



VINTAGE

ATTACHMENT

JOHN BOWLBY

Contents

Cover
About the Book
About the Author
Dedication
Title Page
Preface

PART I: THE TASK

1. Point of View
 - Some characteristics of the present approach*
 - Theories of motivation*
 - Note on the concept of feedback in Freud's theorising*
2. Observations to be Explained

PART II: INSTINCTIVE BEHAVIOUR

3. Instinctive Behaviour: An Alternative Model
 - Introduction*
 - Some principles of control systems*
 - Control systems and instinctive behaviour*
 - Adaptation: system and environment*
 - Note on literature*
4. Man's Environment of Evolutionary Adaptedness
5. Behavioural Systems Mediating Instinctive Behaviour
 - Types of behavioural system*
 - Co-ordination of behavioural systems*
 - Higher processes of integration and control*
6. Causation of Instinctive Behaviour
 - Activation and termination of behavioural systems*

Incompatible behavioural systems: results of simultaneous activation

Sensory input and its transformation

7. Appraising and Selecting: Feeling and Emotion

Introduction

Philosophical problems

Processes that are felt

Is feeling or emotion causative of behaviour?

The communicative role of feeling and emotion

8. Function of Instinctive Behaviour

Functions of behavioural systems and other consequences of their activity

Problems of terminology

9. Changes in Behaviour during the Life-cycle

10. Ontogeny of Instinctive Behaviour

Changes that occur during the ontogeny of behavioural systems

Restriction of range of effective stimuli

Elaboration of primitive behavioural systems and their supersession by sophisticated systems

Integration of behavioural systems into functional wholes

Sensitive periods of development

Imprinting

Comparison of old and new theories of instinctive behaviour

PART III: ATTACHMENT BEHAVIOUR

11. The Child's Tie to his Mother: Attachment Behaviour

Alternative theories

Attachment behaviour and its place in nature

Attachment behaviour in non-human primates

Attachment behaviour in man

12. Nature and Function of Attachment Behaviour

The theory of secondary drive: origin and present status

- The question of imprinting*
- Function of attachment behaviour*
- A note on terminology: 'dependence'*
- Attachment and other systems of social behaviour*
- 13. A Control Systems Approach to Attachment Behaviour
 - Introduction*
 - The roles of child and of mother in mother-child interaction*
 - Forms of behaviour mediating attachment and their organisation*
 - Behaviour typical of two-year-olds in different situations*
 - Activation and termination of systems mediating attachment behaviour*

PART IV: ONTOGENY OF HUMAN ATTACHMENT

- 14. Beginnings of Attachment Behaviour
 - Phases in the development of attachment*
 - Behavioural equipment of the human neonate*
 - Early responses to people*
 - Nature and nurture*
- 15. Focusing on a Figure
 - Introduction*
 - Patterns of differentially directed behaviour*
 - Figures towards whom attachment behaviour is directed*
 - Processes leading to selection of figures*
 - Sensitive phases and the fear of strangers*
 - Spitz's position: a critique*
- 16. Patterns of Attachment and Contributing Conditions
 - Problems to be solved*
 - Criteria for describing patterns of attachment*
 - Some patterns of attachment seen at the first birthday*
 - Conditions of the first year contributing to variation*
 - Persistence and stability of patterns*
- 17. Developments in the Organisation of Attachment Behaviour

PART V: OLD CONTROVERSIES AND NEW FINDINGS

18. Stability and Change in Patterns of Attachment

Further development of infants assessed as securely or anxiously attached

The organisation of attachment: from lability to stability

Development of conceptual perspective taking

19. Objections, Misconceptions and Clarifications

Attachment as an organisational concept

Attachment-caregiving: one type of social bond

References

Acknowledgements

Author Index

Subject Index

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About the Book

In this classic work of psychology John Bowlby examines the processes that take place in attachment and separation and shows how experimental studies of children provide us with a recognizable behaviour pattern which is confirmed by discoveries in the biological sciences. He makes clear that human attachment is an instinctive response to the need for protection against predators, and one as important for survival as nutrition and reproduction.

About the Author

John Bowlby (1907-1990) was educated at the University of Cambridge and University College Hospital, London. After qualifying in medicine, he specialised in child psychiatry and psychoanalysis. In 1946 he joined the staff of the Tavistock Clinic where his research and influential publications contributed to far-reaching changes in the ways children are treated and to radical new thinking about the social and emotional development of human beings.

He held honorary degrees from the Universities of Cambridge and Leicester and received awards from professional and scientific bodies, including the Royal College of Psychiatrists, the British Paediatric Association, the Society for Research in Child Development, the American Psychological Association and the New York Academy of Medicine.

To
URSULA

Attachment and Loss
Volume 1

ATTACHMENT

JOHN BOWLBY



PIMLICO

Preface

IN 1956 WHEN this work was begun I had no conception of what I was undertaking. At that time my object appeared a limited one, namely, to discuss the theoretical implications of some observations of how young children respond to temporary loss of mother. These observations had been made by my colleague, James Robertson, and together he and I were preparing them for publication. A discussion of their theoretical significance seemed desirable and was destined to form the second part of our book.

Events were to prove otherwise. As my study of theory progressed it was gradually borne in upon me that the field I had set out to plough so lightheartedly was no less than the one that Freud had started tilling sixty years earlier, and that it contained all those same rocky excrescences and thorny entanglements that he had encountered and grappled with—love and hate, anxiety and defence, attachment and loss. What had deceived me was that my furrows had been started from a corner diametrically opposite to the one at which Freud had entered and through which analysts have always followed. From a new viewpoint a familiar landscape can sometimes look very different. Not only had I been deceived in the first place, but subsequently progress has been slow. It has also, I believe, often been difficult for colleagues to understand what I am attempting. It may be of help, therefore, if I put my thinking in a historical perspective.

In 1950 I was asked by the World Health Organisation to advise on the mental health of homeless children. This

assignment provided a valuable opportunity to meet with many of the leading workers in the field of child care and child psychiatry and to read the literature. As I wrote in the preface to the resulting report (1951), what struck me amongst those I met was the 'very high degree of agreement existing in regard to both the principles underlying the mental health of children and the practices by which it may be safeguarded'. In the first part of the report I presented evidence and formulated a principle: 'What is believed to be essential for mental health is that the infant and young child should experience a warm, intimate and continuous relationship with his mother (or permanent mother-substitute) in which both find satisfaction and enjoyment.' In the second part I outlined the measures that, in the light of these principles, are necessary if the mental health of children separated from their families is to be safeguarded.

The report proved timely. It helped to focus attention on the problem, contributed to improved methods of care, and stimulated both controversy and research. Yet, as more than one reviewer pointed out, the report had at least one grave limitation. Whereas it had much to say about the many kinds of ill effect that evidence shows can be attributed to maternal deprivation and also about practical measures that may prevent or mitigate these ill effects, it said very little indeed about the processes whereby these ill effects are brought into being. How does it come about that one or another of the events included under the general heading of maternal deprivation produces this or that form of psychiatric disturbance? What are the processes at work? Why should things happen this way? What are the other variables that affect outcome, and how do they affect it? On all these issues the monograph is silent, or nearly so.

The reason for this silence was ignorance—my own and others'—which could not possibly have been made good in the few months in which the report had to be written.

Sooner or later, I hoped, the gap would be filled, though it was unclear when or how.

It was in this frame of mind that I began to give serious attention to observations my colleague James Robertson had been making. With the help of a small grant from the Sir Halley Stewart Trust he had joined me in 1948 to take part in what was intended to be a systematic inquiry into the whole problem of the effects on personality development of separation from mother in early childhood. During an extended reconnaissance of what was at that time largely virgin ground he had observed a number of young children before, during, and after a stay away from home; most of these children were in their second and third years of life and not only were separated from their mothers but for periods of weeks or months were cared for in settings, such as hospital or residential nursery, in which they had no stable mother-substitute. During this work he had been deeply impressed by the intensity of the distress and misery he was witness to whilst the children were away from home and by the extent and duration of the disturbance that was present after they had returned there. No one reading his written reports or viewing the film record he made of one little girl could be left unmoved. Nevertheless, at that time there was no agreement about the significance or relevance of these observations. Some challenged their validity; others recognised that the responses occurred but attributed them to almost anything but loss of mother-figure; yet others conceded that loss was a relevant variable but held that to mitigate its effects was not too difficult and that loss was therefore of less consequence for pathology than we supposed.

My colleagues and I took a different view. We were confident that the observations were valid; all the evidence pointed to loss of mother-figure as a dominant variable, though not the only one; and our experience suggested that, even when other circumstances were favourable,

there was more distress and disturbance than was usually recognised. Indeed, we held the view that the responses of protest, despair, and detachment that typically occur when a young child aged over six months is separated from his mother and in the care of strangers are due mainly to 'loss of maternal care at this highly dependent, highly vulnerable stage of development'. From empirical observation we suggested that 'the young child's hunger for his mother's love and presence is as great as his hunger for food', and that in consequence her absence inevitably generates 'a powerful sense of loss and anger'. We were concerned particularly with the great changes in a child's relation to his mother that are often to be seen when he returns home after a period away; on the one hand, 'an intense clinging to the mother which can continue for weeks, months or years'; on the other, 'a rejection of the mother as a love object, which may be temporary or permanent'. The latter state, to which we later came to refer as detachment, we held to be a result of the child's feelings for his mother having undergone repression.

Thus we reached the conclusion that loss of mother-figure, either by itself or in combination with other variables yet to be clearly identified, is capable of generating responses and processes that are of the greatest interest to psychopathology. Not only so, but these responses and processes, we concluded, are the very same as are known to be active in older individuals who are still disturbed by separations that they suffered in early life. Amongst these responses and processes and amongst forms of disturbance are, on the one hand, a tendency to make excessive demands on others and to be anxious and angry when they are not met, such as is present in dependent and hysterical personalities; and, on the other, a blockage in the capacity to make deep relationships, such as is present in affectionless and psychopathic personalities. In other words, it seemed to us that when we

observe children during and after periods away from mother and in a strange setting we are witnessing responses, and also effects of defensive processes, that are just those that enable us to bridge the gap between an experience of this sort and one or another of the disturbances in personality functioning that may follow.

These conclusions, which sprang naturally from the empirical data, led to a crucial decision of research strategy. Since our aim was to understand how these pathological processes originate and develop, we decided that henceforward we would take as our principal data detailed records of how young children respond to the experiences of being separated from and later of being reunited with mother. Such data, we had come to believe, are of great intrinsic interest and an essential complement to data of a traditional kind derived from the treatment of older subjects. The thinking underlying this decision and some of the original data are reported in papers published between 1952 and 1954; and a film was published during the same period.¹

During the years that have elapsed since this decision was taken my colleagues and I have given much time to the scrutiny of data already collected, the collection and analysis of further data, the comparison of these data with data from other sources, and an examination of their theoretical implications. Amongst the fruits of this work already published is a volume, *Brief Separations* (1966), in which Christoph Heinicke and Ilse Westheimer study responses to be seen during and after a brief separation experienced in a defined setting. In that study not only were the responses observed and recorded in a more systematic way than had been possible in earlier studies but the behaviour of the separated children was compared statistically with the behaviour shown by a matched sample of children living in their own homes and not separated. Within its limits the findings of this later study confirm the

less systematic but more extensive findings of James Robertson and amplify them at a number of points.

In a series of papers published between 1958 and 1963 I have myself discussed some of the theoretical problems raised by these observations. The present three volumes cover the same ground, but do so in a more rigorous way. There is also much additional material.

Volume I is devoted to problems originally tackled in the first paper of the series, 'The nature of the child's tie to his mother' (1958). In order effectively to present the theory to be advanced, which is attempted in [Parts III](#) and [IV](#), it has been necessary to discuss first the whole problem of instinctive behaviour and how best to conceptualise it. The rather long discussion entailed constitutes [Part II](#) of the volume. It is preceded by two chapters forming [Part I](#): the first sets out systematically some of the assumptions from which I start and compares them with Freud's; the second reviews the empirical observations on which I am drawing and gives a précis of them. The aim of all the chapters of [Parts I](#) and [II](#) is to clarify and make more explicit the concepts with which I am working, since these, because they are unfamiliar, have proved puzzling to many clinicians otherwise sympathetic to the work.

Volume II, *Separation*, deals with problems originally tackled in the second and third papers of the series: 'Separation anxiety' (1960a) and 'Separation anxiety: a critical review of the literature' (1961a).

The third volume, *Loss*, deals with problems originally tackled in the subsequent papers: 'Grief and mourning in infancy and early childhood' (1960b); 'Processes of mourning' (1961b); and 'Pathological mourning and childhood mourning' (1963).

Throughout this inquiry my frame of reference has been that of psychoanalysis. There are several reasons for this. The first is that my early thinking on the subject was inspired by psychoanalytic work—my own and others'. A

second is that, despite limitations, psychoanalysis remains the most serviceable and the most used of any present-day theory of psychopathology. A third and most important is that, whereas all the central concepts of my schema—object relations, separation anxiety, mourning, defence, trauma, sensitive periods in early life—are the stock-in-trade of psychoanalytic thinking, until recently they have been given but scant attention by other behavioural disciplines.

In the course of his explorations Freud followed many different lines of thought and tried many possible theoretical constructions. Since his death the contradictions and ambiguities he left behind have caused unease and there have been attempts to tidy up: certain of his theories have been selected and elaborated, others laid aside and neglected. Because some of my ideas are alien to the theoretical traditions that have become established, and so have met with strong criticism, I have been at some pains to show that most of them are by no means alien to what Freud himself thought and wrote. On the contrary, as I hope to show, a great number of the central concepts of my schema are to be found plainly stated by Freud.

Preface to the Second Edition

The principal reason for preparing a revised edition of this work is that during the past fifteen years there have been major developments in the thinking of biologists studying the social behaviour of species other than man. These developments have necessitated significant changes in a few places in [Part II](#), namely the last two sections of [Chapter 3](#) (especially pp. 53-7), in the sub-section of [Chapter 8](#) on altruistic behaviour (pp. 131-2), and in the opening paragraph of [Chapter 9](#) (p. 141).

Another reason is that, since the publication of the first edition, ideas about attachment have been at the centre of

much theoretical discussion and have also provided guidelines for empirical research of the greatest interest. It seemed timely, therefore, to add two new chapters in which certain of the theoretical problems could be clarified and some of the more important research findings described. To provide space, the Appendix reviewing the earlier literature on the nature of the child's tie to his mother has been omitted.

In [Part III](#) few changes have been required, though opportunity has been taken to revise the section of [Chapter 11](#) on non-human primates to take account of the most recent findings.

In [Part IV](#) a large number of incidental revisions have been called for as a result of the intensive research on the early years of human life undertaken during recent years; and attention is drawn to new findings described in more detail in [Chapter 18](#).

The many new publications referred to in the text are incorporated in the revised References. Indexes also have been revised.

¹ These papers, from which the passages quoted are taken, are as follows: Robertson and Bowlby (1952); Bowlby, Robertson, and Rosenbluth (1952); Bowlby (1953); Robertson (1953); and Ainsworth and Bowlby (1954). The film is Robertson (1952).

Part I
THE TASK

Chapter 1

Point of View

The extraordinary intricacy of all the factors to be taken into consideration leaves only one way of presenting them open to us. We must select first one and then another point of view, and follow it up through the material as long as the application of it seems to yield results.

SIGMUND FREUD (1915b)¹

DURING NEARLY FIFTY years of psychoanalytic investigation Freud tried first one and then another point of view from which to start his inquiries. Dreams, the symptoms of neurotic patients, the behaviour of primitive peoples were amongst the varied data he studied. But, although in his search for explanation he was in each case led to events of early childhood, he himself only rarely drew for his basic data on direct observation of children. The result is that most of the concepts that psychoanalysts have about early childhood have been arrived at by a process of historical reconstruction based on data derived from older subjects. This remains true even of ideas that stem from child analysis: the events and processes inferred belong to a phase of life that is already passed.

The point of view from which this work starts is different. For reasons that are described in the preface it is believed that observation of how a very young child

behaves towards his mother, both in her presence and especially in her absence, can contribute greatly to our understanding of personality development. When removed from mother by strangers young children respond usually with great intensity; and after reunion with her they show commonly either a heightened degree of separation anxiety or else an unusual detachment. Since a change in relations of one or other of these kinds, or even of both compounded, is frequent in subjects suffering from psychoneurosis and other forms of emotional disturbance, it seemed promising to select these observations as a starting-point; and having adopted this point of view to 'follow it up through the material as long as the application of it seems to yield results'.

Because this starting-point differs so much from the one to which psychoanalysts are accustomed, it may be useful to specify it more precisely and to elaborate the reasons for adopting it.

Psychoanalytic theory is an attempt to explain the functioning of personality, in both its healthy and its pathological aspects, in terms of ontogenesis. In creating this body of theory not only Freud but virtually all subsequent analysts have worked from an end-product backwards. Primary data are derived from studying, in the analytic setting, a personality more or less developed and already functioning more or less well; from those data the attempt is made to reconstruct the phases of personality that have preceded what is now seen.

In many respects what is attempted here is the opposite. Using as primary data observations of how very young children behave in defined situations, an attempt is made to describe certain early phases of personality functioning and, from them, to extrapolate forwards. In particular, the aim is to describe certain patterns of response that occur regularly in early childhood and, thence, to trace out how similar patterns of response are to be discerned in the

functioning of later personality. The change in perspective is radical. It entails taking as our starting-point, not this or that symptom or syndrome that is giving trouble, but an event or experience deemed to be potentially pathogenic to the developing personality.

Thus, whereas almost all present-day psychoanalytical theory starts with a clinical syndrome or symptom—for example, stealing, or depression, or schizophrenia—and makes hypotheses about events and processes which are thought to have contributed to its development, the perspective adopted here starts with a class of event—loss of mother-figure in infancy or early childhood—and attempts thence to trace the psychological and psychopathological processes that commonly result. It starts in fact with the traumatic experience and works prospectively.

A shift of this kind in research orientation is still unusual in psychiatry. In physiological medicine, on the other hand, it occurred long ago and an illustration drawn from that field may help to illustrate the point. When a study of the pathology of chronic infection of the lungs is undertaken today, an investigator is no longer likely to start with a group of cases all showing chronic infection and attempt to discover the infective agent or agents that are at work. It is more likely that he will start with a specified agent, perhaps tubercle or actino-mycosis or some newly identified virus, in order to study the physiological and physiopathological processes to which it gives rise. In so doing he may discover many things which are not immediately relevant to chronic infective pulmonary conditions. Not only may he throw light on certain acute infectious and sub-clinical conditions, but he is almost sure to discover that infections of other organs besides lungs are the work of the pathogenic organism he has selected for study. No longer is his centre of interest a particular

clinical syndrome; it has become instead the manifold sequelae of a particular pathogenic agent.

The pathogenic agent the effects of which are to be discussed is loss of mother-figure during the period between about six months and six years of age. Before considering the basic observations that are used, however, it is well to complete the description of the ways in which the approach adopted differs from the traditional one and to discuss a few of the criticisms that it has met with.

Some characteristics of the present approach

One of the differences has already been alluded to. Instead of data obtained in the treatment of patients, the data drawn on are observations of the behaviour of young children in real-life situations. Now such data are sometimes regarded as of only peripheral concern to our science. Occasionally comment implies that, by its very nature, the direct observation of behaviour can provide information of only a superficial kind and that it contrasts sharply with what, it is held, is the almost direct access to psychical functioning that obtains during psychoanalytic treatment. As a result, whenever direct observation of behaviour confirms conclusions reached in the treatment of patients it is regarded as of interest, whereas when it points in some other direction it is apt to be laid aside as of little import.

Now I believe an attitude of this sort to be based on fallacious premises. In the first place we must not overrate the data we obtain in analytic sessions. So far from our having direct access to psychical processes, what confronts us is a complex web of free associations, reports of past events, comments about the current situation, and the patient's behaviour. In trying to understand these diverse manifestations we inevitably select and arrange them according to our preferred schema; and in trying to infer

what psychological processes may lie behind them we inevitably leave the world of observation and enter the world of theory. Whilst the manifestations of psychological processes met with in the consulting room are often unusually rich and varied, we are nonetheless still far from having opportunity for direct observation of psychological process.

Indeed, the opposite is probably nearer the truth. Philosophers of mind hold that, in the life of an individual, it is the 'patterns of behaviour' perceptible in infancy that 'must be the original endowment from which the purely mental states develop'; and that what is later regarded as 'inner', be it an emotion, an affect, or a fantasy, is 'a residue' that remains when all forms of associated behaviour are reduced to vanishing point (Hampshire, 1962). Since the capacity to restrict associated behaviour increases with age, it is evident that the younger the subject the more likely are his behaviour and his mental state to be the two sides of a single coin. Provided observations are skilled and detailed, therefore, a record of the behaviour of very young children can be regarded as a useful index of their concurrent mental state.

In the second place, those who are sceptical of the value of direct observation of behaviour habitually underrate the diversity and richness of the data that can be obtained. When young children are observed in situations that lead to anxiety and distress it is possible to obtain data that are plainly relevant to many concepts central to our discipline—love, hate, and ambivalence; security, anxiety, and mourning; displacement, splitting, and repression. It will, indeed, be argued that observation of the onset of detached behaviour in a child who is spending a few weeks in strange surroundings away from his mother is as close as we can get to observing repression actually occurring.

The truth is that neither class of data is intrinsically better than the other. Each is relevant to the problems with

which psychoanalysis grapples and the contribution made by each is likely to be enhanced when seen in conjunction with the contribution made by the other. Binocular vision is better than the vision of either eye used separately.

Another way in which the approach adopted differs from the traditional psychoanalytic one is that it draws heavily on observations of how members of other species respond to similar situations of presence or absence of mother; and that it makes use of the wide range of new concepts that ethologists have developed to explain them.

A main reason for valuing ethology is that it provides a wide range of new concepts to try out in our theorising. Many of them are concerned with the formation of intimate social bonds—such as those tying offspring to parents, parents to offspring, and members of the two sexes (and sometimes of the same sex) to each other. Others are concerned with conflict behaviour and ‘displacement activity’; others again with the development of pathological fixations, in the form either of maladaptive behaviour patterns or of unsuitable objects to which behaviour is directed. We know now that man has no monopoly either of conflict or of behaviour pathology. A canary that first starts building its nest when insufficient building material is available not only will develop pathological nest-building behaviour but will persist in such behaviour even when, later, suitable material can be had. A goose can court a dog-kennel and mourn when it is overturned. Ethological data and concepts are therefore concerned with phenomena at least comparable to those we as analysts try to understand in man.

Nevertheless, until the concepts of ethology have been tried out in the field of human behaviour we shall be in no position to determine how useful they are. Every ethologist knows that, however valuable a knowledge of related species may be in suggesting what to look for in a new species under investigation, it is never permissible to

extrapolate from one species to another. Man is neither a monkey nor a white rat, let alone a canary or a cichlid fish. Man is a species in his own right with certain unusual characteristics. It may be therefore that none of the ideas stemming from studies of lower species is relevant. Yet this seems improbable. In the fields of infant-feeding, of reproduction, and of excretion we share anatomical and physiological features with lower species, and it would be odd were we to share none of the behavioural features that go with them. Furthermore, it is in early childhood, especially the preverbal period, that we might expect to find these features in least-modified form. May it not be that some at least of the neurotic tendencies and personality deviations that stem from the early years are to be understood as due to disturbance in the development of these bio-psychological processes? Whether the answer proves to be 'yes' or 'no' it is only common sense to explore the possibility.

Where Freud Stands

So far four characteristics of the point of view adopted have been described—a prospective approach, a focus on a pathogen and its sequelae, direct observation of young children, and a use of animal data—and reasons have been given for favouring each of them. Because few psychoanalysts adopt this standpoint, however, and because fear is sometimes expressed that to work from it represents a break with tradition that may be dangerous, it is of interest to see where Freud stands. In respect of each of the four characteristics in turn, first Freud's views are described and then the position adopted in this book is elaborated.

In a paper of 1920 Freud discusses the serious limitations of the retrospective method. He notes:

So long as we trace the development from its final outcome backwards, the chain of events appears continuous, and we feel we have gained an insight which is completely satisfactory or even exhaustive. But if we proceed the reverse way, if we start from the premises inferred from the analysis and try to follow these up to the final result, then we no longer get the impression of an inevitable sequence of events which could not have been otherwise determined. We notice at once that there might have been another result, and that we might have been just as well able to understand and explain the latter. The synthesis is thus not so satisfactory as the analysis; in other words, from a knowledge of the premises we could not have foretold the nature of the result.

A main reason for this limitation, Freud points out, is our ignorance of the relative strengths of different aetiological factors. He cautions:

Even supposing that we have a complete knowledge of the aetiological factors that decide a given result ... we never know beforehand which of the determining factors will prove the weaker or the stronger. We only say at the end that those which succeeded must have been the stronger. Hence the chain of causation can always be recognized with certainty if we follow the line of analysis, whereas to predict it along the line of synthesis is impossible (Freud, 1920b, *S.E.*, **18**, pp. 167-8).

This passage shows plainly that Freud was in no doubt what the limitations of the traditional method of inquiry are. Though a retrospective method provides much evidence regarding the kinds of factor that are likely to be aetiological, not only may it fail to identify all of them but it

is in no position to evaluate the relative strengths of those it does identify. The complementary roles in psychoanalysis of retrospective and prospective studies are indeed only a special instance of the complementary roles in other spheres of knowledge of the historical method and the method of the natural sciences.

Although in every kind of historical study the retrospective method has an established place and many and great contributions to its credit, the method's inability to determine the relative parts that different factors play in causation is an acknowledged weakness. Where the historical method is weak, however, that of the natural sciences is strong. As is well known, scientific method requires that, having examined our problem, we frame one or more hypotheses regarding the causes of the events in which we are interested, and do so in such a way that from them testable predictions can be deduced. On the accuracy of such predictions hypotheses stand or fall.

There can be no doubt that if psychoanalysis is to attain full status as one of the behavioural sciences, it must add to its traditional method the tried methods of the natural sciences. Whilst the historical method will always be a principal method of the consulting room (as it continues to be in all branches of medicine), for research purposes it can and should be augmented by the method of hypothesis, deductive prediction, and test. The material of this book is presented as a preliminary step in the application of this method. Throughout, the aim has been to concentrate on events and their effects on children, and to cast theory in a form which lends itself to predictions that are testable. To frame such predictions in detail and to test even a few of them are tasks for the future.

As both Rickman (1951) and Ezriel (1951) have argued, prediction and test can, if we wish, be employed during the treatment of patients; but such procedures can never test hypotheses about earlier development. For testing the

developmental theory of psychoanalysis, therefore, predictions made on the basis of direct observation of infants and young children, and often tested by the same method, are indispensable.

In employing this method it is necessary to begin by selecting a proposed aetiological factor to see whether it indeed has all or any of the effects attributed to it. This brings us to the second feature of the approach—the study of a particular pathogenic agent and its sequelae.

In considering Freud's views on this matter it is necessary to distinguish between his views on aetiological factors in general and his views on the role of the particular factor that has been selected for study here. We start with his general position.

When we examine Freud's views on factors that are causative of neuroses and allied disturbances we find that they centre always on the concept of trauma. This is as much so in his final formulations as in his earliest ones—a fact that has tended to be forgotten. Thus, in each of his very late works, *Moses and Monotheism* (1939) and the *Outline* (1940), he gives a number of pages to a discussion of the nature of trauma, the age-range during which the individual appears to be specially vulnerable, the kinds of event that may be traumatic, and the effects that they seem to have on the developing psyche.

Of these, it is the nature of trauma that is central to Freud's thesis. He concludes, as others have done, that there are two sorts of factor engaged—the event itself and the constitution of the individual experiencing it; in other words, that trauma is a function of interaction. When an experience evokes unusual pathological reaction, Freud argues, the reason is that it makes excessive demands on the personality; it does so, he postulates, by exposing the personality to quantities of excitation greater than it can deal with.

As regards constitutional factors, Freud supposes that individuals must vary in the extent to which they can meet such demands, so that 'something acts as a trauma in the case of one constitution but in the case of another would have no such effect' (*S.E.*, **23**, p. 73). At the same time, he holds, there is a particular phase of life, the first five or six years, during which every human being tends to be vulnerable. The reason for this, he believes, is that at that age 'the ego ... is feeble, immature and incapable of resistance'. In consequence, the ego 'fails to deal with tasks which it could cope with later on with utmost ease', and instead resorts to repression or splitting. This, Freud believes, is the reason that 'neuroses are acquired only in early childhood' (*S.E.*, **23**, pp. 184-5).

When Freud speaks of 'early childhood', it is important to remember he has in mind a period of several years; in *Moses* he refers to the first five years and in the *Outline* to the first six. Within this span, he thinks, 'the periods between the ages of two and four seem to be the most important' (*S.E.*, **23**, p. 74). The early months are not especially in his mind, and he expresses himself uncertain of their significance: 'How long after birth this period of receptivity begins', he writes, 'cannot be determined with certainty' (*S.E.*, **23**, p. 74).

This, then, is Freud's general theory of aetiology. The particular theory advanced here conforms closely to it. Separation from mother, it is argued, can be traumatic within the definition proposed by Freud, especially when a child is removed to a strange place with strange people; furthermore, the period of life during which evidence shows it to be traumatic coincides closely with the period of childhood that Freud postulates is specially vulnerable. The following brief sketch of how the views advanced about separation from mother fit Freud's concept of trauma affords an opportunity to outline the central thesis of this book.

Freud defines his concept of trauma in terms of causal conditions and of psychological consequences. In both respects separation from mother in the early years fits. As regards the causal conditions, separation in a strange setting is known to induce intense distress over a long period; this is in keeping with Freud's hypothesis that trauma results when the mental apparatus is subjected to excessive quantities of excitation. As regards consequences, it can be demonstrated that the psychological changes that regularly succeed the prolonged distress of separation are none other than repression, splitting, and denial; and these, of course, are precisely the defensive processes that Freud postulates are the result of trauma—are, indeed, the processes to account for which Freud advanced his theory of trauma. Thus, it can be shown that the aetiological agent selected for study is simply a particular example of the kind of event that Freud conceived as traumatic. As a result the theory of neurosis elaborated here is in many respects a variant only of the traumatic theory advanced by Freud.

Nevertheless it must be noted that, although separation from mother fits well with Freud's general theory of neurosis and, moreover, that separation anxiety, loss, and mourning are given an increasingly important place in his theorising, only on rare occasions does he single out an event of separation or loss in the early years as a source of trauma. When he refers to the sorts of event that can be traumatic Freud, in his later writings, is rather guarded; indeed, the terms he uses to describe them are so general and abstract that it is by no means always clear what he has in mind. For example, in *Moses and Monotheism* he states only that 'They relate to impressions of a sexual and aggressive nature, and no doubt also to early injuries to the ego (narcissistic mortifications)' (*S.E.*, **23**, p. 74). Admittedly, a commonly held view is that early separation is to be understood as an early injury to the ego; but,