger implications of an infant who not only seems to prefer the face, but also the human voice, the human skin, and the human body, rather than neutral or non-human stimuli.

Findings of conversational synchrony between adult voice and neonate body movement (Condon, 1977; Condon, 1979; Condon and Sander, 1974a, 1974b) have led to a research impasse discussed at length by Rosenfeld (1981). Once again, it appears that the impasse is due to differences in world hypotheses, not to the demands of scientific research. The findings show that as early as the first day of life the infant "moves in precise and sustained segments of movement that are synchronous with the articulated structure of adult speech" (Condon and Sander, 1974a, p. 99). These rhythmic movements had not been observed prior to the Condon-Sander research design because of their short duration (a small fraction of a second) and their complexity of location and coordination in the neonate's body. But as a result of that research design, coordinated movements in synchronization with adult speech patterns (regardless of which language is spoken, e.g., English or Chinese) can be seen. They show up, to take a typical example, in the neonate's head, left elbow, right shoulder, left shoulder, right hip, and toe of left foot (*ibid.*, p. 100). As Rosenfeld points out, these discoveries have immense implications for the study of language, communication, the nature of the infant, and social interaction: "Surely these discoveries must rate among the most significant of any science" (Rosenfeld, 1981, p. 73).

The research impasse comes about because no one outside of researchers connected with Condon and Sander have even *attempted* to replicate the experiments, despite their importance, and despite the heated state of research in human infant studies. Stratton (1982a, pp. 140-141), who cites only the 1974 study by Condon and Sander, sees the matter as one more instance in his "chronicle of loose ends" (Stratton, 1982a, pp. 140-141).

Rosenfeld has serious doubts about the findings of Condon and Sander, on logical and methodological grounds, and expresses the hope that his own call for independent replications will be heard (Rosenfeld, 1981, p. 74). The disinterest in replication of these widely publicized experiments considerably puzzles Rosenfeld. In terms of the present study, the importance of the human body as a function of intelligent social interaction in the infant-adult relationship is probably what turns most researchers away. At the same time (as Rosenfeld suggests, p. 74), those who welcome the results are also afraid that replication might not validate them, and hence do not attempt it.

An analogous research development (though one that has not resulted in a controversy) is the finding by Stechler, Bradford, and Levy (1966) that neonates of two to six days old show body behavior correlated with their giving visual attention to anything they are looking at. The conclusion is reached by measuring skin potential, which is inherently tied to the autonomic nervous system. Infants do not have to be *moving* in order for their bodies to be very active: "It is clear that electrodermal reactivity is enhanced during the state of target fixation" (p. 1248). But while some of Stechler's other research is incorporated in the comprehensive volume by the distinguished researcher M.M. Haith (1980) on the organization of newborn visual activity, the findings on skin potential and visual focussing are not even mentioned. By narrowing his own focus, Haith produces a hypothesis that is purely neurological: neonates engage in "visual activity" in order to "keep the firing rate of visual cortical neurons at a high level." Such a level is judged to be adaptively essential for later development, and is not explicable as a stimulus-response situation (Haith, 1980, pp. 106-118). Haith thus emphasizes brain neurology, to the exclusion of all else whether it be affect, emotional value for the infant of visual activity, or the body activity shown by Stechler. Brain is here taken as synonymous with the human being, while the rest of the body is ignored. In other words: a choice of brain over body. Had Condon and Sander come up with results showing that the infant *brain* is coordinated with adult speech but not that the body is intimately involved, Rosenfeld's puzzlement over the lack of interest in replications might not have been necessary.

This supposition is born out by Daniel Stern's well-known "microanalysis" of mother-infant interaction (Stern, 1971, 1977). Stern's findings give good empirical support for showing that the infant (age 3½ months) takes an active role in regulating its own contact with his or her mother through eye contact and its avoidance, thus countering earlier widespread assumptions of infantile passivity and helplessness, as well as psychoanalytic expectations that regard neonatal functioning as a matter of symbiotic fusion with the mother, a situation that does not allow for a perceptual differentiation on the infant's part of the two different bodies. Indeed, Chodorow has argued from psychoanalytic evidence that the infant could not even be aware of the good care it is getting from its mother (!) because, as Winnicott had put it, the infant exists as "almost a continuity of the physiological provisions of the prenatal state" (Chodorow, 1978, p. 84, quoting Winnicott, 1960, p. 592). Stern reports that the infant's active turning away of his head to break contact occurs as early as the second week of life (Stern, 1971, p. 503; following Stechler and Carpenter, 1967; Stechler and Latz, 1966). However, Stern, like Haith, does not mention Stechler's results regarding skin potential in the neonate, even though he cites other research of Stechler (Stern, 1971, p. 516). This is consonant with Stern's own decision that his study of mother-infant interaction should not include body contact: "all periods when the mother is touching the infants and conceivably restricting or causing infant motion were excluded from this analysis" (Stern, 1971, p. 511). Stern does not realize that this exclusion, far from purifying his study, has rendered it a fiction of the human body, in which eye communication is all and body contact is nothing, during interaction.

The research approach of Campos and Stenberg (1981) is superior to that of Stern, if considered from the perspective of the sexual body. Although immersed in the vocabulary of information processing, Campos and Stenberg are clear at least in their realization that within infant cognition, there is nonverbal communication. Contrary to the tradition of Piaget (which was adopted uncritically by some psychoanalytic theorists), they present excellent evidence from recent research on infants and in conjunction with knowledge of animal behavior, to show that "object constancy" is *not* a prerequisite for certain emotional reactions, such as fear of strangers, that infants show sometime after the fifth month (Campos and Stenberg, 1981, pp. 282-284). In fact, what they are using the information processing vocabulary *for* is to study infantile development of affect rather than knowledge as such. Pursuing neither the criterion of semantic comprehension on the part of this infant, nor the high-precision "systems" synchrony proposed by Condon and Sander, Campos and Stenberg still have made important investigations into the ways that "patterned information in the *voice* can . . . specify affective information" (p. 301). But congruent with their conclusion that the neonate does not evince visual preference for faces until the age of five months, they seem to assume that vocal information—which can convey such affects as anger, fear, sadness, boredom, and joy, and can convey these through variations in pitch and vocal rhythm—also only starts to become important to the infant at five months.

At least, that is where Campos and Stenberg were when they reported their findings in 1981. In only the few years since, there has been much more interest in the emotional life of the neonate and infant. Perhaps the complaints of Cicchetti and Pogge-Hesse (1981), mentioned at the beginning of this chapter, concerning the neglect of the emotions in research on infants, signaled the beginning of the end of the wave of cognitive concentrations. By June 1984, Campos was able to comment on a new study of neonates (Gaensbauer and Hiatt, 1984) which showed that an *abused* infant as young as three months old could look sad:

> Psychoanalytic theory held that an infant could not feel true sadness until he had formed a strong attachment to his mother or some other caretaker—by about eight months. Then he would feel sad when he was separated from that special caretaker. (Campos, quoted in Goleman, 1984d)

The new findings indicate an earlier arrival for sadness, in some infants—and not one that has to do with separation anxiety.

It appears that the infant research "explosion" continues to explode, giving us new and surprising findings, with a general direction that tends to refute more and more the psychoanalytic theory of the neonate. A year before Campos and Stenberg published their excellent survey on infantile emotional development and their own theory of "social referencing," DeCasper and Fifer published new experimental findings in *Science* which showed that infants of three *days* old (or less) show a preference for their own mother's voices (DeCasper and Fifer, 1980). That a newborn would be capable of altering its sucking patterns in order to hear its mother's voice and to tune out other voices and sounds is an indication of the bodily immediacy of voice, a mother's voice, for the sexual body at the earliest age. It would be interesting to know what impact this finding would have on the theories of Campos and Stenberg, who do not cite the work of DeCasper and Fifer, probably because it simply was unavailable to them when they went to press.

Other recent evidence corroborates the findings on infant's and mother's voices by showing that mothers learn to recognize their own infant's cry, distinguishing that cry from others in the neonatal nursery, within three to eight days after giving birth (Morsbach and Bunting, 1979). The tendency of research to concentrate on visual interactions in the mother-infant dyad (Stern, 1971, 1977) might have obscured this reciprocal recognition of voice within the maternal-infant dyad. Similarly, visual relations as an emphasis of research would have tended to overlook olfactory processes; but there now is evidence that shortly after birth, infants are drawn to the odor of their own mother (and not to the odor of another mother), just as mothers are able to recognize their own infant's odor three or four days after giving birth (Schaal et al., 1980). The evidence concerning mothers' recognition of their own infant's cries and of the reciprocal olfactory recognition of infant and mother is cited in support of the maternal-infant bonding theory (Klaus and Kennell, 1982), to which I will now turn.

The Maternal-Infant Bonding Controversy

Maternal-infant bonding is the theory that there is a "sensitive period" in the first few hours (or days) following birth in which valuable emotional ties are created through somatosensory and visual contact between mother and infant. As a theory, it is distinguished from the notion of bonding as a generalized process of attachment taking place over weeks, months and years, such as is described by Sluckin, Herbert, and Sluckin (1984). The maternal-infant bonding theory was developed by Marshall H. Klaus and John H. Kennell, both professors of pediatrics, who presented evidence for it in the 1970's, in professional articles (Kennell, Trause, and Klaus, 1975; Klaus et al., 1972) and in a modified, popular format in their book, *Maternal-Infant Bonding* (Klaus and Kennell, 1976). A variety of replication and parallel studies in the U.S., Germany, Guatemala, Brazil, and Sweden have provided confirmation, while other studies fail to replicate the results. In 1982, the concept was given a negative scientific evaluation in a comprehensive review by the psychologists Michael E. Lamb and Carl-Philip Hwang (Lamb and Hwang, 1982). The review by Lamb and Hwang is likely to be regarded as persuasive. For example, Alice Rossi, who at first had thought highly of the concept, now refers to their study as a definitive cause for rejecting the maternal-infant bonding theory (personal communication, August 8, 1983; Rossi, 1984). The issues concerning the sexual body in this controversy will be approached through my own critical comment on Lamb and Hwang's review.

That review is so detailed and inclusive that the reader is likely to lose sight of which studies might apply to Klaus and Kennell's claims and which do not. For example, a study in which *clothed* "newborn" babies were handed to their mothers is included by Lamb and Hwang as "another failure to replicate." Another "large and careful study" with insignificant results is described even though the infants were not given to the mothers until seven or more hours after birth (Lamb and Hwang, 1982, p. 23)—clearly not in time for the critical period as set forth by Klaus and Kennell. When the dust finally clears, it seems to me that Lamb and Hwang have exactly *one* strong failure to replicate to report. This is by Svejda, Campos, and Emde (1980). These researchers entitle their article "Mother-Infant 'Bondings': Failure to Generalize."

Stella Chess and Alexander Thomas have promptly reprinted this report of failure in their *Annual Progress in Child Psychiatry and Child Development* (1981), where it underscores their own research emphasis: a longitudinal study following the infant into adult life, under the rubric of such variables as "activity level," "attention span," "persistence," "quality of mood," "distractibility," "threshold of responsiveness," "approach or withdrawal," and "intensity of reaction." (See discussion in Jackson and Jackson, 1978, pp. 191-195; also Goleman, 1984d.) These—and even the characteristic of "rhythmicity" which Chess and Thomas study—are defined too abstractly to admit of anything sexual. It is not surprising that they welcome a study, begun while their own massive project is still in progress, which assures us that nothing important really happens during the critical period. That period occurs too early to figure in their own work, or to have been investigated in their project. They are probably apprehensive, as well, over their own reliance on interviews with the parents for the first three years of their project, for what they term "the obvious reason that no one else is in a position to supply as detailed and comprehensive picture of the child as they can" (Chess, Thomas, and Birch, 1965, p. 24). Besides, they had concluded on the basis of preliminary study that "the newborn infant's behavior varied significantly . . . even hour from hour, and that data collection and analysis would be an exceedingly demanding and complex process" (Thomas and Chess, 1977, p.

20). They also believe that not until the child is between the ages of six and ten years can reliable predictions concerning personality be made (Thomas and Chess, 1980, p. 114). With all this distaste for the study of the neonate, it is little wonder that Chess and Thomas welcome a "failure to generalize" concerning the early bonding theory.

But this "failure" concerns observations made over the first 36 hours after the infants' birth. Lamb and Hwang emphasize that it is a very well-designed study; they credit it as a failure to replicate within their category of "short-term" consequences of early bonding. In fact, the Klaus and Kennell theory extends to findings and speculations well beyond this short term, and several other studies *confirm* the short-term benefits to some degree. There is hardly a clear-cut "failure" involved.

What else is in Lamb and Hwang's critical review? Aside from further irrelevant studies of clothed infants (1982, pp. 15-16), there is a large amount of positive confirmation, which they attempt to discredit. In a study of "104 lower income primaparae and their newborns," it was found that mother-infant dyads in which early contact had occurred, had several notable indications of emotional bonding: the mothers were less easily distracted from the infants, the mothers and infants both seemed more calm and relaxed than were the control dyads, and the mothers more frequently took the initiative in "stimulating the infant with facial communicative behavior" (Lamb and Hwang, 1982, pp. 14-15). The Brazelton scales showed that the early contact infants "showed fewer stress responses" (p. 15). These confirmations are eagerly disparaged by Lamb and Hwang: ". . . these findings may simply have occurred because the early contact infants were more sleepy" (p. 15). While there is a report in the experiment that they were harder to arouse from sleep, the objection seems more exaggerated than the claims. Similarly, in one of the Swedish studies of short-term effects, the research findings included a report that the babies in the control group cried "a great deal more" during the observation period at 36 hours. Lamb and Hwang suggest that this discredits the positive results of the study: it leads "one to wonder whether the group differences were attributable to differences in the infants' behavior" (p. 14). That 20 babies just happen to behave differently in a control group from 22 other babies in an early-contact group is not a glowingly persuasive scientific argument! On the face of it, the difference in crying is another indication of the effects of early contact or its lack, even if the experimenters were not *measuring* crying. In a follow-up study of long-term effects for this same group, Lamb and Hwang similarly try to explain away the confirmation of the value of maternal-infant early contact with an *ad hoc* hypothesis that perhaps "the control group was temperamentally more irritable (remember that they cried at both 36 hours and 3 months)" (p. 21).

A number of experimental observations in Guatemala concerning early contact and breastfeeding are described *en bloc* by Lamb and Hwang, who give the impression that these studies have no confirmatory value. In one of these studies, however, "it was found that the early contact mothers breastfed significantly longer than mothers in the control group. In addition, these infants gained more weight over the first 6 and 12 months" (p. 25). Lamb and Hwang distract from this finding by first reporting two other Guatemalan studies, performed under different conditions, where the evidence is not favorable to the theory. They comment: "Only in one of three studies, therefore, was there evidence that early contact affected the duration of breastfeeding" (p. 14). To be sure, the evidence of these three studies is difficult to evaluate, and it does seem to conflict. Yet the study of breastfeeding duration, at a hospital that served mothers from lower middle and middle class families, is hardly negligible. Lamb and Hwang comment further that in any case the mothers in that sample did not nurse their babies as long as Guatemalans are usually reported to do, but they fail to mention the influence of infant formula giveaways on nursing habits in the third world, which are widely reported to have discouraged breastfeeding.

The Guatemalan confirmation might have been considered in conjunction with another study from Sweden. This is a careful study, and it showed that "mothers who experienced early contact showed more contact and fewer noncontact behaviors" during two different observation periods (p. 17). According to Lamb and Hwang, another careful study in Germany reported in 1981 that mothers who had had early contact with their newborns showed more tender touching and cuddling on the second and third days than did the control mothers (Lamb and Hwang, p. 17). These differences were not observed on the fourth through ninth days. Lamb and Hwang are again led to suggest that the short term effects simply disappear: there may be "a short-lived beneficial effect for some mothers" (p. 17). Interpreted in the light of Klaus and Kennell's original theory, however, this decreasing difference would be evidence of a critical period in the first three days, and the suggestion that the benefits are gone once the additional cuddling and touching is not repeated is a myopic reading of a complex biopsychological situation. Benefits are not measured solely by whether the same behavior continues, but by what difference the somatosensory affection will make later on. Some of the long term results are in other modes than touching.

Thus, as part of their supporting data, Klaus and Kennell (1976, pp. 58-59) cite the interesting ongoing work by Ringler (see Ringler, Kennell, Jarvella, Navojosky, and Klaus, 1975; Ringler, Trause, Klaus, and Kennell, 1978), which indicates in a controlled study that some of the same infants who had received the additional early contact showed a number of surprising differences at age two, and then at age five, with comparable infants who had not had that contact. Lamb and Hwang seize upon the finding that in four different tests given to the five year olds, "there were no group differences in any test" (Lamb and Hwang, p. 20). Despite this, they do acknowledge a major series of findings in these studies: there was a positive correlation between mothers' IQ in the early contact group and their children's linguistic and intellectual development on some specific measures. What the findings mean was summarized by the researchers themselves thus:

> . . . the present summary suggests that how mothers speak to their 2-year-olds is associated with the children's speech and language comprehension at 5, but only among pairs who experienced extra postpartum contact . . . . Much detailed research is still needed to document the steps through which this occurs. (Ringler et al., 1978, p. 865)

The fact that these correlations did *not* occur in the control group is a difficult matter for Lamb and Hwang to explain. They admit that they cannot (p. 14). But they also express no support for the further detailed research that Ringler et al. call for.

A study of infant-mother interaction when the early contact infants had reached the age of two (Ringler et al., 1975) has bearing on the issue of self-regulation, since it was found that the early contact mothers "issued fewer commands to their children but asked more questions" (Marano, 1981, p. 66). Lamb and Hwang make no comment on this issue, nor would their adherence to value-free science permit them to have commented. From their point of view, the linguistic development advances on the part of the early contact infants are merely one more "measure," and are probably insignificant given that on many other measures no significant results were discovered. From the perspective of the sexual body, however, the enhancement of self-regulation in children's language interactions with their mothers could be hypothetically linked to an enhancement of sexual self-regulation. Such a relationship could be investigated with suitable research design.

In another study by Kennell, Trause and Klaus (1975), interesting differences were found at the child's age of 42 months in a group of babies who had been born preterm but had received considerably more early maternal contact than had a control group. In the words of Money, who takes a favorable view of the approach, here is what the study showed:

> At 42 months, the early-contact and the late-contact babies had average IQs of 99 and 85, respectively, and there was a correlation ($r = 0.71$) between IQ and the amount of time the mothers had spent looking at their babies during a filmed feeding at age one month. (Money, 1980, pp. 168-169)

But as Lamb and Hwang describe the same findings, they come out thus:

> . . . the mothers who had been allowed into the nursery looked at their infants more while feeding them than did the mothers who had been separated. No other behavioral differences were reported, and there is no conceptual reason to interpret a difference in looking as an indication of closer bonding . . . . At 42 months, the children in the early

> contact group reportedly had higher IQs but again the lack of information about the sample and about the statistical procedures makes this finding hard to evaluate. (Lamb and Hwang, 1982, pp. 11-12)

In this instance, Lamb and Hwang have methodological reservations which I take very seriously. For one thing, the group of infants that had been separated from their mothers were *separated for three weeks*; so that what the experiment investigated was much more than the effects of early contact. By three weeks, the early-contact pairs were much more familiar with one another. And the statistical information given in the report was indeed too sketchy. To say that the IQ differences are "hard to evaluate" is an understatement; actually, in their 1982 edition, Klaus and Kennell retract that finding entirely: the IQ differences were the result of a statistical error (Klaus and Kennell, 1982, p. 46). On the other hand, to maintain that there is no "conceptual reason" to think that eye contact has anything to do with closer bonding sounds like cultivation of the traditional blind spot that cognitively oriented researchers have toward the body. Their review is simply not a responsive attitude toward results that are still most suggestive about the effects of early somatosensory contact and interaction, and which come out of a new research strategy which after all was only about five years old itself at the time of the report.

Others have made much of the fact that Klaus and Kennell's sample of mothers came from a special social group: poor and black. Kaye (1982, pp. 254-255) and Leiderman (1980) believe that the Hawthorne effect was decisive. But some of the Swedish, German, and even Guatemalan positive evidence cited by Klaus and Kennell is based on middle class mothering.

It is useless to claim that the Lamb and Hwang review is biased; it merely reflects the psychological assumptions of the authors. At the surface these are methodological, but inevitably underlying assumptions regarding human nature and "what is good for people" are also involved. Without trying to further resolve the issue of the correctness of the review article, I would at least add this: it suffers from a lack of scientific commonsense, if I may use such a term. Klaus and Kennell pointed out that their original study involved hospitalized women who had had some anesthetics and drugs during childbirth, and who did not have their babies placed in contact with them until one or two hours after birth. This would not have been the situation in home-birth delivery (Klaus and Kennell, 1976, p. 59). The authors may have been hinting that an inquiry into maternal-infant bonding should be directed at a situation involving immediate post-birth contact between mother and infant. In fact, some of the Swedish studies in which positive results were obtained did have this condition. By 1982, Klaus and Kennell were much clearer on this point, because they now had new research findings (Emde and Robinson, in press) from another area, showing that in the first hour of life the newborn is likely to experience a quiet, alert state with a capacity for viewing the environment with wide-open eyes. In the second through fourth hours, on the other hand, the newborn is likely to go into a deep sleep. The first hour seems all the more crucial in this light.

The further challenge to research is to study maternal-infant contact at its optimum, in natural childbirth delivery. This is a challenge to perform a delicate but valuable investigation without disturbing the contact. Lamb and Hwang were interested not in the inquiry but in dampening it. They end their study with warnings that the whole idea of maternal-infant bonding could be socially dangerous. It could make some parents feel guilty for not having provided this kind of birth experience; it could encourage a simplistic panacea for all the needs of the mother who comes from lower socioeconomic classes; and worst of all, it could be highly embarrassing to the professionals in the field of behavioral pediatrics (Lamb and Hwang, pp. 31-33). This theory may bring that field into "long-term disrepute." But in my reading, what is in "disrepute" is the sterile scepticism which prematurely denigrates the pioneering work of Klaus and Kennell. The other dangers referred to by Lamb and Hwang are real, but they could be avoided without throwing out the bonding theory, a theory which raises basic questions about the sexual body.

World Hypotheses Behind the Bonding Controversy

The maternal-infant bonding controversy obviously is not a purely academic one. The issue is a threatening one in itself to those whose world hypotheses have little use for a category of natural, biological value in which body contact is essential.

Some feminist scholars see the interest in bonding as evidence of a reversion to sexism (Breines and Gordon, 1983, pp. 496-499). This is part of a larger dispute within feminist thought regarding the importance of biological factors in the quality of women's lives. Alice Rossi (1977, 1981) is among those who hold that parenting is a biosocial affair, and that women have a bodily relation to mothering that cannot be swept away by changes in ideology. On the other side are Nancy Chodorow (1978), who relies primarily on the psychoanalytic theory of object relations, and many other feminist thinkers. In empirical psychological studies, bonding tends to be dismissed as an unconfirmed "emotional" theory. Eva Reich has emphasized maternal-infant bonding in her past several years of lecturing in several parts of the world, even though she is not referred to by other discussants (e.g., Sheleff, 1981, p. 230) who wish to emphasize the mother-infant relationship without considering sexuality. The bonding advocates sometimes speak as if the infant and mother will provide a revival of the pre-Freudian "innocence" that denied infantile sexuality altogether. The loss of Eva Reich's contribution to most academic and therapeutic research is especially unfortunate because she has specialized in brief therapy techniques for "re-bonding," given to mothers who voluntarily present themselves as lacking a sense of emotional contact with their very young infants (E. Reich, 1980). The potential value of such work is too basic to be ignored.

Opponents of the bonding theory may not have heard of Eva Reich, but it is harder to understand why they would often fail to cite the most comprehensive statement of the theory which they oppose. The indispensible text, available since 1976, is the book edited by Klaus and Kennell, containing within it critical comments by (among others) the pediatrics specialist, T. Berry Brazelton, an anthropologist, B. Lozoff, and one ex-president of the American Psychoanalytical Association, Albert J. Solnit. The key chapter, "Human Maternal and Paternal Behavior," is written by the two editors, but includes numerous critical comments from this panel (Klaus and Kennell, 1976, pp. 38-98). Another recent survey of the maternal-infant bonding in *Journal of Child Psychology and Psychiatry* mentions neither Prescott's overall studies of the value of affectional touching in infancy nor this key work by Klaus and Kennell in the very field being surveyed (Sluckin, Sluckin, and Sluckin, 1982). (This negative report on bonding is *also* promptly reprinted by the wife-husband research team of Chess and Thomas in their *Annual Progress* series. They never seem to find any favorable article worth inclusion; see Chess and Thomas, 1984a.) The Klaus and Kennell main volume is cited neither by Breines and Gordon, in their recent feminist rejection of bonding, nor by any of the writers in Stratton's *Psychobiology of the Newborn* (1982b), although both of these sources cite more restricted publications by Klaus and Kennell. But the book-length presentation is uniquely important for several reasons. For one, it allows scope for employment of multidisciplinary approaches to suggest an overall theory. Second, as Diesing has pointed out, a theory in the social sciences depends on "contextual validation" in which a convergence of evidence from many sides is brought to bear; some of it is "weak" evidence *if* considered in isolation from the theory (Diesing, 1971). Third, two of the strongest points raised in the feminist scholars' objections would have to be considerably modified were the more comprehensive statement taken up. In their central chapter Klaus and Kennell (1976) do *not* base their work on mammalian studies, as Breines and Gordon (1983, p. 496) have charged; the facts of "Maternal Behavior in Mammals" are treated as a separate chapter and are not referred to in the main one. Neither is the complaint that bonding is limited to the mother's role in parenting, and is hence a sexist investigation, born out by the Klaus and Kennell chapter, plainly entitled "Human Maternal and Paternal Behavior." The authors in fact define the concerns of their book as "the bond a mother or father forms with his or her newborn infant" (p. 1). As if to underscore the point, Klaus and Kennell titled the second edition of their book (1982), *Parent-Infant Bonding*. One of the reports cited by Klaus and Kennell (1976, p. 69) is by an anthropologist, J. Schreuber, who told them of *extended* bonding patterns in a small farming community in Italy: "Within 5 minutes of the birth, the parents, grandparents, and, on the average, five other relatives, will have kissed the baby" (Klaus and Kennell, 1976, p. 39). Eva Reich, whom I have heard lecture on the topic of bonding, has also made it clear that bonding is not limited to the mother (E. Reich, 1980).

All of which is not to pretend that a father is as central to the early life of the neonate as the mother. On this opinions differ. Lamb and Goldberg (1982), in a comprehensive review of research on the father-child relationship, strongly suggest that no differences are to be found between the responses of fathers and mothers to neonates. They cite two extensive and careful studies in which parental response to the baby's crying was investigated; these showed "no sex differences on measures of physiological responses *although the mothers reported more extreme emotions*" (Lamb and Goldberg, 1982, p. 65, emphasis added). The clause I have emphasized seems to me to render the intended conclusion dubious, inasmuch as emotions are tied to physiological functions: if they are "more extreme" in mothers, that *is* a difference in physiological response. Klaus and Kennell, while they do discuss the father's role in bonding, may be fundamentally correct in assuming that the bonding relationships they are investigating occur more readily between mother and infant. (In my discussion of Lichtenstein, Chapter Nine, I will come back to this topic.)

It should be evident that when I describe Klaus and Kennell in their own language their theory can be imagined as a plausible one, whereas the largely truncated descriptions of their theory in the writings of their critics makes it seem misconceived, naive, falsified, malicious, and not even worth knowing. This has ever been the case with the study of the sexual body. The issue can be stated as a struggle between those who take biology as an essential part of the human psyche and those who want biology to be out of the picture. When biology goes, the body goes, and the sexual body need never be considered. There probably is some simplification in the assumptions of those who defend the theory of maternal-infant bonding. As Rossi points out (personal communication, August 8, 1983), no one should suggest that human *survival* mechanisms are dependent upon the experience of mother-infant contact during the first hours after birth. Evolutionary process would not have fixed upon one lone normative event, so easily violated. But until researchers become more generally disposed to attend to the "natural" bodily connections in birth and infancy, the cycle of exaggerated claims and misconceived disconfirmations will go on indefinitely.

"Bonding" would seem to be a concept especially easy for its critics to dismiss as naive, because it is so obviously a metaphor, and its proponents seldom speak of it as such. So for that matter is "attachment" a metaphor, but probably it does not raise any ungainly images in the mind: we know that the baby is not literally "attached" to the mother, whereas we are not so sure that something *purely* metaphorical is being implied with the term "bonding." And indeed it is not: maternal-infant bonding is a metaphor implying connections between two bodies which are established through contact and through energy exchanges, including particularly an exchange of the emotion, love. This may account for some of the animosity of those who would like to discuss it as if it were not a metaphor at all, but a literal fact of some sort. There is a whole additional type of curiously linear argument which I will now take up. Behind the literalness, I suggest, is another instance of that "contempt for the body" which Dewey located in the history of morals (Dewey, 1934, p. 20). The assumption seems to be, if it is a body-to-body contact, it must be a simple matter. Hence, Arney (1980), Sluckin, Sluckin, and Sluckin (1982), and Chess and Thomas (1984b) continually tend to speak of bonding as if it involved nothing more complicated than putting one body next to another for a few hours. They have no difficulty "refuting" this ghost of the sexual body.

Biology, the Sexual Body, and Feminist Psychoanalytic Thinking

As support for their arguments, Gordon and Breines cite a more comprehensive treatment of the maternal-bonding issue by Arney (1980), a man. Arney has a reasonable methodological skepticism concerning the study by Ringler et al., (1975); how can we be sure that the children who had higher IQs and a higher degree of self-regulation at age five actually got those qualities from the early extra contact with their mothers during the first hours of postnatal life? Any group over a five year period would show numerous changes in other areas, such as perhaps improved relations between the parents, or special input of learning from unidentified sources. Arney also makes two more general claims, however, which display the underlying issues in a glaring light. In his world hypothesis, human beings are defined as human because they create their meanings for themselves out of their experiences; bonding, he declares, is a concept that denies human meanings because any meaning it creates is biological rather than human in nature. This division of the subject on ideological grounds rules out interest in bonding or in any other aspect of the sexual body. The very realization that maternal-infant bonding creates (or might create) meanings (relational, affective, experiential) would be exactly what is unacceptable, to this point of view, since it would make it all too evident that we do not step outside of nature to live our lives; instead we are part of nature (cf. Dewey, 1929b, p. 307).

The other major claim by Arney is even balder: that research showing the existence of maternal-infant bonding in humans not only is flawed, it must always be invalid no matter how well it is done!: "It would be impossible to conduct research that could conceivably lead to the conclusions of bonding research . . ." (Arney, 1980, p. 551). By this, he appears to mean that it would be impossible to take a group of mothers and infants, plus a control group, and follow them under conditions of airtight control, subtracting all other causes of good maternal-infant interaction, and thus replicate the benefits of bonding. You might be able to observe infants in their cribs for a few hours after birth, in other words, under conditions of stringent control, but never a live relationship between infant and mother over a five year period in normal social conditions.

The problem here, finally, is one of intelligence, in Dewey's sense. If people are ever to make intelligent decisions, such as what sort of childbirth practices to adopt, they cannot be expected to ignore all indications that appear to give weight to the metaphor of the maternal-infant "bond" simply because there could never be fully conclusive evidence for it. The actual choice would be made not in terms of conclusive/inconclusive evidence, but in respect to what alternatives available for child-rearing appear to be the most sane and healthy: the value judgments of such terms cannot be avoided. Arney's approach to the problem of maternal-infant bonding is thus to obstruct rather than face it.

The proponents of the "sensitive period" that occurs shortly after birth may eventually come to be discredited; if such reviewers as Lamb and Hwang, Sluckin, Sluckin, and Sluckin, and Gordon and Breines, are to be taken seriously, the idea of such bonding already has been discredited. Nonetheless there is still no reply to Prescott's simple formulation of the issue:

> No other mammal except the human mammal separates its newborn from its mother at birth which prevents immediate sucking at the breast. These departures from mammalian universal behavior can only be considered abnormal. (Prescott, 1979, p. 100)

In Freud's original classic period, there would have been no reason to deny this. Today, however, the sexual body is usually kept out of sight in psychoanalytic thought.

Nancy Chodorow's work, and her controversy with the non-psychoanalytic feminist sociologist, Alice Rossi, forms a significant confrontation between an approach which denies the significance of biological considerations and one which takes the sexual body seriously. Chodorow's book, *The Reproduction of Mothering: Psychoanalysis and the Sociology of Gender* (1978), is a major feminist reinterpretation of psychoanalytic thought. This also contains no reference to the work of Klaus and Kennell, although it takes a strongly negative view of the alleged evidence for maternal instinctive behavior. The author argues, in the context of a detailed reconsideration of psychoanalytic object relations theory, that this theory provides convincing support for the position that "motherhood" is not instinctual, but a sociologically induced gender-role combination brought about by society. Nor is the mothering role a neutral one: the very preponderance of women in the role of mothers assures that both men and women will have developed as infants much stronger early "object relations" with mothers, not fathers. They will create incurably ambivalent hate-love objects in their mothers; indeed it is likely that there will be more hate than love, given the denial of the value of woman that is built into the mothering role. The cycle will reproduce itself indefinitely, unless the one basic change that could break the cycle is introduced: the equal sharing of "primary," i.e., close, early, nurturing, parenting (mothering), by men and women.

When confronted with objections from Alice Rossi that there are very likely biological considerations that make the mothering role primarily that of the natural mother, Chodorow gave skeptical replies. Thus to Rossi's data showing that the infant's cry produces an increase of oxytocin in the mother, which stimulates nursing, uterine healing and erotic contractions after childbirth, Chodorow countered that Rossi had not shown that oxytocin stimulation had to do with anything more than lactation itself; who could say that it had any connection with bonding? Besides, was it not possible that non-lactating women produce oxytocin too? It might be supposed that this is an irrelevant answer, since Rossi's point concerned the psychophysiological interaction of a nursing mother with the voice of her infant, and then with a nipple/mouth contact, whereas other women who were not nursing, although they might produce the chemical, could not experience the nursing cycle. And, Chodorow advises, do not forget men: maybe they have an oxytocin level comparable to that of a nursing mother, for all we know (Chodorow, 1978, p. 18-19). Actually, there is some reason to believe that oxytocin is produced in the male body as part of the orgasmic cycle (Davidson, 1980, pp. 308-309), but again, the mere production of the substance is not the issue so far as the sexual body interaction of caretaker and child is concerned. Chodorow is trying to reduce the bonding metaphor to a set of literal components.

On the basis of such reasoning Chodorow urged that Rossi's arguments be dismissed. However, in an exchange with Rossi a few years later, Chodorow began to change her outlook toward the relevance of biological and evolutionary evidence:

> I have been convinced by Rossi's argument that I and other feminists must be open to the investigation of biological variables and that those who argued or implied that such investigation is illegitimate were wrong. We are embodied creatures, and our experience as people of either sex includes this embodiment. There is certainly some biological basis, or influence, to some sex- and gender-linked experiences, though this biology does not dichotomize the human population perfectly, nor do all men and women have the same reproductive or sexual experiences. (Chodorow, 1981, p. 507)

It is to Chodorow's credit that she could make this shift. Not surprisingly, her work continues to be known and praised for its refusal to regard biological factors as important in mothering (Breines and Gordon, 1983, p. 498). The reconsideration just quoted may turn out to have no effect, particularly since it was followed at once with Chodorow's reindorsement of psychoanalytic object relations theory, for "its rich account of how we come to make something psychologically" of our embodiment, "which includes our sexuality, creating in the process important parts of our personality and our emotional life" (Chodorow, 1981, p. 507). Now I maintain that this praise for object relations is both misplaced and misconceived. It is misplaced because the body hatred of Melanie Klein and even of Winnicott infused their theories with a negative evaluation of the sexual body; it is misconceived because of the mind-over-body bias built into the formulation. What could be implied, after all, by the phrase "how we come to make something psychologically" of our sexuality, but another disparagement of the sexual, except as it may serve to furnish material for "higher" things? The steps up Plato's ladder toward the ideal await any such formulation as Chodorow's. From an interdisciplinary standpoint, and from the perspective of the present study, it appears that Chodorow has acknowledged that in her book she was only able to argue the noninstinctual, nonbiological nature of mothering by avoiding rather than confronting the relevant disciplines. She was enabled to do this by virtue of her reliance on the very psychoanalytic thought which she continues to regard as the discipline of choice for understanding the sexual subordination of women. On the basis of that theory, she believes, for example, that the young infant has an "absolute physiological and psychological dependence, and . . . total lack of development of its adaptive ego functions . . ." (Chodorow, 1978, p. 83). From this position it would not be possible for her to adapt to the findings of recent infant research which indicate in many ways that the belief is false.

The Survival of Maternal-Infant Bonding and the Sexual Body

It bears repeating that mother-infant interactions are sexual interactions, if psychoanalytic theory is accepted at all. What the dimensions of this sexuality might be is a question permanently occluded by avoiding the problems of contact, uterine contractions, hormonal changes in pregnancy, body energy considerations, infantile sexuality and the woman's sexual life as an adult. One major impetus of the upsurge in interest in maternal-infant bonding has been to begin to refocus on all these considerations within the earliest hours and days of neonatal life. The eye-contact between newborn and mother, as observed by students of bonding,

> is not very different from what happens when a man and woman fall in love: mutual gazing, touching, fondling, nuzzling, kissing. (Marano, 1981, p. 65)

One can simply declare such observations a "category mistake." But, as Pepper used to ask, "on whose categories?" If that shortcut is not taken, we are left with an overlap of the languages of adult-adult and adult-infant love, comparable to the original creative confusion caused by Freud's use of the term "sexuality." The sex therapist Offit (1977) is the only writer, to my knowledge, to report that sexual stimulation of mothers by their babies is a serious *problem* for adult relationships in some contexts:

> Caring for babies is at once a source of immense maternal satisfaction, a huge erotic stimulus, and a tedious drudgery . . . . It becomes routine to shut off the sexual arousal afforded by intimate body contact all day long. Many mothers masturbate regularly at their children's naptime to relieve the sexual tension generated, although they are not aware of their children's role in nourishing it. This leaves them less interested at night. (Offit, 1977, p. 155)

Offit lacks the Reichian distinction, here, of adult genital gratification which would make a great difference in defusing such arousal, and in preventing sexual energy from becoming directed toward incestuous fantasy. Nonetheless, she has described a little-known problem about the adult sexual body in its relations with infants. It sheds a startling light, I think, on reports that men are "jealous" of their own babies, for the attention that mothers give those babies. It would appear that in some instances there would be a sexual body reason for this, and not a mere disturbance through the common male fantasy, in our society, of possessing the woman. To know that maternal-infant contact could produce such a pattern of arousal and discharge may throw light on Chodorow's discomfort with the uncritical adulation of the "intensity" of mothering the young infant, expressed by male psychoanalytic theorists (Chodorow, 1978, p. 87).

Although the psychobiology of the dynamic maternal-infant interaction is a matter of obvious, overriding interest in infant research, the "army of observers and researchers" (Roiphe and Galenson, 1981, p. x) has been far from responsive to some of the most basic and obvious needs for knowledge in this field. The following comment, from a review of research on the nutrition and feeding of the newborn is fairly astonishing:

> Apart from a few workers such as Richards and Bernal, there has been little interest in the study of the naturally occuring infant and mother feeding behaviours . . . . Perhaps even more surprisingly, rarely has a comparison been made between breast and bottle feeding beyond the first week of the infant's life, and certainly not to explore the potential precursors of obesity. This is particularly surprising since breast and bottle feeding are so different and could provide very different (learning) experiences according to technique. (Wright and Crow, 1982, p. 341)

Wright and Crow go on to report a most interesting comparative longitudinal study of their own on breast and bottle feeding, with infants from four days up to six months of age. The results show a startling contrast in feeding patterns for the two groups. As the authors point out, "Interpretation of such data is not easy" (p. 352). It would be much more feasible to evaluate it if the infant research disciplines had not already created an asexual theoretical and experimental context for such studies. It remains to be seen if the fine research start and brilliant initiative of these two researchers will find any echo in the discipline. Unfortunately, Wright and Crow do not cite (and probably are unaware of) Jean Ritter's much earlier study of self-regulation in breastfeeding, discussed above (Chapter Four). Their study, excellent as it is, is narrowly focused on nutrition and feeding, with little psychological inquiry except toward the cognitive dimension, that is, to learning. The sexual body, so near at hand, is not considered.

Nor is it considered in Kaye's excellent research into the social significance of biological mother-infant feeding interactions. Kaye (1982), whose book received such a warm review from Bruner, deserves much praise for his discovery that the human infant feeds in a rhythmic pattern of "bursts" of sucking, followed by a pause, and that during the pause, the mother intuitively jiggles the infant, causing a delay in the onset of the next "burst" of sucks. This pattern is not found in other primates. Nor is the jiggling apparently a culturally determined behavior. Kaye reasonably speculates that the mothers intuitively convert the biologically given burst-pause cycle into a precursor of mother-infant taking of turns, and thus into an early social interchange. Kaye did not recognize the sexual dimensions of sucking (possibly he would deny these exist); he also did not consider the variable *quality* (not just the fact) of body-to-body contact between mother and infant, as a factor in creating the variable quality of social interchange. The jiggling can convey disruption, character stiffening, a tone of hurrying-up, or it can suggest affectionate contact and ways of dealing with people.

On the Sexual Body of Neonates

Infantile genitality, unlike the problems discussed in the past several sections, is not a research area producing vital controversies. However, this is primarily because of avoidance, not because there is no pioneering experimental observation which might have had the effect on this field that Fantz' methodology had on the study of neonatal visual perception. Here I would like to refer to some of the findings of Peter H. Wolff. Wolff, like Fantz, is credited with a key role in originating the great infant research "explosion" (Stone, Smith, and Murphy, 1973, p. 6), and like Fantz, his methodology was surprisingly commonsensical: he spent long hours, sometimes as many as 18 consecutive hours (Stone et al., p. 239) in careful observation of the infant's "state," such as whether the infant was awake or asleep, agitated or quietly alert. Wolff found that infant boys of as young as four days have cycles of penile erections while they sleep; these cycles coincide with their Rapid Eye Movement during sleep, which are known to be a major time of dreaming in children and adults (Wolff, 1966). Wolff's observations were accompanied by suggestions of a theoretical nature that indicate the ease with which complicated bio-psychic rhythms may be disrupted, and how substitute behavior may be overlaid on top of the same neurobiological channels:

> Fourth day, 25 minutes of regular sleep. The record begins immediately after the infant has voided. During the first 12 minutes he has seven erections, each lasting about one minute, and separated on the average by about 30-second intervals. On three occasions while the penis is erect a jar to the crib elicits a vigorous extensor startle followed by an immediate complete detumescence that persists for one and a half minutes (in contrast to the usual interval between erections, which never exceeds 39 seconds). When the crib is no longer jarred, erections resume; now each erection lasts about two minutes, and sequential erections are separated by approximately 20-second intervals . . . . This record indicates that spontaneous erections may occur immediately after urination and therefore independent of bladder distention; that erections, mouthing movements, and spontaneous startles can substitute for each other; that elicited startles can inhibit spontaneous discharges; and that mouthing movements and erections have their own specific rhythms. (Wolff, 1966, pp. 22-23)

It is entirely likely that most psychoanalytic observations on the infant have been made without awareness of this material, or at least without any consideration of what it may mean, although the raw fact that baby boys get erections was certainly known and referred to in the early Vienna years. Not surprisingly, given the cultural predisposition I have alluded to, it then dropped out of theoretical and indeed clinical or even experimental notice for a very long time.

The psychoanalyst Phyllis Greenacre (1952), however, did deal with the topic, in 1952. She even cited studies of infantile erection in the psychological literature dating back to 1917 (Martinson, 1981b, p. 59, has unearthed a French study published in 1883!). Greenacre's major source for her own psychoanalytic considerations was a large, careful study by H.M. Halverson, a Yale psychologist (Halverson, 1938, 1940). For Greenacre, Halverson provided evidence of the infant's innate tendency toward anxiety. Unfortunately Halverson's study was an almost perfect instance of the experimental artifact. As Martinson (1981b) has argued, Halverson's finding that penile erection did not occur while the neonatal boy was sucking at the breast could have come out of the experimental set-up itself, which

> served to deter from a full pleasurable response—apparently no caressing or fondling by the mother, no eye-to-eye contact, no opportunity for the infant to touch the mother's face, or to place its fingers in her mouth existed. The question left unanswered is how many of those infant boys would have responded with penile erections under normal nursing and cuddling conditions. (Martinson, 1981b, p. 65)

In other words the crucial missing variable once again is Prescott's somatosensory affectional contact. Martinson, whose critique of the Yale experiment I have just cited, is one of the handful of social scientists who realizes Prescott's importance (Martinson, 1981b, p. 72). Greenacre, writing in 1952, could not have known either Prescott or the findings of Wolff. Nonetheless, it is significant for the fate of the sexual body, in theory and research, that Halverson's anxious, upset neonatal boys with penile erections could figure in the psychoanalytic literature, whereas the later article (Wolff, 1966, where the boys are functioning in accordance with a natural rhythm of the sexual body) never seemed to have had an impact. With Wolff's much respected work, however, the topic has been set forth indelibly for further investigation.

Yet interest in it has been slight. Anneliese Korner's study published in 1969 is the only notable exception, to my knowledge. Her sample consisted of 32 infants, 17 of them male, at 45 to 88 hours after birth, all of them bottle-fed Caucasians. The study called for observations not only of erections, but of neonatal startles, smiles and "reflex sucks." Some of Korner's conclusions are of great interest for anyone maintaining (as I am) that sexuality is especially problematical and not yet understood: "The data . . . suggest that erections are spontaneous behaviors which occur over and above other discharge behaviors" (Korner, 1969, p. 1048). Observer agreement as to whether an infant did or did not have an erection was 100% (p. 1044). There was "a highly significant [statistical] relation between the occurrence of erections and the infant's state." As in Wolff's study, the erections "most commonly occurred during irregular sleep, and during that state the large majority are seen while the infant is having rapid eye movement" (p. 1047). However, "Erections were also noted during waking states, particularly during crying. These were brief and in a sense not entirely spontaneous, since the proprioceptive feedback from the diffuse activity associated with crying undoubtedly served as a stimulus" (p. 1047). Unlike evidence for adult males, Korner's data did not indicate anything like a 95% correlation of REM sleep periods and erections, but as she pointed out, the findings "nevertheless demonstrate the beginnings of such an association" (p. 1047). The observations suggest possibilities for further research, and the distinctions Korner employed, such as between spontaneous and non-spontaneous discharge, were most interesting ones. Yet little happened: no research boom followed on infant genitality, although Korner herself suggested one of the most obvious questions: could it be that females as well as males have "an additional discharge channel" that complements the erections of the neonatal boys? (p. 1048). Korner continued to do extensive research herself on newborns, but by 1977, she was emphasizing the female's greater cutaneous receptivity and oral behavior, such as reflex smiles and rhythmical mouthing as a differentiation from the male, who tended toward more total body movement (Korner, 1977). She found that "the overall rate of spontaneous discharge behaviors was almost identical for males and females *when erections were excluded* . . ." (p. 13, emphasis added), but said nothing about what role these erections could have.

The first hint I found of a positive answer to Korner's initial question—as to whether little girls might have "an additional discharge channel" to match the male infantile erection—appeared in a book by Mary Calderone and Eric

Johnson, *The Family Book About Sexuality* (1981). What they say is that "a girl baby every few hours shows sign of fluid in her vagina, similar to the lubricating fluid that will be produced later when she is sexually aroused." This rhythmic occurrence begins "soon after birth . . ." (p. 18). This still does not tell us whether the cycles are correlated with anything else, such as REM dreaming.

Let me acknowledge that in this instance I am giving the perspective of the sexual body the benefit of a doubt. In fact, the *evidence* for Calderone's assertions regarding the girl neonate's rhythmic vaginal lubrication is far from persuasive. It stems from a report given by a Norwegian researcher at the Montreal conference on childhood sexuality in 1979, and that report is quite lacking in experimental details (Langfeldt, 1980). Langfeldt has given further details in another article, but his findings must be considered preliminary, rather than conclusive (Langfeldt, in press). The perspective of the sexual body, however, would impel research to continue on this finding, and even to presume that a basis for it will emerge. The fact that only an informal report at a specialized conference on child sexuality is the source of the data thus far is a problem, but it is also a reflection of the low priority given to the sexual body by researchers. As for the human neonatal male erection, Calderone and Dr. James W. Ramey now report that there is good evidence that it begins during prenatal life (Calderone and Ramey, 1983).

Unfortunately we are in no position to begin to understand our data concerning infantile genital sexuality because we have developed a culture of science dedicated for the most part to investigating a nonexistent organism, the asexual infant. When research threatens to bring results that are not evidence for asexuality, little is heard of it. A study of newborn behavior during the first two days after birth (Phillips, King, and DuBois, 1978) came up with the results that newborn boys are indeed a good deal more active than girls. Judged by two observers who did not know the sex of any of the infants, the boys had a higher "total activity score," that is, the boys had higher levels of wakefulness, facial grimacing, and various kinds of body movements such as movement of hands and feet than did girls. The study was carefully controlled, and did not include any boys who had been circumcised. The presence of "experimenter effect" here is made very dubious, especially in view of Phillips'—the chief investigator's—own statement that she expected *not* to find these sex differences (Phillips, quoted in "Researcher Finds," 1978). The two graduate students in psychology who did the rating of the infants were women: this was not a case of men finding what they wanted to see. Phillips' report appears to conflict with the interpretation of Korner, based on earlier extensive observations of newborns, in which no differences in activity levels were found between boys and girls (Korner, 1977). Although the authors call for further research into sex differences at birth, I am unaware of any follow-up. Clearly, some would find the results not pleasing for ideological reasons, although, as the authors point out, a high level of spontaneous bodily activity is not necessarily an advantage in life; it may in fact be associated with the high incidence of "hyperactivity" in males (Werry, 1968). And no one seems to be very interested in evidence by Friedman, Bruno, and Vietze (1974), showing that two week old girls are better learners in some respects than are boys of the same age: they respond better to "discrepancy" (novelty). To further such investigations would be to compromise the asexual portrait of infancy now being created in many research efforts.

Similarly, there seems to be no interest in following up Brackbill's finding that circumcision of the neonatal boy (average age, 37 hours) has an effect on the boy's response to *auditory stimulation*. Recently circumcised boys showed a statistically significant difference in heartrate from that of the uncircumcised boys or from that of the girls (Brackbill, 1975). Such a finding could be a clue, however minor, to the little understood psychophysiology of the infantile sexual body. The article is not so much as mentioned in Stratton's comprehensive review volume of the *Psychobiology of the Human Newborn*, although several other items by Brackbill are cited. Circumcision—a matter if there ever was one, of the sexual body—is not worth discussing for those who see the study of the newborn as ultimately the study of how the newborn acquires knowledge, rather than of how the sexual body lives through the early months of life.

If we consult the psychoanalytic literature even of the present day, however, we find the sexual body at least some of the time. Roiphe and Galenson (1981) argue unabashedly on the basis of much clinical evidence for the presence in girls of (not culturally caused) penis envy. They make the by-now-rare observation that "Very early genital zone experiences during the first 16 months of life contribute to a vague sense of genital awareness and undoubtedly exert an influence over many ego functions" (p. 284). This, as they point out, is a position consistent with the early Freud. Somewhere in the non-psychoanalytic literature there must be—or should be—a consideration of this genital awareness in the very youngest humans. Somewhere there is a researcher or theorist who will discuss it for us without involving us in the doctrine of penis envy. But where?

Infancy and Mind-Body Unity

There remains the possibility that the two models, one of a cognitively competent infant and the other of an emotionally intact, sexual infant (stemming respectively from recent infant research and from the Reichian branch of psychoanalytic thought), will be combined into a single "object of knowledge" (Dewey, 1929b). Within psychoanalytic tradition there has been the related concept of the "undifferentiated" infantile psyche, a term referring to the alleged inability of the neonate to have thoughts in any sense recognizable by the civilized adult. Indeed the neonate is not even credited with being able to know that its mother is not an extension of its own body. That aspect of undifferentiation may be set aside, at some point in the future, under the weight of findings which indicate that the neonate indeed knows that difference. A strict reading of empirical evidence about babies would not support the notion of an undifferentiated state, and Peterfreund, one of the theorists seeking a reform in psychoanalytic thought, has thrown the idea on the trash-heap (Peterfreund, 1978). I myself have reconsidered doing the same, in the years since I strongly endorsed the undifferentiated state concept (Efron, 1973), not foreseeing at the time that a great deal of evidence would soon make it doubtful. There may be something not only salvageable but vital in the concept of undifferentiation, however, which would make simply discarding it unwise. Viewing the infant as one whose feeling and thinking capacities include the sexual and the cognitive, without the benefit or burden of having verbal levels to distinguish one from the other, is at least one way for us to *imagine* that in human life, mind and body are not split. I think that is what the unorthodox analyst, Ernst Schachtel was telling us in his great book, *Metamorphosis* (1959). Perhaps that is why the book was never reviewed, so far as I can determine, in any of the psychoanalytic journals. If there is a period in the development of each baby in which mind-body unity is a fact rather than a wish, that would be a major qualifier for my own speculative theory of a "residual asymmetrical dualism" as the human condition (Efron, 1980). It would also, more importantly, be part of the life history of each of us, which we could then wonder about and use to reconsider our experiences.

Research orientations in this matter of the neonate's sexual body are beset by an either/or proposition that seems to be implicit in our thinking as civilized adults. We tend either to suppose that the neonate is born with a complete set of emotions and cognitions as Reich finally thought (Placzek, 1981, p. 118)—*or* that lacking this complete set, the neonate is not a complete human being. Karl Popper has seriously urged on us the consideration that a baby cannot measure up to Immanuel Kant's criteria of a person:

> "A person is a subject that is responsible for his actions," and "A person is something that is conscious, at different times, of the numerical identity of its self." (quoted by Popper, in Popper and Eccles, 1977, p. 115)

I suspect it is in reaction to this strong cultural tradition of defining the person in this way that Carroll Izard has theorized that a very young infant has the whole set of emotions of the adult, including those of guilt and shame (as discussed in Cicchetti and Pogge-Hesse, 1981, pp. 249-258). We are trapped so long as we imagine that the newborn must either be completely equipped with emotions and cognition, or else no person exists. As long as the neonate has enough sense to know pleasure, pain, and what Izard was finally constrained to postulate as "a bodily sense of me" (quoted by Cicchetti and

Pogge-Hesse, 1981, p. 254), then he or she is a person, and that implies being born into selfhood. What we are learning about the infantile sexual body makes this a reasonable position to take.

But that position would not be accepted by the one psychoanalyst who has attempted to integrate the research findings on neonates over the past 20 years with psychoanalytic theory (Lichtenberg, 1983a). In *Psychoanalysis and Infant Research* (1983a), Joseph Lichtenberg concedes that there must now be modifications in psychoanalytic theory, but ends by avoiding the issues, insofar as the issues are of the sexual body. His theory of affects, for example, endorses a greater positive role for affect in infant development than psychoanalytic theory had been inclined to allow, but dismisses the possibility that there is any sexual energy, in Freud's sense of libido, involved in such development (p. 170). Again, my point is not to deny that there are difficulties with the libido theory, but to underscore the tendency to "revise" psychoanalytic theory so that it becomes asexual. Lichtenberg's general orientation is toward a psychology of the self, in which affects contribute to cognitive integration and self-esteem, but it is difficult to see why the "erotogenic zones and early libidinal phases" of classical theory must therefore "give way" to an "undifferentiated," that is, nonsexual, ego functioning (Lichtenberg, 1983a, p. 182). Lichtenberg avoids crediting the research "explosions" with fundamentally altering the psychoanalytic model of infantile life by means of a simple defense mechanism: although the infant can do all sorts of things we had not realized before 1960, when we were still in the days of the passive imperceptive infant, all of those things are done on a basis of "preprogrammed" response (p. 162). Only the infant who has advanced to the age of about 18 months can be credited with genuinely *human* abilities, as Lichtenberg in effect defines them, for only then do we find an organism capable of mental "representation and symbolic formation" (p. 165). Needless to say, this conclusion is Lichtenberg's own interpretation of a mass of evidence regarding the competence of neonates and younger infants, and could surely be disputed. However, the real issue is whether the infant can be said to *know* pleasure and pain, through a bodily sense of itself. Lichtenberg obviously does not think so: because of the "nonsymbolic nature of the infant's mind" (p. 173), it can hardly be said to know anything. It merely performs according to preprogrammed "action patterns," even when it interacts with its mother or other caretaker. "For the infant, affects are felt; for the parents, affects are both felt and labelled" (p. 173). The flatness of this distinction hardly requires rebuttal; taken as it is phrased, it implies that the neonate cannot identify a pleasurable experience with sufficient clarity (that is, with a "label") to attempt to repeat it, nor could the neonate learn to locate any thing or person that is causing it pain.

Lichtenberg in fact is aware that there is ample evidence showing a capacity in the neonate for distinguishing pleasure from pain, but insists that this "differentiation" has nothing in common with any "symbolic process" (p.

58); for him, such a process would imply "a level of organization definitively different from that of the biological-neurophysiological-behavioral level found in the first year" (p. 58). Establishing the allegedly "definitively different" level of the second half of the second year, however, proves to be a taxing exercise in semantics, leading Lichtenberg into further hyphenized neologisms. Thus, the infant has a "sense of the self-as-a-whole," which "emerges" in the second year, but it already has a "self-in-action" prior to that point in the developmental timetable (p. 114). The "unity of the sense of self" is built up during first-year experiences of pleasure and "unpleasure" (pp. 124-125), but this unity of self does not count, somehow, as evidence for the existence of "the person as a total entity" (p. 146). In fact, the "sense of self" comes later than the neonatal stage, as Lichtenberg proposes in another place in his text (p. 30). The reason for these unclear distinctions is probably to be found in Lichtenberg's world hypothesis, which I infer requires a category of pure mind in order to qualify any human sexual body as a human being: if experience can be "manipulated intracerebrally and interpersonally" (p. 109), then it would qualify as evidence for the self as a "whole" and would reflect the "total entity of the person" (p. 109). On the other hand, whenever an infant seems to be responding to its own situation in an immediate, feelingful manner, without the distancing effect of a cerebral "objective" sense of itself viewed as if by an "outsider" (p. 145), then the infant's perceptual system has "collapsed" into the earlier developmental form of organization. The term "collapse" (or "collapsing," "collapses") is used repeatedly, and with no apparent awareness of its negative connotation, to describe the psychological processes which go on in the neonate's mind (pp. 54, 134, 135, 137).

Despite expressing admiration for the research of the past 20 years which has shown that the neonate is far from the passive, imperceptive artifact manufactured by earlier theories, Lichtenberg plainly does not think the neonate should be credited with having much of a mind. He believes that it will take the infant "several years" *after* reaching the threshold of symbolic representation to learn to "differentiate genital sensations from bowel, bladder, and perineal sensations" (p. 144). Once again, psychoanalytic theory is reverting, in this formulation by Lichtenberg, to the construct of a baby unable to perceive its own body accurately.

Obviously an infant so handicapped by its very state could not be trusted to provide any standard for its own self-regulation. Lichtenberg in fact discusses the need for "human interrelatedness and self-regulation" (p. 205), but when referring to the neonate, he can only imagine "regulation" through the caretaker's provision of "a background of successful overall regulatory effects" (p. 178). It does not seem to occur to Lichtenberg that the "preprogrammed" capacities he dismisses as unimportant for the consideration of the notion of self, are precisely the sexual body aspects of the neonate's existence, and are the potential basis for a genuine self-regulation based on the sexual body. Unwittingly, Lichtenberg has recreated the mind-body split, with the body consigned to the dumb world of the "nonsymbolic," and the mind once again raised to the level of that which is truly human. The early stage of neonatal behavior, which he labels as "preadaptedness," can only become respectable and human when it is transformed into "a form of adaptedness that is psychoanalytically meaningful" (p. 44). Although he concedes that psychoanalytic theory has perhaps overestimated the mind's freedom from "our animate and inanimate surround" (p. 35), Lichtenberg is intent on building his model of the infant on the "intrapsychic representational world" (p. 35) of the mind. In that mentalized world, the psyche is largely independent of bodily functioning; the psyche governs somatic experience but is not based upon it, nor is it obliged to allow any gratification of sexuality. The absence in Lichtenberg's model of a theory of sexuality assists in eliminating the Freudian challenge of the sexual body.

Lichtenberg's book avoids the necessary confrontation between psychoanalysis and infant research which it purports to bring about. By soft-pedalling the issue (e.g., calling the demise of the validity of the helpless, symbiotically dependent model of the neonate a "new twist to our view of object relations," p. 17), Lichtenberg perhaps has hoped not to alarm the psychoanalytic establishment. But a real confrontation may occur in the future, and it may cause the kind of shift in psychoanalytic theory which would once again bring it back to a functioning awareness of the sexual body. At this point, it also remains to be seen whether the new wave of research that has begun to emphasize rather than avoid the emotional life of infants (Goleman, 1984c) will be capable of encountering the sexual body in the object of knowledge which it will create, and in the "self" or "person" it will construct. If one asks *why* bother with a definition for the infant of the self, or of the person, the answer might be that not to attempt such an act of "naming" (Dewey and Bentley, 1949, pp. 154-167) is socially harmful. As Pepper pointed out long ago in his argument about the nature of definitions (Pepper, 1946), definitions can have pernicious social consequences when they conceal certain value judgments. The judgment that a newborn is without a self or is not yet a person has helped to reinforce all those authoritarian assumptions regarding child-rearing which come into play when a human being is considered radically incomplete until he or she acquires a central core of personal identity that only the family can supply. There is no longer any reason to suppose that psychological science supports such a judgment. Probably there never was. Kant's specification that a self must take responsibility for its actions tells us that Kant thought very highly of the ideal of responsibility, but that ideal should not be imposed upon human infants as they are perceived by adults.