LECTURE 2

The origins of attachment theory

In the spring of 1981 the American Orthopsychiatric Association invited me to New York to receive the Fourth Blanche Ittleson Award and to address members of the Association on the history of my work in the field of attachment and loss. After thanking members for the honour they were doing me, I also took the opportunity to express my deep gratitude to the three American foundations, the Josiah Macy Junior, the Ford, and the Foundations Fund for Research in Psychiatry, which had supported our work at the Tavistock Clinic during the critical decade starting in 1953.

After the meeting the editor of the Association's journal asked me to expand my remarks by giving an account of what we knew at that time in the field I have been exploring, how we arrived at that knowledge, and the directions which further research should take. In reply I explained that I was in no position to be an objective historian in a field that had for long been controversial and that all I could attempt was to describe the story as I recalled it and to point to a few of the empirical studies and theoretical ideas that had been influential in shaping it. My personal biases, I explained, would inevitably be everywhere evident.

During the nineteen-thirties and forties a number of clinicians on both sides of the Atlantic, mostly working independently of each other, were making observations of the ill effects on personality development of prolonged institutional care and/or

frequent changes of mother-figure during the early years of life. Influential publications followed. Listing authors in alphabetical order of surname, these include the following: Lauretta Bender (Bender and Yarnell 1941, Bender 1947), John Bowlby (1940, 1944), Dorothy Burlingham and Anna Freud (1942, 1944), William Goldfarb (1943 a, b, and c and six other papers, summarized 1955), David Levy (1937), and René Spitz (1945, 1946). Since each of the authors was a qualified analyst (except for Goldfarb who trained later), it is no surprise that the findings created little stir outside analytical circles.

At that point, late 1949, an imaginative young British psychiatrist, analytically oriented and recently appointed to be Chief of the Mental Health Section of the World Health Organisation, stepped in. Requested to contribute to a United Nations study of the needs of homeless children, Ronald Hargreaves* decided to appoint a short-term consultant to report on the mental health aspects of the problem and, knowing of my interest in the field, invited me to undertake the task. For me this was a golden opportunity. After five years as an army psychiatrist, I had returned to child psychiatry determined to explore further the problems I had begun working on before the war; and I had already appointed as my first research assistant James Robertson, a newly qualified psychiatric social worker who had worked with Anna Freud in the Hampstead Nurseries during the war.

The six months I spent with the World Health Organisation in 1950 gave me the chance not only to read the literature and to discuss it with the authors, but also to meet many others in Europe and the United States with experience of the field. Soon after the end of my contract I submitted my report, which was published early in 1951 as a WHO monograph entitled *Maternal Care and Mental Health*. In it I reviewed the far from negligible evidence then available regarding the adverse influences on personality development of inadequate maternal care during early childhood, called attention to the acute distress of young children who find themselves separated from those they know

* Ronald Hargreaves's premature death in 1962, when professor of psychiatry at Leeds, was a grievous loss to preventive psychiatry.

and love, and made recommendations of how best to avoid, or at least mitigate, the short- and long-term ill effects. During the next few years this report was translated into a dozen other languages and appeared also in a cheap abridged edition in English.

Influential though the written word may often be, it has nothing like the emotional impact of a movie. Throughout the nineteen-fifties René Spitz's early film *Grief: A Peril in Infancy* (1947), and James Robertson's *A Two-Year-Old Goes to Hospital* (1952) together had an enormous influence. Not only did they draw the attention of professional workers to the immediate distress and anxiety of young children in an institutional setting but they proved powerful instruments for promoting changes in practice. In this field Robertson was to play a leading part 'e.g. Robertson 1958, 1970).

Although by the end of the nineteen-fifties a great many of those working in child psychiatry and psychology and in social work, and some also of those in paediatrics and sick children's nursing, had accepted the research findings and were implementing change, the sharp controversy aroused by the early publications and films continued. Psychiatrists trained in traditional psychiatry and psychologists who adopted a learning-theory approach never ceased to point to the deficiencies of the evidence and to the lack of an adequate explanation of how the types of experience implicated could have the effects on personality development claimed. Many psychoanalysts, in addition, especially those whose theory focused on the role of fantasy in psychopathology to the relative exclusion of the influence of real life events, remained unconvinced and sometimes very critical. Meanwhile, research continued. For example, at Yale Sally Provence and Rose Lipton were making a systematic study of institutionalized infants in which they compared their development with that of infants living in a family (Provence and Lipton 1962). At the Tavistock members of my small research group were active collecting further data on the short-term effects on a young child of being in the care of strange people in a strange place for weeks and sometimes months at a time (see especially the studies by Christoph Heinicke (1956) and, with Ilse Westheimer, (1966)),

whilst I addressed myself to the theoretical problems posed by our data.

Meanwhile the field was changing. One important influence was the publication in 1963 by the World Health Organisation of a collection of articles in which the manifold effects of the various types of experience covered by the term 'deprivation of maternal care' were reassessed. Of the six articles, by far the most comprehensive was by my colleague Mary Ainsworth (1962). In it she not only reviewed the extensive and diverse evidence and considered the many issues that had given rise to controversy but also identified a large number of problems requiring further research.

A second important influence was the publication, beginning during the late fifties, of Harry Harlow's studies of the effects of maternal deprivation on rhesus monkeys; and once again film played a big part. Harlow's work in the United States had been stimulated by Spitz's reports. In the United Kingdom complementary studies by Robert Hinde had been stimulated by our work at the Tavistock. For the next decade a stream of experimental results from those two scientists (see summaries in Harlow and Harlow 1965 and Hinde and Spencer-Booth 1971), coming on top of the Ainsworth review, undermined the opposition. Thereafter nothing more was heard of the inherent implausibility of our hypotheses; and criticism became more constructive.

Much, of course, remained uncertain. Even if the reality of short-term distress and behavioural disturbance is granted, what evidence is there, it was asked, that the ill effects can persist? What features of the experience, or combination of features, are responsible for the distress? And, should it prove true that in some cases ill effects do persist, how is that to be accounted for? How does it happen that some children seem to come through very unfavourable experiences relatively unharmed? How important is it that a child should be cared for most of the time by one principal caregiver? In less developed societies it was claimed (wrongly as it turns out) that multiple mothering is not uncommon. In addition to all these legitimate questions, moreover, there were misunderstandings. Some supposed that advocates of the view that a child should be cared

for most of the time by a principal mother-figure held that that had to be the child's natural mother – the so-called blood-tie theory. Others supposed that, in advocating that a child should 'experience a warm intimate and continuous relationship with his mother (or permanent mother-substitute)', proponents were prescribing a regime in which a mother had to care for her child twenty-four hours a day, day in and day out, with no respite. In a field in which strong feelings are aroused and almost everyone has some sort of vested interest, clear unbiased thinking is not always easy.

A new look at theory

The monograph *Maternal Care and Mental Health* is in two parts. The first reviews the evidence regarding the adverse effects of maternal deprivation, the second discusses means for preventing it. What was missing, as several reviewers pointed out, was any explanation of how experiences subsumed under the broad heading of maternal deprivation could have the effects on personality development of the kinds claimed. The reason for this omission was simple: the data were not accommodated by any theory then current and in the brief time of my employment by the World Health Organisation there was no possibility of developing a new one.

The child's tie to his mother

At that time it was widely held that the reason a child develops a close tie to his mother is that she feeds him. Two kinds of drive are postulated, primary and secondary. Food is thought of as primary; the personal relationship, referred to as 'dependency', as secondary. This theory did not seem to me to fit the facts. For example, were it true, an infant of a year or two should take readily to whomever feeds him and this clearly was not the case. An alternative theory, stemming from the Hungarian school of psychoanalysis, postulated a primitive object relation from the beginning. In its best-known version, however, the one advocated by Melanie Klein, mother's breast is postulated as the

first object, and the greatest emphasis is placed on food and orality and on the infantile nature of 'dependency'. None of these features matched my experience of children.

But if the current dependency theories were inadequate, what was the alternative?

During the summer of 1951 a friend mentioned to me the work of Lorenz on the following responses of ducklings and goslings. Reading about this and related work on instinctive behaviour revealed a new world, one in which scientists of high calibre were investigating in non-human species many of the problems with which we were grappling in the human, in particular the relatively enduring relationships that develop in many species, first between young and parents and later between mated pairs, and some of the ways in which these developments can go awry. Could this work, I asked myself, cast light on a problem central to psychoanalysis, that of 'instinct' in humans?

Next followed a long phase during which I set about trying to master basic principles and to apply them to our problems, starting with the nature of the child's tie to his mother. Here Lorenz's work on the following response of ducklings and goslings (Lorenz 1935) was of special interest. It showed that in some animal species a strong bond to an individual mother-figure could develop without the intermediary of food: for these young birds *are not fed by parents* but feed themselves by catching insects. Here then was an alternative model to the traditional one, and one that had a number of features that seemed possibly to fit the human case. Thereafter, as my grasp of ethological principles increased and I applied them to one clinical problem after another, I became increasingly confident that this was a promising approach. Thus, having adopted this novel point of view, I decided to 'follow it up through the material as long as the application of it seems to yield results' (to borrow a phrase of Freud's).

From 1957, when *The Nature of the Child's Tie to his Mother* was first presented, through 1969 when *Attachment* appeared, until 1980 with the publication of *Loss* I concentrated on this task. The resulting conceptual framework* is designed to accommodate all

* This is the term Thomas Kuhn (1974) now uses to replace 'paradigm', the term he used in his earlier work (Kuhn 1962).

those phenomena to which Freud called attention – for example love relations, separation anxiety, mourning, defence, anger, guilt, depression, trauma, emotional detachment, sensitive periods in early life – and so to offer an alternative to the traditional metapsychology of psychoanalysis and to add yet another to the many variants of the clinical theory now extant. How successful these ideals will prove only time will tell.

As Kuhn has emphasized, any novel conceptual framework is difficult to grasp, especially so for those long familiar with a previous one. Of the many difficulties met with in understanding the framework advocated, I describe only a few. One is that, instead of starting with a clinical syndrome of later years and trying to trace its origins retrospectively, I have started with a class of childhood traumata and tried to trace the sequelae prospectively. A second is that, instead of starting with the private thoughts and feelings of a patient, as expressed in free associations or play, and trying to build a theory of personality development from those data, I have started with observations of the behaviour of children in certain sorts of defined situation, including records of the feelings and thoughts they express, and have tried to build a theory of personality development from there. Other difficulties arise from my use of concepts such as control system (instead of psychic energy) and developmental pathway (instead of libidinal phase), which, although now firmly established as key concepts in all the biological sciences, are still foreign to the thinking of a great many psychologists and clinicians.

Having discarded the secondary-drive, dependency theory of the child's tie to his mother, and also the Kleinian alternative, a first task was to formulate a replacement. This led to the concept of attachment behaviour with its own dynamics distinct from the behaviour and dynamics of either feeding or sex, the two sources of human motivation for long widely regarded as the most fundamental. Strong support for this step soon came from Harlow's finding that, in another primate species – rhesus macaques – infants show a marked preference for a soft dummy 'mother', despite its providing no food, to a hard one that does provide it (Harlow and Zimmermann 1959).

Attachment behaviour is any form of behaviour that results

in a person attaining or maintaining proximity to some other clearly identified individual who is conceived as better able to cope with the world. It is most obvious whenever the person is frightened, fatigued, or sick, and is assuaged by comforting and caregiving. At other times the behaviour is less in evidence. Nevertheless for a person to know that an attachment figure is available and responsive gives him a strong and pervasive feeling of security, and so encourages him to value and continue the relationship. Whilst attachment behaviour is at its most obvious in early childhood, it can be observed throughout the life cycle, especially in emergencies. Since it is seen in virtually all human beings (though in varying patterns), it is regarded as an integral part of human nature and one we share (to a varying extent) with members of other species. The biological function attributed to it is that of protection. To remain within easy access of a familiar individual known to be ready and willing to come to our aid in an emergency is clearly a good insurance policy – whatever our age.

By conceptualizing attachment in this way, as a fundamental form of behaviour with its own internal motivation distinct from feeding and sex, and of no less importance for survival, the behaviour and motivation are accorded a theoretical status never before given them – though parents and clinicians alike have long been intuitively aware of their importance. As already emphasized, the terms 'dependency' and 'dependency need' that have hitherto been used to refer to them have serious disadvantages. In the first place 'dependency' has a perjorative flavour; in the second it does not imply an emotionally charged relationship to one or a very few clearly preferred individuals; and in the third no valuable biological function has ever been attributed to it.

It is now thirty years since the notion of attachment was first advanced as a useful way of conceptualizing a form of behaviour of central importance not only to clinicians and to developmental psychologists but to every parent as well. During that time attachment theory has been greatly clarified and amplified. The most notable contributors have been Robert Hinde who, in addition to his own publications (e.g. 1974), has constantly guided my own thinking, and Mary Ainsworth who, starting in

the late fifties, has pioneered empirical studies of attachment behaviour both in Africa (1963, 1967) and in the USA (Ainsworth and Wittig 1969; Ainsworth *et al.* 1978), and has also helped greatly to develop theory (e.g. 1969, 1982). Her work, together with that of her students and others influenced by her (which has expanded dramatically since this lecture was given and is described in some detail in Lecture 7), has led attachment theory to be widely regarded as probably the best supported theory of socio-emotional development yet available (Rajecki, Lamb, and Obmascher 1978; Rutter 1980; Parkes and Stevenson-Hinde 1982; Sroufe 1986).

Because my starting point in developing theory was observations of behaviour, some clinicians have assumed that the theory amounts to no more than a version of behaviourism. This mistake is due in large part to the unfamiliarity of the conceptual framework proposed and in part to my own failure in early formulations to make clear the distinction to be drawn between an attachment and attachment behaviour. To say of a child (or older person) that he is attached to, or has an attachment to, someone means that he is strongly disposed to seek proximity to and contact with that individual and to do so especially in certain specified conditions. The disposition to behave in this way is an attribute of the attached person, a persisting attribute which changes only slowly over time and which is unaffected by the situation of the moment. Attachment behaviour, by contrast, refers to any of the various forms of behaviour that the person engages in from time to time to obtain and/or maintain a desired proximity.

There is abundant evidence that almost every child habitually prefers one person, usually his mother-figure, to whom to go when distressed but that, in her absence, he will make do with someone else, preferably someone whom he knows well. On these occasions most children show a clear hierarchy of preference so that, in extremity and with no one else available, even a kindly stranger may be approached. Thus, whilst attachment behaviour may in differing circumstances be shown to a variety of individuals, an enduring attachment, or attachment bond, is confined to very few. Should a child fail to show such clear discrimination, it is likely he is severely disturbed.

The theory of attachment is an attempt to explain both attachment behaviour, with its episodic appearance and disappearance, and also the enduring attachments that children and other individuals make to particular others. In this theory the key concept is that of behavioural system. This is conceived on the analogy of a physiological system organized homeostatically to ensure that a certain physiological measure, such as body temperature or blood pressure, is held between appropriate limits. In proposing the concept of a behavioural system to account for the way a child or older person maintains his relation to his attachment figure between certain limits of distance or accessibility, no more is done than to use these well-understood principles to account for a different form of homeostasis, namely one in which the set limits concern the organism's relation to clearly identified persons in, or other features of, the environment and in which the limits are maintained by behavioural instead of physiological means.

In thus postulating the existence of an internal psychological organization with a number of highly specific features, which include representational models of the self and of attachment figure(s), the theory proposed can be seen as having all the same basic properties as those that characterize other forms of structural theory, of which the variants of psychoanalysis are some of the best known, and that differentiate them so sharply from behaviourism in its many forms. Historically attachment theory was developed as a variant of object-relations theory.

The reason why in this account I have given so much space to the concept and theory of attachment is that, once those principles are grasped, there is little difficulty in understanding how the many other phenomena of central concern to clinicians are explained within the framework proposed.

Separation anxiety

For example, a new light is thrown on the problem of separation anxiety, namely anxiety about losing, or becoming separated from, someone loved. Why 'mere separation' should cause anxiety has been a mystery. Freud wrestled with the problem and advanced a number of hypotheses (Freud 1926; Strachey 1959).

Every other leading analyst has done the same. With no means of evaluating them, many divergent schools of thought have proliferated.

The problem lies, I believe, in an unexamined assumption, made not only by psychoanalysts but by more traditional psychiatrists as well, that fear is aroused in a mentally healthy person only in situations that everyone would perceive as intrinsically painful or dangerous, or that are perceived so by a person only because of his having become conditioned to them. Since fear of separation and loss does not fit this formula, analysts have concluded that what is feared is really some other situation; and a great variety of hypotheses have been advanced.

The difficulties disappear, however, when an ethological approach is adopted. For it then becomes evident that man, like other animals, responds with fear to certain situations, not because they carry a *high* risk of pain or danger, but because they signal an *increase* of risk. Thus, just as animals of many species, including man, are disposed to respond with fear to sudden movement or a marked change in level of sound or light because to do so has survival value, so are many species, including man, disposed to respond to separation from a potentially caregiving figure and for the same reasons.

When separation anxiety is seen in this light, as a basic human disposition, it is only a small step to understand why it is that threats to abandon a child, often used as a means of control, are so very terrifying. Such threats, and also threats of suicide by a parent, are, we now know, common causes of intensified separation anxiety. Their extraordinary neglect in traditional clinical theory is due, I suspect, not only to an inadequate theory of separation anxiety but to a failure to give proper weight to the powerful effects, at all ages, of real-life events.

Not only do threats of abandonment create intense anxiety but they also arouse anger, often also of intense degree, especially in older children and adolescents. This anger, the function of which is to dissuade the attachment figure from carrying out the threat, can easily become dysfunctional. It is in this light, I believe, that we can understand such absurdly paradoxical behaviour as the adolescent, reported by Burnham (1965), who,

having murdered his mother, exclaimed, 'I couldn't stand to have her leave me.'

Other pathogenic family situations are readily understood in terms of attachment theory. One fairly common example is when a child has such a close relationship with his mother that he has difficulty in developing a social life outside the family, a relationship sometimes described as symbiotic. In a majority of such cases the cause of the trouble can be traced to the mother who, having grown up anxiously attached as a result of a difficult childhood, is now seeking to make her own child her attachment figure. So far from the child being over-indulged, as is sometimes asserted, he is being burdened with having to care for his own mother. Thus, in these cases, the normal relationship of attached child to caregiving parent is found to be inverted.

Mourning

Whilst separation anxiety is the usual response to a threat or some other risk of loss, mourning is the usual response to a loss after it has occurred. During the early years of psychoanalysis a number of analysts identified losses, occurring during childhood or in later life, as playing a causal role in emotional disturbance, especially in depressive disorders; and by 1950 a number of theories about the nature of mourning, and other responses to loss, had been advanced. Moreover, much sharp controversy had already been engendered. This controversy, which began during the thirties, arose from the divergent theories about infant development that had been elaborated in Vienna and London. Representative examples of the different points of view about mourning are those expressed in Helene Deutsch's *Absence of Grief* (1937) and Melanie Klein's *Mourning and its Relation to Manic-Depressive States* (1940). Whereas Deutsch held that, due to inadequate psychic development, children are unable to mourn, Klein held that they not only can mourn but do. In keeping with her strong emphasis on feeding, however, she held that the object mourned was the lost breast; and, in addition, she attributed a complex fantasy-life to the infant. Opposite though these theoretical positions are, both were con-

structed using the same methodology, namely by inferences about earlier phases of psychological development based on observations made during the analysis of older, and emotionally disturbed, subjects. Neither theory had been checked by direct observation of how ordinary children of different ages respond to a loss.

Approaching the problem prospectively, as I did, led me to different conclusions. During the early nineteen-fifties Robertson and I had generalized the sequence of responses seen in young children during temporary separation from mother as those of protest, despair, and detachment (Robertson and Bowlby 1952). A few years later, when reading a study by Marris (1958) of how widows respond to loss of husband, I was struck by the similarity of the responses he describes to those of young children. This led me to a systematic study of the literature on mourning, especially the mourning of healthy adults. The sequence of responses that commonly occur, it became clear, was very different from what clinical theorists had been assuming. Not only does mourning in mentally healthy adults last far longer than the six months often suggested in those days, but several component responses widely regarded as pathological were found to be common in healthy mourning. These include anger, directed at third parties, the self, and sometimes at the person lost, disbelief that the loss has occurred (misleadingly termed denial), and a tendency, often though not always unconscious, to search for the lost person in the hope of reunion. The clearer the picture of mourning responses in adults became, the clearer became their similarities to the responses observed in childhood. This conclusion, when first advanced (Bowlby 1960, 1961), was much criticized; but it has now been amply supported by a number of subsequent studies (e.g. Parkes 1972; Kliman 1965; Furman 1974; Raphael 1982).

Once an accurate picture of healthy mourning has been obtained, it becomes possible to identify features that are truly indicative of pathology. It becomes possible also to discern many of the conditions that promote healthy mourning and those that lead in a pathological direction. The belief that children are unable to mourn can then be seen to derive from generalizations that had been made from the analyses of children whose

mourning had followed an atypical course. In many cases this had been due either to the child never having been given adequate information about what had happened, or else to there having been no one to sympathize with him and help him gradually come to terms with his loss, his yearning for his lost parent, his anger, and his sorrow.

Defensive processes

The next step in this reformulation of theory was to consider how defensive processes could best be conceptualized, a crucial step since defensive processes have always been at the heart of psychoanalytic theory. Although as a clinician I have inevitably been concerned with the whole range of defences, as a research worker I have directed my attention especially to the way a young child behaves towards his mother after a spell in a hospital or residential nursery unvisited. In such circumstances it is common for a child to begin by treating his mother almost as though she were a stranger, but then, after an interval, usually of hours or days, to become intensely clinging, anxious lest he lose her again, and angry with her should he think he may. In some way all his feeling for his mother and all the behaviour towards her we take for granted, keeping within range of her and most notably turning to her when frightened or hurt, have suddenly vanished – only to reappear again after an interval. That was the condition James Robertson and I termed detachment and which we believed was a result of some defensive process operating within the child.

Whereas Freud in his scientific theorizing felt confined to a conceptual model that explained all phenomena, whether physical or biological, in terms of the disposition of energy, today we have available conceptual models of much greater variety. Many draw on such interrelated concepts as organization, pattern, and information; while the purposeful activities of biological organisms can be conceived in terms of control systems structured in certain ways. With supplies of physical energy available to them, these systems become active on receipt of certain sorts of signal and inactive on receipt of signals of other sorts. Thus the world of science in which we live is radically

different from the world Freud lived in at the turn of the century, and the concepts available to us immeasurably better suited to our problems than were the very restricted ones available in his day.

If we return now to the strange detached behaviour a young child shows after being away for a time with strange people in a strange place, what is so peculiar about it is, of course, the absence of attachment behaviour in circumstances in which we would confidently expect to see it. Even when he has hurt himself severely, such a child shows no sign of seeking comfort. Thus signals that would ordinarily activate attachment behaviour are failing to do so. This suggests that in some way and for some reason these signals are failing to reach the behavioural system responsible for attachment behaviour, that they are being blocked off, and the behavioural system itself is thereby immobilized. What this means is that a system controlling such crucial behaviour as attachment can in certain circumstances be rendered either temporarily or permanently incapable of being activated, and with it the whole range of feeling and desire that normally accompanies it is rendered incapable of being aroused.

In considering how this deactivation might be effected I turn to the work of the cognitive psychologists (e.g. Norman 1976; Dixon 1971, 1981) who, during the past twenty years, have revolutionized our knowledge of how we perceive the world and how we construe the situations we are in. Amongst much else that is clinically congenial, this revolution in cognitive theory not only gives unconscious mental processes the central place in mental life that analysts have always claimed for them, but presents a picture of the mental apparatus as being well able to shut off information of certain specified types and of doing so selectively without the person being aware of what is happening.

In the emotionally detached children described earlier and also, I believe, in adults who have developed the kind of personality that Winnicott (1960) describes as 'false self' and Kohut (1977) as 'narcissistic', the information being blocked off is of a very special type. So far from its being the routine exclusion of irrelevant and potentially distracting information that we engage in all the time and that is readily reversible, what are being excluded in these pathological conditions are the

signals, arising from both inside and outside the person, that would activate their attachment behaviour and that would enable them both to love and to experience being loved. In other words, the mental structures responsible for routine selective exclusion are being employed – one might say exploited – for a special and potentially pathological purpose. This form of exclusion I refer to – for obvious reasons – as defensive exclusion, which is, of course, only another way of describing repression. And, just as Freud regarded repression as the key process in every form of defence, so I see the role of defensive exclusion.* A fuller account of this, an information-processing approach to the problem of defence, in which defences are classified into defensive processes, defensive beliefs, and defensive activities, is given in an early chapter of *Loss* (Bowlby 1980).

An alternative framework

During the time it has taken to develop the conceptual framework described here Margaret Mahler has been concerned with many of the same clinical problems and some of the same features of children's behaviour; and she also has been developing a revised conceptual framework to account for them, set out fully in her book *The Psychological Birth of the Human Infant* (Mahler, Pine, and Bergman 1975). To compare alternative frameworks is never easy, as Kuhn (1962) emphasizes, and no attempt is made to do so here. Elsewhere (e.g. Bowlby 1981) I describe what I believe to be some of the strengths of the framework I favour, including its close relatedness to empirical data, both clinical and developmental, and its compatibility with current ideas in evolutionary biology and neurophysiology; whilst what I see as the shortcomings of Mahler's framework are trenchantly criticized by Peterfreund (1978) and Klein (1981).

In brief, Mahler's theories of normal development, including her postulated normal phases of autism and symbiosis, are shown to rest not on observation but on preconceptions based on traditional psychoanalytic theory and, in doing so, to ignore

* As Spiegel (1981) points out, my term 'defensive exclusion' carries a meaning very similar to Sullivan's term 'selective inattention'.

almost entirely the remarkable body of new information about early infancy that has been built up from careful empirical studies over the past two decades. Although some of the clinical implications of Mahler's theory are not very different from those of attachment theory, and her concept of return to base to 'refuel' is similar to that of use of an attachment figure as a secure base from which to explore, the key concepts with which the two frameworks are built are very different.

Research

Nothing has been so rewarding as the immense amount of careful research to which the early work on maternal deprivation has given rise. The literature is now enormous and far beyond the compass of an account of this sort to summarize. Fortunately, moreover, it is unnecessary since a comprehensive and critical review of the field has been published by Rutter (1979) who concludes by referring to the 'continuing accumulation of evidence showing the importance of deprivation and disadvantage on children's psychological development' and expressing the view that the original arguments 'have been amply confirmed'. A principal finding of recent work is the extent to which two or more adverse experiences interact so that the risk of a psychological disturbance following is multiplied, often many times over. An example of this interactive effect of adverse experiences is seen in the findings of Brown and Harris (1978) derived from their studies of depressive disorders in women. (During the last decade this group has published many further findings of the greatest interest, see Harris (in press).)

Not only is there this strongly interactive effect of adverse experiences but there is an increased likelihood for someone who has had one adverse experience to have another. For example, 'people brought up in unhappy or disrupted homes are more likely to have illegitimate children, to become teenage mothers, to make unhappy marriages, and to divorce' (Rutter 1979). Thus adverse childhood experiences have effects of at

least two kinds. First they make the individual more vulnerable to later adverse experiences. Secondly they make it more likely that he or she will meet with further such experiences. Whereas the earlier adverse experiences are likely to be wholly independent of the agency of the individual concerned, the later ones are likely to be the consequences of his or her own actions, actions that spring from those disturbances of personality to which the earlier experiences have given rise.

Of the many types of psychological disturbance that are traceable, at least in part, to one or another pattern of maternal deprivation, the effects on parental behaviour and thereby on the next generation are potentially the most serious. Thus a mother who, due to adverse experiences during childhood, grows up to be anxiously attached is prone to seek care from her own child and thereby lead the child to become anxious, guilty, and perhaps phobic (see review in Bowlby 1973). A mother who as a child suffered neglect and frequent severe threats of being abandoned or beaten is more prone than others to abuse her child physically (DeLozier 1982), resulting in the adverse effects on the child's developing personality recorded, amongst others, by George and Main (1979). Systematic research into the effects of childhood experiences on the way mothers and fathers treat their children has only just begun and seems likely to be one of the most fruitful of all fields for further research. Other research leads are described in a recent symposium edited by Parkes and Stevenson-Hinde (1982).

My reason for giving so much space in this account to the development of theory is not only because it has occupied so much of my time but because, as Kurt Lewin remarked long ago, 'There is nothing so practical as a good theory', and, of course, nothing so handicapping as a poor one. Without good theory as a guide, research is likely to be difficult to plan and to be unproductive, and findings are difficult to interpret. Without a reasonably valid theory of psychopathology, therapeutic techniques tend to be blunt and of uncertain benefit. Without a reasonably valid theory of aetiology, systematic and agreed measures of prevention will never be supported. My hope is that in the long term the greatest value of the theory proposed

may prove to be the light it throws on the conditions most likely to promote healthy personality development. Only when those conditions are clear beyond doubt will parents know what is best for their children and will communities be willing to help them provide it.

LECTURE 3

Psychoanalysis as art and science

During the summer of 1978 I was invited to give a number of lectures in Canada. Among the invitations was one from the Canadian Psychoanalytic Society to give their academic lecture to the annual meeting of the Society to be held in Quebec City. The topic I selected is one which had concerned me for some years, and about which I believe there is still a great deal of confused thinking.

In taking as my theme psychoanalysis as art *and* science I want to draw attention to what I believe to be two very different aspects of our discipline – the art of psychoanalytic therapy and the science of psychoanalytic psychology – and in doing so to emphasize, on the one hand, the distinctive value of each and, on the other, the gulf that divides them – in regard both to the contrasting criteria by which each should be judged and the very different mental outlook that each demands. In emphasizing these distinctions, I cannot help regretting that the word *psychoanalysis* came early to be used ambiguously as Freud himself described it. 'While it was originally the name of a particular therapeutic method', he writes in his autobiography (1925), 'it has now also become the name of a science – the science of unconscious mental processes'.

The distinction I am drawing, of course, is not confined to psychoanalysis. It applies in every field in which the practice of