

Perspective

An occasional series in which contributors reflect on their careers and interests in psychiatry.

A Contribution by John Bowlby

The Tavistock Clinic

In giving a brief account of salient events in my professional life I will start in 1937 when I was aged 30 and had just completed my formal training as a child psychiatrist and psychoanalyst. Apart from a medical background and an interest in psychology, my choice of career had been determined by what I had seen and heard during the six months that I had spent in a school for disturbed children between my pre-clinical training at Cambridge, where I had also read natural sciences and psychology, and completing my medical qualification at University College Hospital. During my time at the school I had worked with children and adolescents whose difficulties I know now to be typical of much personality disorder, and had been exposed to hypotheses, derived from the 'new psychology' emanating from Vienna, regarding the role of childhood experience in their origin. Accordingly I had decided to train as a psychoanalyst. This I began before qualifying medically and continued whilst spending eighteen months at the Maudsley, learning the psychiatry of adults as one of Aubrey Lewis's early students. This proved a productive relationship, not least because on many questions we agreed to differ.

Although my training as a psychoanalyst had been undertaken in the belief that Freud attributed the emotional problems of his patients to the traumatic experiences they had had within their families of origin during their early years, I gradually realized that he had abandoned that opinion long since and had concluded instead that the events he had believed important, had not occurred in reality but had been the products of his patients' imagination. This was then, and until recently has remained, the dominant view in the psychoanalytic world.

I thought otherwise. As a result of my experiences at the school for disturbed children and at the London Child Guidance Clinic, where from 1936 I trained and worked, I became convinced that many of the problems with which I was confronted, both in children and in adults, had their origin in faulty and sometimes disrupted relationships between the patient and his parents. This belief created problems. One was how to demonstrate a causal relationship to sceptical colleagues; another how to account for these sorts of experience having the kinds of effect on personality development I believed they had.

In by far the majority of cases I saw, I believed that the trouble lay in the way the parents treated the child or, in the case of adolescents and adults, how their parents had treated them in years gone by. In some it seemed evident that the parents had never wanted the child. In others it was clear that one or other of them had major emotional problems of

their own which, having originated during their own childhoods, had for many years been distorting their relationships with their children. In still others mistaken ideas about the dangers of spoiling or the efficacy of punishment were leading to consequences the reverse of what was intended.

At that time such ideas were intensely controversial, not only in psychiatric circles and among the general public but also among my psychoanalytic colleagues. A common charge was that in advancing these views one was merely scapegoating parents. Another, from traditional psychiatrists, that in any case there was no evidence. All the big guns were on the other side.

It was because of these difficulties that I decided to concentrate on the emotional and behavioural problems resulting from the prolonged separation of young children from their parents. My interest in these problems had been kindled whilst I was at the school for disturbed children. There, for example, I had known an adolescent boy who had been thrown out of a public school for repeated stealing. Although socially conforming, he made no friends and seemed emotionally isolated—from adults and peers alike. Those in charge attributed his condition to his having never been cared for during his early years by any one motherly person, a result of his illegitimate birth. Thus, I was alerted to a possible connection between prolonged deprivation of maternal care during early childhood and the development of a personality apparently incapable of making affectional bonds and, because immune to praise and blame, prone to repeated delinquencies. Since a number of children and adolescents we saw at the clinic seemed to conform to this personality pattern and also to have had very disrupted relations with mother or mother-substitute during their first five years, I collected a series of cases and demonstrated a significant correlation between this form of personality and a disrupted early experience. Although only a correlation, the very detailed histories I was able to obtain in some cases led me to believe that the association was truly causal.

From a research point of view to concentrate on cases of this type had great advantages. First, one was not necessarily attributing the child's condition to the behaviour of his parents, because some of the prolonged disruptions had resulted from events outside the parent's control. One such was the then current medical practice of isolating even very young children for months at a time in a fever hospital. Amongst my cases were a boy and a girl each of whom at the age of eighteen months had been admitted to an isolation hospital and having caught a succession of fevers had not emerged again until nine months later. In each case the

mother described how her child had not recognized her and had thereafter never made a close relationship either with herself or with any other member of the family.

A second research advantage was that basic information about these experiences could be obtained reasonably reliably. A third was that, if these experiences had the effects I supposed, there was the possibility of preventive action.

Then came the war and within six months I had said goodbye to child psychiatry. As an army psychiatrist for five years, I fought my battles from Salisbury Plain and the Heights of Hampstead. The reason for this was that I early became caught up in devising methods for the selection of officers, and after some months on a Selection Board was posted to the Research and Training Centre. There I had the task of discovering how successful or otherwise the new selection procedures were. In designing and conducting the follow-up, my mentor was Eric Trist, a clinical and social psychologist of great gifts. Others I got to know and to learn much from at the Centre were Wilfred Bion, Jock Sutherland and Ben Morris. My three years there were invaluable in giving me a postgraduate education in psychology and research method. Not only that but, by becoming associated with the group who were planning the post-war Tavistock, the ground was prepared for my subsequent activities.

Early in 1946 immediately after leaving the army, I was delighted to be asked to take responsibility for the children's department at the Tavistock Clinic, which I soon renamed the Department for Children and Parents. For the first two years my time was occupied in recruiting staff, organizing clinical services and developing training programmes for child psychiatrists, educational psychologists and social workers, followed by one for non medical child psychotherapists. I was also training secretary at the Institute of Psychoanalysis and took a part in devising the arrangements whereby the Institute provided two parallel courses, reflecting outlooks of the London and Viennese groups respectively. Not until 1948 was it possible to start on research again.

There being no money for research at the Tavistock, I managed to obtain a small grant to undertake a project in my chosen field, the effects on personality development of the separation of a young child from his mother-figure. This enabled me to appoint my first research assistant, James Robertson, a social worker who was already familiar with parts of the problem from having worked with Anna Freud at her war-time residential nurseries. One of his first activities was to visit a number of long-stay nurseries and hospitals to see whether we could locate a sample of children who had been inmates for long periods when younger and who could now be followed up. For various reasons this project was not particularly productive. What proved of more immediate interest and concern was the plight of the young children whom he saw on these visits. Whatever the long-term effects of such experiences might be, the short-term effects were both unmistakable and distressing. Robertson therefore

made a series of studies of young children, mainly between the ages of 12 and 36 months. His methods of recording were similiar to those of a field naturalist.

While Robertson was engaged on this work, I had accepted an invitation from the Chief of the Mental Health Section of the World Health Organization, who was familiar with my pre-war study, to write a report on the mental health of homeless children. This was a wonderful opportunity, since it enabled me to travel in Europe and the States, to meet colleagues in child care and child psychiatry, notably Anna Freud, David Levy, William Goldfarb and René Spitz, and to read the literature. My report, the product of six months hard labour, was published in 1951 as the monograph *Maternal Care and Mental Health*.

The broad conclusions of this report received wide acceptance amongst those familiar with the problem, though in other quarters, psychiatric, psychological and sociological, it gave rise to much controversy. Nevertheless, the ideas advanced were no longer eccentric, and research grants became less difficult to come by. That enabled me to appoint two new members of my research group, one experienced, Mary Ainsworth, and the other not, Rudolph Schaffer; both have subsequently made notable contributions to our understanding of early child development.

Meanwhile, Robertson was continuing his observations and had also made a film record of one child through an eight day stay in hospital. This film *A Two Year Old Goes to Hospital* (1952) provides a vivid illustration of the distressful responses we were studying. His detailed observations, made on a heterogeneous sample of cases, were the data on which, together, we generalized the sequence of responses commonly seen when a young child is cared for by strange people in strange surroundings—first, protest and an attempt to recover mother, secondly despair of doing so and depression and finally, emotional detachment from her. In addition, we drew attention to the child's acute fear after his return home lest he be sent away again.

These findings had a number of immediate applications to which Robertson devoted himself. A notable contribution was his evidence to the Platt Committee on the non-medical care of children in hospital which had a big effect on the Committee's recommendations. These included changes in hospital practice to enable mothers to stay with their children or else to visit them freely, instead of excluding them. Thereafter, the applied side being so well looked after, I was free to concentrate on the theoretical implications of our observations.

Although it was clear that many variables play a part in determining how a child responds to an event of the kind described, we felt confident that the most weighty was the loss of his mother-figure. But why should a young child be so distressed 'merely' by the loss of his mother? And why after return home should he become so apprehensive lest he lose her again? And what psychological processes could account

for his distress and for the phenomenon of detachment? Might answers to these questions, I asked myself, cast light on such clinical states as 'over-dependency', separation anxiety, depression and defence? My confidence that they might was strengthened by what I was observing in my clinical work. Similarities in the ways adolescent and adult patients respond to separation and loss, and the ways young children respond to a separation of the kind we were studying, were too obvious to be ignored. In all likelihood, I thought, these similarities were no accident but reflected a true identity of response overriding all differences of age.

It was with this varied collection of observations, clinical experiences and inferences in mind that I concluded that the first task for theory was to understand the nature of the child's tie to his mother. During the summer of 1951 a friend had mentioned to me the work of Lorenz on the following responses of ducklings and goslings, and this led me to ethology. Here, I found, a new world, one in which scientists of high calibre were investigating in non-human species many of the problems with which I was 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. Much of their work, moreover, was based on field observations of animals going about their daily business. Amongst other things, their work showed that in some animal species a strong bond to an individual mother-figure could develop without the intermediary of food, that it could develop rapidly during a sensitive phase early in life, and that it tended to endure. This provided an alternative model for consideration and one that had a number of features which seemed possibly to fit the human case. From that moment I was on a new track.

In the event, what I have done has been to look afresh at a broad range of empirical data, drawn from both the child development and the clinical fields, to see to what extent ethological principles can usefully be applied. The first fruits of my thinking appeared in a series of papers published between 1958 and 1963, of which the first was 'The nature of the child's tie to his mother' and the others on problems of separation anxiety and mourning. Next, I started on a book

to cover the same ground, only more thoroughly. Half way through, it became clear that a second volume would be needed; and half-way through that fission occurred again. In these three volumes, the last of which is just out (*Attachment and Loss III: Loss, Sadness and Depression*), I have found myself proposing a new theoretical framework for understanding problems of personality development and psychopathology, a framework different to those adopted either by my psychoanalytic colleagues or by learning theorists, though it incorporates many ideas from both. Among claims I make for it are that it sticks closely to the data, that though its concepts are psychological they are compatible with those of neurophysiology and developmental biology, and that it conforms to the ordinary criteria of a scientific discipline.

During the many years I have worked on this undertaking I have received an enormous amount of help from friends and colleagues, and also sympathetic treatment by my employers in the National Health Service and the Medical Research Council. Among members of my research group, James Robertson, Christoph Heinicke and Colin Murray Parkes have each played a key role in collecting and reporting basic data. Among those who have helped on theory construction, Robert Hinde has been pre-eminent. Mary Ainsworth has not only made invaluable studies of early development but has amplified theory in significant ways. I am deeply indebted to my colleagues in the Department for Children and Parents at the Tavistock who over many years encouraged and facilitated my work.

The greatest rewards for anyone who attempts research are to see his findings being applied and such further research as his ideas may engender. In both these respects I count myself fortunate. As regards practice, a greater understanding of young children has led to many changes in child care, while a grasp of how attachment as a concept differs from dependency is leading to a shift in therapeutic perspective. As regards science, however controversial my ideas may have been and still be, and however stringent the criticisms levelled at them, they are now being examined, amplified, modified and tested, possibly to destruction, by able workers in half a dozen disciplines. No one could ask for more.

The S. H. Foulkes Prize

The Group Analytic Society (London) would like to draw the attention of interested professionals working in the field of group psychotherapy to the S. H. Foulkes Prize to be given by the Institute of Group Analysis, Rome, for original work on group analysis. The prize is worth IL500,000 and will be awarded every six years on the occasion of an European Symposium. Papers, in English only, of 5,000 to 10,000 words should be submitted to: Dr Fabrizio Napolitani, Viale, Parioli 90, 00197 Rome, Italy by 31 March, 1981. Arrangements will be made for the publication of the paper.

ECT Project

I am grateful to all who, with variable enthusiasm, have responded to the ECT questionnaires. After two follow-up letters we have now reached 85 per cent response (by January this may be nearly 90 per cent). Computer analysis of the information collected has now begun, and we cannot accept any more Part A questionnaires. If there are any outstanding 3-month record sheets of patients, please complete and return these by 31 January 1981.

JOHN PIPPARD
Research Office