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## **John Bowlby and ethology : a study of cross-fertilization**

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**JOHN BOWLBY AND ETHOLOGY:  
A STUDY OF CROSS-FERTILIZATION**

FRANK C.P. VAN DER HORST

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# **JOHN BOWLBY AND ETHOLOGY: A STUDY OF CROSS-FERTILIZATION**

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*Voor Thijn & Janne*



“[R]eserves and misconceptions, inevitable when strangers from strange disciplines first meet, will recede and give place to an increasing grasp of what the other is attempting and why; to cross-fertilization of related fields; to mutual understanding and personal friendship.”  
(Bowlby in Foss, 1969, p. xiii)



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# **CHAPTER 1.**

## **GENERAL INTRODUCTION**



### **Roots of attachment theory**

In the late 1950s and early 1960s, British child psychiatrist Edward John Mostyn Bowlby (1907-1990) in a series of six papers (Bowlby, 1958c, 1960a, 1960b, 1961a, 1961b, 1963a) basically formulated what is now known as 'attachment theory'. He later elaborated his ideas in his trilogy *Attachment and loss* (Bowlby, 1969/1982, 1973, 1980a). Attachment theory, in which Bowlby tried to explain how and why children form bonds with their parents and caregivers, has been influential ever since its initial formulation.

Bowlby's theorizing on the mother-child relationship was the ultimate result of his interest in issues of separation. In her description of Bowlby's early life, Van Dijken (1998) has shown that the roots of this interest lie in his own early childhood, in experiences while working as a volunteer in several progressive schools, and in clinical observations when training as a psychoanalyst shortly before the Second World War. Bowlby was shaped by the psychoanalytic training he received from his supervisors Joan Rivière (1883-1962) and Melanie Klein (1882-1960), but he held different opinions about the influence of internal and external factors on child development and clinical problems. Bowlby's focus was more on observation of real life events and experimentation, while Klein emphasized "research limited to analytic sessions" (Bowlby, 1940a, p. 154) and unconscious fantasies as the origin of psychopathology. As a result of this theoretical disagreement, Bowlby's position within the *British Psychoanalytical Society* at one point was rather precarious (Van Dijken, Van der Veer, Van IJzendoorn & Kuipers, 1998; Van der Horst, Van der Veer & Van IJzendoorn, 2007). But by ignoring what he considered limited views of some of his psychoanalytic colleagues and taking an eclectic approach instead, Bowlby arrived at new and revolutionary insights. In her study, Van Dijken (1998, p. 161) concluded that "by combining and synthesizing the various viewpoints he accepted, Bowlby gradually developed his own view," a view that "was enriched by ethological insights and by Ainsworth's contribution".

This thesis builds on Van Dijken's findings and describes the 'ethological insights' that enriched Bowlby's view on the mother-child relationship. Starting point of the current study is the publication of Bowlby's (1951, 1952) report on maternal deprivation for the World Health Organization (WHO) published in 1951 and the many different issues of separation that Bowlby reported in this study. It will be argued that, eventually, these results led Bowlby to ethology as a new theoretical approach that could explain his observations of (separation in) children. The influence of Bowlby's thinking will be discussed, as well as the broader influence of research by ethologists and animal psychologists. First, for a better understanding of what Bowlby sought in ethology, in the next paragraph an overview of the rise of ethology as a new discipline will be given.

### **The rise of ethology as a discipline**

On December 12, 1973 the *Nobel Prize in Physiology or Medicine* was awarded to three scientists who had devoted their academic work to the study of animal behavior. Karl von Frisch (1886-1982), Konrad Lorenz (1903-1989), and Niko Tinbergen (1907-1988) were distinguished "for the creation of a new science – ethology, the biological study of behaviour" (Hinde & Thorpe, 1973). The word ethology, from the Greek *ἦθος* (ethos) meaning character or custom and *λόγος* (logos) meaning word or description, has been traced back as far as

the seventeenth century, but its current meaning as the scientific study of (animal) behavior was only attributed to it in the first quarter of the twentieth century (Jaynes, 1969). According to Lorenz (1981, p. 1)

[e]thology, the comparative study of behavior, is easy to define: it is the discipline which applies to the behavior of animals and humans all those questions asked and those methodologies used as a matter of course in all the other branches of biology since Charles Darwin's time.

Until the beginning of the previous century animal behavior was explained by using the concept of 'instinct', though there was no clear description or understanding of what that concept implied. In *On the origin of species* (1859), Charles Darwin (1809-1882) already used the term as one of the pillars of evolutionary theory: instinct was a characteristic that was influenced by natural selection just as morphology was. Instincts had to be adaptive to give the organism an advantage in its environment. After Darwin though, the analogies between animals and humans were mainly studied by (comparative) psychologists in an effort to understand the behavior and psyche of animals. It was presumed, for example by behaviorists, that the regularities found in animal behavior hold for humans as well. In their studies evolution and adaptivity to the environment were largely ignored. It was only during the 1920s that zoologists put evolution and adaptivity of instincts back on the agenda. The people responsible for this change of focus, the forerunners of ethology, were Whitman and Craig in the United States, Selous and Huxley in Britain and Heinroth in Germany (Roëll, 2000; Kruuk, 2003; Burkhardt, 2005).

Charles Otis Whitman (1842-1910) was an American biologist who, just as many other ethologists *avant la lettre*, was fascinated by animal life and ornithology from an early age. He advocated a broad approach to biological research, including observation and experimentation. His basic assumption in interpreting behavior was that "instinct and structure are to be studied from the common standpoint of phyletic descent" (Whitman, 1899 in Roëll, 2000, p. 28). Animal habits should thus be studied in the same scientific manner that anatomy and morphology were and behavior should be seen from an evolutionary viewpoint. Whitman's influence on European ethology was mainly indirect through his student Wallace Craig (1876-1954), who corresponded extensively with Lorenz between 1935 and 1937 on Whitman's ideas. This new approach to the study of 'instinct' made no headway in the United States in this early period though (Roëll, 2000; Burkhardt, 2005).

In Europe, the new study of instincts and animal behavior did find fertile soil. In England, naturalist Edmund Selous (1857-1934) followed Whitman's scientific tradition of studying animals: "the habits of animals are really as scientific as their anatomies" (Selous, 1905 in Burkhardt, 2005, p. 92). Selous was praised by colleagues for his detailed observations of bird behavior. His pioneering work inspired Julian Huxley (1877-1975) in England and Tinbergen's mentor Jan Verwey (1899-1981) in the Netherlands to do field studies of their own (Roëll, 2000). In Germany Oskar Heinroth (1871-1945) had great influence on the development of ethology as a scientific study through his close contacts with Lorenz. Heinroth was director of an ornithological field station and was fully devoted to

making descriptions of bird behavior. In the 1930s, he and Lorenz had much contact on comparative studies of behavior; eventually it was Lorenz who would lay the theoretical foundations for their new approach (Roëll, 2000; Kruuk, 2003; Burkhardt, 2005). Lorenz attributed the founding of ethology to the decisive discovery made by Whitman, Heinroth and himself “that movement patterns [of different species] are homologous” (Lorenz, 1981, p. 3). From that time the study of behavior was approached in the same manner as the study of morphology of animals was. In the following years, Lorenz as the theorist and Tinbergen as the more empirically-minded researcher would lay the foundations of this new discipline.

Lorenz and Tinbergen first met at a symposium on the concept of instinct in Leiden in November 1936. They had started corresponding the year before and Tinbergen used Lorenz’s (1935) very influential work *Der Kumpan in der Umwelt des Vogels* in courses he taught at Leiden University (Roëll, 2000). After their first meeting, both men felt that they were personally and intellectually connected, especially because the work of the one so wonderfully complemented that of the other. According to Tinbergen (1974, p. 198): “Lorenz’s extraordinary vision and enthusiasm were supplemented and fertilised by my critical sense, my inclination to think his ideas through, and my irrepressible urge to check our ‘hunches’ by experimentation”. In the year following their first encounter Tinbergen would spend some months at Lorenz’s home in Altenberg where they carried out simple experiments with various animals. Their subsequent friendship was to be decisive for the development of ethology as a new approach. Here we will discuss this development from the mid-1930s to the early 1950s – approximately the time Bowlby’s attention was first drawn to its relevance for studies of human behavior – by briefly discussing Lorenz’s (1935) *Der Kumpan* and Tinbergen’s (1951) *The study of instinct*. These works give a far from complete picture of ethology, but they do account for the ethological notions Bowlby was provided with. It was the English translation of *Der Kumpan*, published in the American ornithological journal *The Auk* (Lorenz, 1937), that set Bowlby on track for his interest in ethology as a framework for his ideas on separation in 1951 (Bowlby, 1969/1982, p. xviii; Bowlby, Figlio & Young, 1986; Ainsworth & Bowlby, 1991; Hinde, 2005). Tinbergen’s *The study of instinct* appeared in the same year as “ethology’s first major text” and “a benchmark for how far ethology had come” (Burkhardt, 2005, p. 371). Bowlby’s attention was immediately drawn to it (Van der Horst et al., 2007).

### **Lorenz’s *The companion in the bird’s world***

Lorenz’s main contribution to ethology is the formulation of several key concepts in his most influential work, *Der Kumpan in der Umwelt des Vogels* (Lorenz, 1935), later translated into English as *The companion in the bird’s world* (Lorenz, 1937). It was basically Lorenz’s attempt to summarize his ideas up to that point and to provide others with a theoretical framework for comparative research of animal behavior. It came to be regarded as an impressive and authoritative work, receiving very favorable reviews in the United States and England, from American psychologist Margaret Morse Nice (1883-1974), Craig, and Huxley amongst others. One could say that Lorenz earned himself an international reputation with its publication.

*Der Kumpan* made use of many concepts that had earlier been introduced by German physiologist Jacob von Uexküll (1864-1944), with whom Lorenz cooperated closely in the 1930s. The central concept in *Der Kumpan* is the 'social releaser', a stimulus that elicits instinctual behavior (more specifically those features of a fellow member of the same species an animal reacts to). Lorenz assumed that lower animals such as birds are not adapted to their environment by learned behavior – as humans are – but that they make use of differentiated instinctive behavior patterns. These patterns have been built up during evolution because of their survival value. These instinctive behaviors only have to be 'released' or triggered by the environment. The reaction to a specific releaser is laid down in an 'innate releasing mechanism' (IRM) in the organism. The structured pattern of movements that follows a releaser is called a 'fixed action pattern' (FAP). This is the genetically programmed core of a species typical behavior, it is a highly stereotyped innate movement pattern based on activity in a specific coordinating centre in the central nervous system. A FAP runs to completion regardless of further stimulation. With these concepts, Lorenz linked external stimuli with the internal, innate behavior patterns of the animal. In an animal's social life Lorenz identified several releasers of instinctual behavior, so-called *Kumpane* or companions: the parent-companion, the child-companion, the sex-companion, the social-companion and the brother-and-sister-companion. Lorenz's description of the IRM made it possible to make a clear distinction between instinctual and learned behavior (Lorenz, 1935, 1937; cf. Roëll, 2000; Burkhardt, 2005; Hinde, 2005).

Probably the most interesting concept Lorenz described in *Der Kumpan* was a phenomenon that was neither instinctive nor learned. Lorenz narrated how young graylag goslings (*Anser anser*) and jackdaws (*Corvus monedula*) do not recognize members of their own species directly after birth, but show a strong following response to the first moving object in their surroundings. He named this response 'imprinting'. The concepts of imprinting, companion, and releaser are closely related: because the animal does not instinctively recognize members of its own species, imprinting provides it with this information in a sensitive period directly after birth. In this sensitive period a preference for members of the own species is established and hereby companions in the environment become able to elicit instinctive behaviors (Lorenz, 1935, 1937).

### **Tinbergen's *The study of instinct* and the four whys**

In 1951, the year Bowlby first turned to ethology, Tinbergen published his seminal work on *The study of instinct*, in which he described the state of the art in ethology. Though published while working at Oxford University, the book is a reflection of Tinbergen's ideas and research from his time in Leiden and "an extension of a series of lectures delivered at New York in February 1947" (Tinbergen, 1951, p. v). *The study of instinct* was of great importance to the field as "it was this book that put ethology on the map" (Kruuk, 2003, p. 149). Central in Tinbergen's book are the 'four whys' or four questions regarding the behavior of animals. These four questions related to the causation, the ontogeny, the function, and the evolution of instinctive behavior. Tinbergen's focus was the question of the causation of innate behavior, mainly because to that point it had been the focus of research by him, Lorenz, and others.

To understand the causes of innate behavior, Tinbergen proposed a hierarchical organization of instinctive behaviors. Tinbergen also differentiated between influences on the behavior of the organism by external factors (such as sensory stimuli or sign stimuli) and internal factors (what Tinbergen called “physiological mechanisms”: e.g. hormones, internal sensory stimuli, and intrinsic or automatic nervous impulses generated by the central nervous system). He stated that the internal factors controlled the motivation of the organism and the so-called appetitive behavior (e.g., looking for food or courtship patterns prior to mating). Also, the internal factors determined the threshold needed to release the instinctive behavior. But the behavior is not elicited without external factors unblocking the IRM and releasing the actual consummatory act (as laid down in a FAP). Tinbergen (1951, p. 103) exemplifies this reasoning with an account of the reproductive behavior of the male three-spined stickleback (*Gasterosteus aculeatus aculeatus*):

In spring, the gradual increase in length of day brings the males into a condition of increased reproductive motivation, which drives them to migrate into shallow fresh water. Here... a rise in temperature, together with a visual stimulus situation received from a suitable territory, releases the reproductive pattern as a whole. The male settles on the territory, ... it reacts to strangers by fighting, and starts to build a nest. Now, whereas both nest-building and fighting depend on activations of the reproductive drive as a whole, no observer can predict which one of the two patterns will be shown at any given moment. Fighting for instance, has to be released by a specific stimulus, viz. ‘red male intruding into the territory’. Building is not released by this stimulus situation but depends on other stimuli. Thus these two activities, though both depend on activation of the reproductive drive as a whole, are also dependent on additional (external) factors. The influence of these latter factors is, however, restricted, they act upon either fighting or building, not on the reproductive drive as a whole.

In this example the reproductive behavior is the appetitive behavior that builds up due to internal factors and leads to a decrease of the threshold. The instinctive behavior, though, is only elicited by external factors (e.g., a male intruder) and this external stimulus unblocks the IRM and results in a FAP (namely fighting). The behavior itself takes away the motivation for the animal to strive for the stimulus. The hierarchical coordination of different IRM’s results in suppression of other behavioral systems when a specific behavioral system is activated. In some instances, different and conflicting drives are activated (e.g., fleeing and fighting). In these cases displacement activities may occur (such as nest building or courting behavior towards a male intruder).

The topic of the causation of behavior took up more than half of the book. Tinbergen touched upon the three other questions, but in much less detail. Nevertheless, *The study of instinct* is generally seen as the work that “brought order in the perceived chaos of behaving animals” (Kruuk, 2003, p. 149) and that explained animal behavior in all its dimensions. Its huge impact was mainly due to Tinbergen’s all-embracing approach. Later,

many of his ideas were dismantled and would be replaced with new concepts and views, but for the time being Tinbergen had made ethology count.

### **Sources of information**

The findings in this thesis are based on many different sources. Of course, we relied heavily on the original writings of Bowlby and many of the ethologists he interacted with. Also, experts of attachment theory (e.g., Robert Hinde, Stephen Suomi, Joan Stevenson-Hinde, Howard Steele, Everett Waters, and Inge Bretherton) were willing to be interviewed on the cross-fertilization of ethology and attachment theory, each addressing the issue from their own expertise and perspective. Another important and very rich source of information were archival materials, mainly located at the Wellcome Library for the History and Understanding of Medicine. The Archives and Manuscripts section there holds Bowlby's personal archive since the death of Bowlby's wife Ursula in 1999. Harry Harlow's personal papers were available through Helen LeRoy, who has been very helpful in providing us with his correspondence and was willing to be interviewed on Harlow's role in attachment theory as well. Others were kind enough to provide us with some of Bowlby's correspondence (e.g., Adriaan Kortlandt, Joan Stevenson-Hinde). Of great value were the issues of the *British Medical Journal* and *The Lancet*, which contained several of Bowlby's letters but also many articles and letters by his colleagues in the medical world who reflected upon his work.

### **Aims of the current study**

The general aim of this thesis is to describe the cross-fertilization of attachment theory and ethology. The study has three specific aims:

1. to describe the several different issues of separation that Bowlby reported in his report for the WHO and that could not be explained with the psychoanalytic ideas that he had been familiar with to that point;
2. to give an account of the importance of ethology as a new framework for Bowlby to explain mother-child interactions in early life and (more specifically) the role Robert Hinde played in this regard;
3. to narrate the interaction between John Bowlby and Harry Harlow and the importance of the empirical evidence provided by Harlow's studies on separation in rhesus monkeys.

### **Outline of the present thesis**

The outline of this thesis is as follows. In *Chapter 2* different issues of separation of young children that Bowlby encountered during the late 1930s, 1940s, and early 1950s are discussed. These issues include separation due to war-time evacuations, observations made in residential nurseries, the discussion concerning visiting of children in hospital, results of clinical studies, and studies on the so-called 'hospitalization' effect. This description of "unexplained observations" is followed by an account of the cross-fertilization of ideas of Bowlby and various leading European scientists in the field of ethology in *Chapter 3*. From the 1950s Bowlby was in personal and scientific contact with the likes of Tinbergen, Lorenz and Hinde and he used their new viewpoints and theoretical framework to explain his earlier observations and to construct his new theory on attachment. These ethologists in

their turn were influenced and inspired by Bowlby's thinking. Attention will be paid to Bowlby's influence on ethological studies in general and on Robert Hinde's work more specifically. After a short intermezzo, *Chapter 4* will show how Bowlby made the move from theoretical claims to empirical evidence through his interactions with American animal psychologist Harry Harlow, with whom he was in close contact from 1957 through the mid-1970s. Bowlby profited highly from Harlow's experimental work on the effects of separation in infant rhesus monkeys. Here again, an attempt is made to delineate the cross-fertilizing aspect of the interaction by showing that Harlow in his turn was influenced and inspired by Bowlby as well. *Chapter 5*, based on interviews conducted with Harlow's student and attachment expert Stephen J. Suomi, is a comprehensive illustration of the influence of ethology and animal research on attachment theory in recent studies and vice versa. Finally, in *Chapter 6* the evidence presented in this thesis will be integrated and discussed. Here we will address the issue of Bowlby's scientific descent: was it Freudian or Darwinian?



## CHAPTER 2.

# LONELINESS IN INFANCY: JOHN BOWLBY AND ISSUES OF SEPARATION

Parts of this chapter were published as:

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## **Introduction**

In attachment theory, John Bowlby attributed potentially harmful effects to separation of a child from its mother or mother-substitute. Bowlby stated that “young children, who for any reason are deprived of the continuous care and attention of a mother or a substitute-mother, are not only temporarily disturbed by such deprivation, but may in some cases suffer long-term effects which persist” (Bowlby, Ainsworth, Boston & Rosenbluth, 1956, p. 211) and that a “rupture leads to separation anxiety and grief and sets in train processes of mourning” (Bowlby, 1961b, p. 317). Bowlby’s whole career was focused around the theme of separation and its consequences and he was fairly single-minded in that sense. The roots of Bowlby’s interest in issues of separation have been extensively documented by Van Dijken (1997, 1998; cf. Van Dijken et al., 1998) and lie in his own early childhood and in clinical experiences when training as a psychoanalyst shortly before the Second World War.

Although the importance of different observations of the consequences of separation for Bowlby’s thinking and for the development of attachment theory is self-evident, so far little attempt has been made to give a complete overview of the different studies on the effect of separation and deprivation that drew the attention of many in the 1940s and 1950s and to which Bowlby was exposed. This chapter is an attempt to do so. What exactly was known or believed about separation effects shortly before, during and after the Second World War, when Bowlby wrote his first letters to scientific journals and published his first articles? Here we may distinguish between findings from several different but interconnected areas. Attention will be paid to observations made during wartime evacuations and in residential nurseries, to the discussion concerning visiting of children in hospital, to results of clinical studies by the so-called ‘English school’ of psychoanalytically oriented psychiatrists and psychologists, and, finally, to results of studies on the ‘hospitalization’ effect. It will be argued that Bowlby met with and was heavily influenced by leading researchers in the field of psychology and psychiatry while working on his report for the World Health Organization (WHO). Finally, we will also take a closer look at films by Spitz (1947) and Robertson (1952, 1958c) that supported these new ideas on the effects of maternal deprivation and greatly influenced public opinion – at least in Britain.

From a discussion of these different ‘issues of separation’ it will become clear how, in the 1940s and 1950s, Bowlby gathered (retrospective) evidence for his views on the early mother-child relationship that would refute classic psychoanalytic views. Shortly before he first came across ethology in 1951, Bowlby (1951) summarized his findings on separation and deprivation in a report for the WHO. He eventually turned to the ethological perspective to explain his observations on the influence of early environment on the development of children.

## **Issues of separation: Evacuation of children**

Sadly enough, the Second World War supplied psychologists and psychiatrists with many opportunities to observe the effects of parent-child separations. As early as 1924 a committee chaired by John Anderson started to lay out plans for the evacuation of children in case of aerial bombing by a ‘belligerent’ force. These evacuations were part of the so-called Air Raid Precautions (ARP) and were necessary because at that time there was no

efficient way of stopping air attacks. The official evacuations started on E-day, September 1, 1939 – the day of the German invasion of Poland and two days before the British declaration of war. Within days, 734,883 unaccompanied children were evacuated from the London area to the countryside (Editorial, 1940). Immediately, details of this operation and its effects on the children started to fill the editorial and correspondence columns of the leading medical journals – the *British Medical Journal* and *The Lancet*. On September 9, an editorial (1939a) hailed the “successful exodus” of the evacuated children. On November 1, a discussion in the House of Lords led to the conclusion that “the evacuated children were happy and were gaining in health. Very often the hosts, too, were happy” (Editorial, 1939b, p. 977).

Not everyone was satisfied though. General practitioners warned against the dangers (spreading of vermin, uncleanliness) and undesirable social effects (Carling, 1939; Evans, 1939; Prance, 1939; Thursfield, 1939). In reception areas, people felt that “the scum of the town ha[d] been poured into the clean countryside” (Keir, 1939, p. 745). Also, it soon turned out that from a psychological viewpoint the evacuation of children was not a complete success. Frequent bed-wetting and other nervous symptoms were often observed in evacuated children. Feelings of concern about the emotional well-being of the children were expressed. Rickman (1939, p. 1192), in a letter to *The Lancet*, expressed his doubts about the plan to separate a child from two to five from its mother, because “at a time when his need for security, and the comforting assurance of familiar faces, is great, his removal from his parents will tax him severely... [and] may show [itself] in unsatisfactory or unhappy social relationships later in life”. In the *British Medical Journal* psychoanalysts Donald Winnicott, Emmanuel Miller, and John Bowlby protested against the evacuations for similar reasons:

It is quite possible for a child of any age to feel sad or upset at having to leave home, but... such an experience in the case of a little child can mean far more than the actual experience of sadness. It can in fact amount to an emotional ‘black-out’ and can easily lead to a severe disturbance of the development of the personality which may persist throughout life. [E]vacuation of small children without their mothers can lead to very serious and widespread psychological disorder. For instance, it can lead to a big increase in juvenile delinquency in the next decade. (Bowlby, Miller, & Winnicott, 1939, pp. 1202-1203)

Clearly, here Bowlby and his colleagues referred to Bowlby’s (1944, 1946) early work on the ‘forty-four juvenile thieves’. They may have somewhat overstated their case, but for many children the sudden evacuation was indeed traumatic (cf. Wolf, 1945, for an attempt to summarize the findings). Many years later, Wicks (1988) gathered the often moving memories of persons who spent part of their childhood as an evacuee.

### **Issues of separation: Observations in residential nurseries**

While many children during the Second World War were billeted with private persons, others ended up in residential nurseries, for example, because they lost their parents in an air raid. The great authority in this area became Sigmund’s daughter Anna Freud, who together with Dorothy Burlingham published various books on her experiences with young children in the

Hampstead Nurseries (Burlingham & Freud, 1942, 1944; cf. Freud, 1973). Their often moving accounts “describe the wholly admirable administration of a group of three nurseries (two residential and one for day children)” and include “– with an endearing lack of technical terms – an account of child development and psychopathology so simple and yet so profound that the unlearned in psychology and the experienced psychiatrist alike may read it with enjoyment and profit” (Editorial, 1942b). An example of such a moving account is their description of Dell, a little girl of two-and-a-half years old:

Dell was a beautiful little girl, ... sparkling with life and gaiety... Dell was taken to the nursery where she was deep in play after a few minutes. She said good-bye to her mother in a friendly way, but hardly noticed when her mother left her. Only half an hour [later]... Dell suddenly realized what had happened. She interrupted her play, rushed out of the nursery, and opened every single door... to look for her mother... This lasted a few minutes and then she rejoined the play group. These attacks of frantic search repeated themselves with ever greater frequency. Dell's expression changed, her brightness disappeared, her smile gave way to a... frown which changed the whole aspect of the child. (Freud, 1973, pp. 36-37)

In their studies, Burlingham and Freud posited that it is of the utmost importance for the child's personality formation and the development of consciousness to develop attachments with (substitute) adult persons. The logical people to play this role in the life of residential children are the grown-ups of the nursery. If these grown-ups remain remote and impersonal figures, or if they change so often that no permanent attachment can be formed, there is great danger that the children will show defects in their character development and inadequate adaptation to society, Burlingham and Freud (1944, pp. 105-6) argued. They concluded:

Residential Nurseries offer excellent opportunities for detailed and unbroken observation of child-development. If these opportunities were made use of widely, much valuable material about emotional and educational response at these early ages might be collected and applied to the upbringing of other children who are lucky enough to live under more normal circumstances. (Burlingham & Freud, 1944, p. 108)

To the editors of the *British Medical Journal* it was clear “that these [Hampstead] nurseries are run with an efficiency, devotion, and human understanding that should serve as a model for others, whether in time of peace or of war” (Editorial, 1942b). In subsequent years, Freud would repeatedly intervene in a debate concerning visits to children in hospital, warning against the psychological dangers of separations (cf. Editorial, 1944, 1949; Robertson, 1956). We will now turn to this debate.

**Issues of separation: Visits to children in hospital**

In January 1940, *The Lancet* published an editorial in which it was announced that *Ayr County Hospital* had decided to no longer admit visitors to its children's wards. The editor argued that the danger of infection indeed made forbidding access logical and added the then very common argument that parental visits only upset the child. He was sure that children quickly settle in the hospital and "cheerfully adopt the... staff *in loco parentis*" (Editorial, 1940, p. 179). It was not the children who needed parental visits, the editor argued, but the "over-anxious mother" (ibid.). However, parental stress could be alleviated by interviews with staff and an occasional peep when the child was asleep. The editor concluded his account by stating that in these matters sentiment was not a weighty enough argument. It was Bowlby (1940b) who first reacted to this editorial note. In a letter to the editor he argued that, although more research was needed, there was reason to assume that visiting was essential, especially for younger children. He suspected that non-visiting might lead to chronic delinquency in children and mentioned an antisocial boy of six and a pilfering girl of eight from his practice, both with a history of unvisited hospital stay. Referring to Rickman (1939), he suggested that the younger the child, the more visits are needed. Two weeks later, Edelston (1940), one of Bowlby's colleagues at the London Child Guidance Clinic, supported his argument and stated that, he too, had seen children in the Child Guidance Clinic who suffered from prolonged hospital stays (see below). Edelston added that children's quiet attitude may be deceptive, because they may repress their feelings until they come home. Edelston would several times intervene in debates in *The Lancet* about visiting times in children hospitals, stating the possible harmful effects of separation experiences but at the same time emphasizing that they are not inevitable (Edelston, 1941, 1946, 1953, 1955, 1958).

These letters by two child psychiatrists seem to have had no effect whatsoever on hospital practice. The majority of hospitals vehemently opposed (frequent) visiting by parents for a variety of reasons. Parents brought filthy germs into the wards and only upset their children, who would be crying for hours after they left causing the nursing staff much trouble. Parents only wished to visit their children for egocentric reasons; they were being over-anxious and neurotic. The children themselves certainly did not need the visits; they quickly felt at home in the hospital. Besides, even if a child was not happy – and some doctors and nurses admitted that these children existed – it was always better to have a sad child than a dead child. Taking the viewpoints of the parents, it was also suggested that many parents had no wish or time to visit their children, for example, because they had to travel a long time to the hospital, or there were other children to take care of. And who would make father's tea when he got home from work? (Herzog, 1958a, 1958b; Meadow, 1964; Schoo, 1954) Apparently, parents were seen as ignorant and noisy intruders who only criticized the staff and disturbed the quiet and disciplined course of events in the ward. Meanwhile, the parents themselves had few possibilities to change the existing situation. Even if they had been eloquent and knowledgeable enough and realized that something was awry, there was little that they could do to oppose the medical doctors who had allegedly introduced all those rules to the benefit of their child. In sum, the emotional problems of isolated children in hospital were not appreciated or considered serious enough. And even if

the problems were acknowledged there were always weighty grounds to oppose any change of the existing regulations.



Figure 1. A child in hospital.

What many British people did not know at the time was that both in Britain and abroad other models of child care in hospital were being practiced with considerable success. In 1945, the readers of the *British Medical Journal* first heard of an experiment that had been going on for quite some time in New Zealand. In that year, Henry and Cecile Pickerill, plastic surgeons in Wellington, first described their new method of dealing with the dangers of cross-infection, a method they had already introduced in 1927. Over the next decade, the Pickerills would repeatedly discuss their approach in both the *British Medical Journal* and *The Lancet*, claiming its unprecedented success, and actively participate in the debate about child care (e.g., visiting regulations) in hospital (Pickerill, 1955a, 1955b; Pickerill & Pickerill, 1945, 1946, 1947, 1954, 1954a, 1954b, 1954c, 1954d). Other writers in these journals regularly referred to the Pickerills' approach and were obviously well acquainted with it. What was that approach? As the Pickerills (Pickerill & Pickerill, 1945) explained, they sought to create an environment where the child would be protected against the danger of cross-infection. To this goal, a separate surgical unit was built with accommodation for 12 mother-child pairs. For, contrary to other approaches, the Pickerills wished to isolate the infant or young child with its mother. The rationale of that idea was their

belief that “a baby is born with a certain degree of passive immunity to its mother’s organisms, and that it acquires further immunity in the next few months... [and that it should be exposed] to no other organisms whatever”. After surgery, the contact between the medical staff and the baby was minimal and mothers took care of the lion’s share of the care of their children. Although the Pickerills stressed the importance of isolation (or rather insulation), they did mention other factors relevant for our account. In 1946, they expressed their opinion that mothers should be happy when taking care of their babies and not “reduced to a nervous wreck by an autocratic ward sister” (Pickerill & Pickerill, 1946). One year later, in their reaction to Spence’s paper (see below), they added that “babies want constant attention and ‘mothering’; to break the bond between mother and baby is to introduce an unnecessary hurdle into treatment” (Pickerill & Pickerill, 1947). From their articles and letters, it also transpires that they wished to create a healthy and happy environment for mothers and children with plenty of sunshine and good food. Later researchers, e.g. Mac Keith (1953), would dismiss the insulation idea as irrelevant and claim that it was the continuous presence of the mother that accounted for the success of their approach. However, it may have been exactly the ‘unsentimental’ aspect of their approach that made it acceptable in medical circles.

In May 1945, the readers of *The Lancet* were able to take note of a letter that was unusual in two respects. First, it was written by a parent. Second, it addressed the issue of social class. The letter was written by Lady Patricia Russell, the third wife of the philosopher Bertrand Russell. She related that she had just returned from America when her 7-year old son Conrad, the later historian and politician, suddenly developed a high fever and had to be admitted to the local hospital. Russell wished to stay with her son but was told to leave at once. This she refused to do. When the doctors arrived after 12 hours, they accused her of bringing “filthy germs” from the United States. Russell left for the night but when she returned the next day her son told her that when he asked for his mother, the nurse “threatened to smack him and removed his teddy-bear”. What made Lady Russell’s letter even more shocking was her observation that as soon as the medical staff realized who she was, she was immediately treated with the utmost courtesy. Apparently, she suggested, “the gross neglect, rudeness, and enforced separation” were reserved for the members of the lower social classes. Russell opposed the existing visiting rules with the following words:

I feel very strongly that when children are patients in hospitals some member of their family should be allowed to remain with them whenever this is possible... to restrict parental visits to two days a week, as in this hospital, is inhuman. (Russell, 1945a)

Russell’s letter elicited rather vehement reactions. Nicholson (1945) and Foster (1945) claimed her story could not be true. Batten (1945) expressed as his opinion that “everybody would deplore the continual presence of a mother at the bedside of a sick child”, and Bliss (1945) wondered whether she was a socialist. However, she also received support from correspondents (Cantab, 1945; Hardy, 1945; Nicholls, 1945) and, most importantly, from the editors of *The Lancet*, who claimed her account was not unique. According to the editors

(Editorial, 1945), removing the teddy-bear was to deprive the boy of his last link with the security of home. Doctors should place themselves in the shoes of the child and its mother. The hospital should always be able to arrange for the mother to stay in comfort if she is needed, and the existing rule should become much more flexible. Kindness, comfort, and attention were the keywords, according to the editors. In her follow-up letter, Russell told she had received many letters with similar stories and once more argued that visiting rules should be relaxed. One of her arguments was that “studies of evacuated children have abundantly proved that young children may be gravely harmed by enforced separation from their parents” (Russell, 1945b).

Russell’s letter was important, because it pointed out a social evil – private patients and their relatives were treated much better and could arrange flexible visiting times – and because her plea for more humane regulations was supported by the editors of one of the most important medical journals of Britain. Of course, much of the problems in this period could be excused by saying that there was a war going on. The nursing staff was underpaid, overburdened, and often unqualified. No wonder they were rude to parents and did not wish to see hordes of parents rushing into the hospital. Such excuses were valid to a degree, but there was more to it. By training and tradition doctors and nurses had never learned to take the viewpoint of the child patients and their parents. It would need very forceful descriptions and eventually films to open their eyes to the feelings of bewildered and frightened young patients. A veritable milestone in this respect was Spence’s (1947) famous lecture on the care of children in hospital. Spence’s description of children’s wards is worth quoting at some length.

The room is vast... The roof is... terrifyingly remote to the eyes of a child who lies many hours gazing at it. Some of the beds are three feet from the ground... to the discomfort of the child who has not slept so far from the ground before... The beds stink just a little... [He conceals] his personal treasures under his pillow until they are again put out of his reach... A plaintive 2-year-old standing behind the bars of his cot clad in a shapeless night-gown with a loose napkin sunk to his ankles below... Night comes on, but there is no bedtime story, no last moment of intimacy, no friendly cuddle before sleep. The nurse is too busy for that... This daily rhythm of anxiety, wonder, apprehension, and sleep is better than it sounds, because it is made tolerable by the extraordinary resilience and gaiety of the children... But it is a deceptive cheerfulness. (Spence, 1947, pp. 127-128)

Spence followed up on his description with a number of practical recommendations. Among other things, he proposed that a number of rooms in each hospital should be special mother-child suites where the mother could live with, nurse, and care for her own child. Thus, he suggested “admit[ting] the mothers to the hospital to nurse their own children. This is no theoretical proposal. I have worked under this arrangement for many years... the majority of all children under the age of 3 derive benefit from it. The mother lives in the same room with her child” (Spence, 1947, p. 128). Spence argued that having such suites would bring many

advantages: the mothers would gain confidence, nurses would learn how to handle children, students would learn courtesy, nurses would have more time for other duties, and so on.

What Spence for some reason did not do in his lecture was to spell out his own experiences with mother-child suites. But the fact of the matter was that he had been practicing this arrangement since he founded the *Babies' Hospital* in Newcastle upon Tyne in 1925. Spence's masterly description of a children's ward and his recommendations for improvement would serve as a model for those who championed a more humane child care in hospitals in the decades to come. Judging by the many references to his work, he came to be seen as one of the principal figures in the debate about child care in hospital.

Meanwhile, the few immediate reactions to the published version of his lecture were not altogether positive. Crosbie (1947) suggested mothers were too busy to take care of their sick child in the hospital and Lorber (1947) claimed he tried Spence's suggestion only to find out that mothers had other children to take care of, or were ill themselves. The Pickerills came to Spence's rescue and suggested that in "extreme cases" a granny could replace the mother. And, of course, they could not help to note that Spence "approves what we did as much as possible for the last 20 years and exclusively for the past 6 years" (Pickerill & Pickerill, 1947).

Spence's lecture was followed by an article by Maclennan (1949) two years later. In that article, she argued that discipline was too harsh in hospitals, that there was an undue emphasis on cleanliness that thwarted the child's natural instincts. Maclennan complained that nurses knew little about child psychology and that the child's emotional needs were ignored when he was "perhaps for the first time in his life, [separated] from the people he loves and from the familiar home atmosphere". Maclennan then proposed a number of very sensible measures: the children should not be left alone too much; ideally, one nurse should take care of one child; children should have the possibility to play; nurses should know something about developmental psychology; children's fears and worries about going home should be discussed with them; the staff should cooperate with parents. Finally, "the parents should be encouraged... to visit their children as often as possible. They should always be given the choice of remaining with their children when they are acutely ill". Maclennan's paper showed once again that there were many people in the 1940s who saw the shortcomings of the existing regulations and advocated radical changes.

The early writings of the Pickerills, Spence, Maclennan, and others were important and influential in the sense that they inspired others and showed that other arrangements of child care in hospital were possible. But massive practical changes in hospital conditions were very slow to come (cf. Monro Davies, 1949). Experiments with living in, such as practiced by Spence, were still the exception. Meanwhile, the editors of *The Lancet* were already convinced that visiting times should be more flexible and argued so repeatedly in no mean words, e.g. "no savage needs to be told that separation from the mother damages young children" (Editorial, 1953a), and "advantages to the child in maintaining real contact with its parents outweigh any of the objections" (Editorial, 1953b). They deplored the fact that so many hospitals had ignored repeated advices by the Ministry of Health to allow daily visiting. In 1952, of 1300 hospitals only 300 allowed daily visiting (Editorial, 1953b; 1953d). But considerable numbers of the readers of *The Lancet* and the *British Medical Journal* were

still unimpressed by their arguments. For example, a certain prof. Moir, consulting surgeon to the *United Leeds Hospitals*, maintained there was “a lot of sloppy sentiment talked about this. If children are left alone for a day or two they forget their parents. The hours in hospital after the visit of parents are chaotic. The children all cry and shriek and will not go to sleep” (Editorial, 1953c; cf. Neville, 1953; Penfold, 1953). In fact, it would take decades before Britain had essentially reached the present system of open visiting of hospitalized children (see Van der Horst & Van der Veer, in press).

### **Issues of separation: Clinical studies**

The first systematic indications that separations from the parents might be potentially harmful came from the observation and investigation of children who visited a Child Guidance Clinic. Psychoanalytically oriented psychiatrists and psychologists working at such clinics often found that problem children were basically insecure and had no fundamental trust in the love of their parents. The so-called ‘English school’ of Tavistock psychiatrists emphasized the importance of a primitive need for security. Adherents to this view thought “that a child begins life completely helpless and dependent, and that it responds with every expression of terror to... loss of mother” and therefore has “a tendency to seek love and security as such” (Dicks, 1939, pp. 20/90). As early as 1935, Suttie wondered whether the “attachment-to-mother is merely the sum of the infantile bodily needs and satisfactions which refer to her [i.e. secondary drive], or whether the *need for a mother is primarily presented to the child mind as a need for company and as a discomfort in isolation*”. He emphasized that “love of mother is primal in so far as it is the *first formed and directed* emotional relationship” (Suttie, 1935/1988, pp. 16/31; original italics). According to Edelston (1943, p. 74), “even the strict psycho-analytical school” had at that time “been compelled... to recognize the importance of this earliest of human needs”. Obviously, this fact did not escape Bowlby’s attention and interest.

Bowlby (1944, 1946) himself actually was one of many who contributed to the weight of clinical evidence with a paper on juvenile thieves, who had been seen and treated between 1936 and 1939 at the London Child Guidance Clinic. In this study, Bowlby compared the case histories of 44 thieves with a control group of 44 non-thieves. Goal of the paper was “a systematic investigation of possible adverse effects in the young child’s environment... and in particular that part of it comprised by the parents” (Bowlby, 1944, p. 125). Bowlby distinguished three different factors that might lead to maladjusted behavior: 1) genetic factors, 2) early home environment, and 3) contemporary environment. To no surprise, Bowlby particularly emphasized the adverse effects in the early environment when a child is “separated from his mother or mother-substitute for long periods or permanently during his first five years of life” (ibid., p. 109). He concluded “that the socially satisfactory behaviour of most adults is dependent on their having been brought up in circumstances... which have permitted... satisfactory development of... object-relationships” (ibid., p. 125).



Figure 2. Photograph of a child therapy session with Bowlby, titled “Just child’s play”. “The doctor and Joan discuss the drawing and after a time, Joan tells him all about it. From the drawing and the things she said, he realized that her trouble was loneliness. The father was in the army, a railway journey from home, and the mother missed her husband too acutely to pay enough attention to the child.” Picture courtesy of the Wellcome Library, London (AMWL: PP/BOW/L.4, nr. 11).

In another study, Edelston (1943) suspected that children’s feelings of insecurity and various forms of misbehavior might be partially caused by earlier hospital stays. In 1938 and 1939, Edelston investigated 42 clinical cases of problem children who had experienced repeated admissions to hospital without the parents being allowed to visit. Edelston found that many of the children afterwards suffered from feelings of being abandoned or unwanted and that they were very anxious, clung to their mother, and, in general, showed disturbed behavior. According to Edelston, the “separation from home (i.e., from the mother) form[ed] the essentially traumatic element in the experience” (p. 14) and “the younger and more helpless the child the greater the separation anxiety” (p. 83). In all, “the determining factor seem[ed] to be the degree of rejection or insecurity *felt by the child*” (p. 85, original italics). Unfortunately, these findings seem to have escaped the attention of experts owing to the outbreak of the war in Britain (cf. Edelston, 1940). Other such studies on hospitalized children did not. To these studies on ‘hospitalization’ we will turn our attention.

**Issues of separation: Studies on the 'hospitalization effect'**

Shortly before and during the Second World War the first studies started to appear concerning the ill-effects of hospitalization of children (e.g., Beverly, 1936; Lowrey, 1940; Dennis, 1941; Bakwin, 1942; Edelston, 1943; Goldfarb, 1943b; Spitz, 1945). One of the first to address the issue of hospitalization was psychiatrist Lawson Lowrey (1940). He observed "the development and integration of personality" (ibid., p. 576) of 28 children who were placed in foster homes and of whom nine were described in detail. The children showed very high percentages of "hostile aggressiveness, temper tantrums, enuresis [bedwetting], speech defects, attention demanding behavior, shyness and sensitiveness, difficulties about food, stubbornness and negativism, selfishness, finger sucking and excessive crying" (p. 579). According to Lowrey, "[t]he conclusion seems inescapable that infants reared in institutions undergo an isolation type of experience" and that children "should not be reared in institutions" (p. 585).

More influential though was the work of pediatrician Harry Bakwin (1942), who described the care of small children in New York's *Bellevue Hospital*. The high mortality rate in this hospital was first attributed to malnutrition and then to cross-infection. In an attempt to lessen the danger of cross-infections, "the open ward... ha[d] been replaced by small, cubicled rooms in which masked, hooded and scrubbed nurses and physicians move[d] about cautiously so as to not stir up bacteria" (Bakwin, 1942, p. 31). Visiting parents were strictly excluded and the infants received a minimum of handling by the staff. Surprisingly enough to people involved at the time, these measures had no effect whatsoever on mortality. Rather by accident, Bakwin noted that infants slowly withered away and, despite their high caloric diets, would only gain in weight after they had returned home. He presumed that the "psychologic neglect" (p. 32) they endured, the total lack of mothering, and the sterile environment in the wards were damaging the children. Following a change in hospital policy, nurses were encouraged to mother and cuddle the children, to pick them up and play with them, and parents were invited to visit. The results of this change in policy were dramatic: despite the increased possibility of infection, the mortality rate for infants under one year of age fell sharply from 30-35 per cent to less than 10 per cent. Bakwin's paper was noticed by experts all over the world, including Britain. The impact of Bakwin's paper in Britain was amplified by the editors of the *British Medical Journal*, who discussed and supported Bakwin's ideas, and stated that "in infancy the loneliness involved in separation may be not only undesirable but lethal" (Editorial, 1942a, p. 345). The editors also noted that Bakwin's descriptions of children's symptoms "correspond disturbingly with those of some observers in our wartime nurseries" and suggested that "the biological unity of mother and little child cannot be disregarded with impunity". Different correspondents (Hutton, 1942; Macdonald, 1942; Salaman, 1942) sided with the editors and enthusiastically welcomed Bakwin's contribution. Bowlby's psychoanalytic colleague Donald Winnicott (1942, p. 465) considered the review of Bakwin's paper "the most important you have published over a long period" and warned that "we cannot take mothers from infants without seriously increasing the psychological burdens which the next generation will have to bear". As we have already seen, at the time Bakwin made his observations in the USA, Edelston

(1943) did a similar (though retrospective) study on separation anxiety in young children in Britain (see above).

In nine publications on the care of (Jewish) children in foster homes in New York, psychologist William Goldfarb (1943a, 1943b, 1943c, 1943d, 1944, 1945a, 1945b, 1947, 1949) compared the prevalence of “aggressive behavior disorders” (Goldfarb, 1943a, p. 250) in foster children with experience in institutions in the first three years of life to the behavior of foster children without such experiences. Goldfarb hypothesized that in the ‘institution group’ these behavior disorders were more likely to be found than in the ‘foster home group’. The conditions in the institutions were similar to those described by Bakwin:

The children... had... been cared for in an institution with... an outstanding programme of medical prevention. Babies... were each kept in their own little cubicles to prevent the spread of epidemic infection. Their only contacts with adults occurred during those few hurried moments when they were dressed, changed, or fed by the nurses. These nurses had neither training nor time and resources to offer love and attention to a large group of babies... [A]lmost complete social isolation during th[e] first year of life, ... and [an] only slight enrichment of experiences that followed in the next two years. (Goldfarb, 1947, p. 456)

Goldfarb (1943b, p. 127) noted that the institutionalized children had “an exceedingly impoverished, meagre, undifferentiated personality with related deficiency in inhibition and control” and a “passivity or apathy of personality”. In the explanation of his findings, Goldfarb laid special emphasis on three main features in the institutions: 1) absence of stimulation, 2) absence of psychological interaction and reciprocal relation with adults, and 3) absence of normal identifications. The sterile climate in which the children lived, apparently had major consequences for later social interaction and Goldfarb concluded that a healthy interaction between children and their caregivers was of the utmost importance.

Psychoanalytically oriented psychiatrist René Spitz had worked on the issue of sterile children’s wards with Katherine Wolf in Austria, before he fled the European continent to New York with hope of joining Bakwin and Goldfarb in their work on deprivation (Blum, 2002). Spitz’s main interest was in the relationship between mother and child and he was the first to coin the terms of ‘hospitalism’ and ‘anaclitic depression’ in children (Spitz, 1945, 1946, 1951; Spitz & Wolf, 1946, 1949). “The term hospitalism designates a vitiated condition of the body to long confinement in a hospital, or the morbid condition of the atmosphere of a hospital” (Spitz, 1945, p. 53). In Spitz’s psychoanalytic jargon, an anaclitic depression was a “psychiatric syndrome of a depressive nature... related to a loss of the love object, combined with a total inhibition of attempts at restitution through help of the body ego acting on anaclitic lines” (Spitz & Wolf, 1946, p. 339). Spitz studied the effect of continuous institutional care of infants under one year of age by comparing children in a nursery to children in a foundling home – as did Goldfarb before him. From his observations, Spitz concluded that 1) affective interchange is necessary for a healthy physical and behavioral development of infants; 2) this interaction is provided by reciprocity between mother (or mother substitute) and child; and 3) deprivation of this reciprocity is dangerous for the

development of the personality of the child. Of the studies on hospitalization discussed here, Spitz's work on the effects of hospitalization was the most influential if we go by the number of citations, but it also came under heavy criticism.<sup>1</sup>

Spitz was attacked by psychologist Samuel Pinneau (1955a, 1955b; cf. Karen, 1994; Spitz, 1955), who essentially pointed his arrows at four different aspects of Spitz's studies: 1) Spitz's refusal to identify the dates and locations of his observations (cf. Anonymous, 1952); 2) the inconsistency of the alleged number of children involved in the observations, which suggested a cross-sectional approach instead of the longitudinal study that Spitz presented; 3) Spitz's failure to account for the different cultural and racial background and socioeconomic status of the groups that were compared; and 4) the doubtful validity of the developmental scale, which jeopardized the interpretation of the test data. Despite this severe criticism, Spitz's work would be highly influential for several decades.

### **Bowlby and the WHO report on deprivation**

After the Second World War, Bowlby became involved in the reorganization of the Tavistock Clinic known as *Operation Phoenix* (Van Dijken, 1998). In January 1946 he was appointed head of the new Children's Department; in July 1947 he was elected deputy director to John Sutherland. His first priority was to recruit staff and organize clinical service, which started in the autumn of 1946. From 1948 Bowlby also planned a research unit, to which James Robertson was the first to be appointed as a research assistant. In line with senior analyst John Rickman's ideas, the Tavistock doctrine at that time was that "there should be no therapy without research and no research without therapy" – a creed that Bowlby fully supported in thought, word and deed (Van Dijken, 1998).

In 1949, Ronald Hargreaves, Bowlby's former colleague at the Tavistock Clinic and during the Second World War, by now Chief of the Mental Health Section at the WHO in Geneva, asked him to do a report on mental health problems of homeless children (Van der Horst et al., 2007). Bowlby read extensively into the early work on deprivation while working on this report in an effort to "draw the strands together into one coherent argument" (Rutter, 1972a, p. 121). To gather information for his report, in the first half of 1950, Bowlby visited various European countries and the United States and consulted experts in the field of psychiatric care. During a five week stay in the USA in March and April, he visited both Spitz and Goldfarb. In a letter to his wife Ursula he discussed his schedule:

As a result of my days [sic] activities I've made a huge number of appointments. On the whole I've been lucky in finding people available. Tomorrow, I'm busy morning [and] afternoon [and] in the evening have dinner with the Goldfarbs... Monday I'm busy all day [and] dine with Spitz... This means I get off to a flying start [and] don't

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<sup>1</sup> A search in the Web of Science® shows that Spitz's (1945) paper alone has more citations [858] than the other studies discussed here combined (Lowrey, 1940 [54 citations]; Bakwin, 1942 [95]; Edelston, 1943 [37]; Goldfarb, 1943a [93], 1943b [135], 1943c [28], 1943d [1], 1944 [25], 1945a [156], 1945b [113], 1947 [55], 1949 [30]).

waste time at the beginning which I'm pleased about. (Bowlby in a letter to Ursula, March 10, 1950; AMWL: PP/BOW/B.1/12)<sup>2</sup>

After meeting with them both, Bowlby was particularly impressed by the work of Goldfarb and wrote about his discussions with him:

All goes exceedingly well here – to the point where time for letter writing is hard to come by. Saturday was busy [and] fruitful, especially coffee with Goldfarb [and] his wife. I [wi]ll be writing a full description of this to Noel [Hunnybun]<sup>3</sup> so will only tell you now that he is a most attractive young man of [thirty-five], American born [and] not the least Jewish<sup>4</sup>, he has been doing no research for [four] years but has now nearly completed his medical studies. He dines with me tomorrow night [and] the possibility of him coming to the Tavi[stock Clinic] for a year will be discussed. That would be a great acquisition. (Bowlby in a letter to Ursula, March 13, 1950; AMWL: PP/BOW/B.1/12)

After his meetings with Goldfarb, Bowlby indeed reported to Noel Hunnybun about Goldfarb's work:

Goldfarb is the real bright spot here, though for the past four years he has been in 'retirement' studying medicine. He is a delightful young man of 35, modest, sensitive and intelligent... His work is not widely known, but is highly regarded in discriminating quarters. Personally he seems to be liked and respected. His studies seem to have been carried out between 1940-1946 off his own bat, and in his spare time... He has done nothing for the past four years, though he has a great deal of interesting material... still unpublished. I raised with him the possibility of his coming to the Tavi[stock Clinic] for 12 months... to write his stuff up into a coherent monograph. He was greatly attracted by the idea and is thinking it over seriously. October 1951 is the earliest he could make as he has to complete a medical internship. He wants to become a psychiatrist and is already training in psycho-analysis. Though it is impossible to judge his ultimate ceiling, there is no doubt about his quality. (Bowlby in a letter to Noel Hunnybun, March 19, 1950; AMWL: PP/BOW/B.1/12)

In a staff meeting on May 11, after Bowlby had returned to England, he would add that Goldfarb would "get a senior job there [at Columbia University]... because I think there is little doubt that he is pretty well the best chap they have got" (Travelogue given by Bowlby,

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<sup>2</sup> AMWL stands for Archives and Manuscripts, Wellcome Library for the History and Understanding of Medicine, 183 Euston Road, London NW1 2BE. The letters PP/BOW stand for Personal Papers Bowlby.

<sup>3</sup> Noel Hunnybun was a senior social worker at the Tavistock Clinic

<sup>4</sup> Perhaps Bowlby expected someone with the name Goldfarb to be Jewish.

May 11, 1950; AMWL: PP/BOW/F.1/1). Goldfarb's move to the Tavistock Clinic was never realized.

Bowlby's travels and research ultimately led to his monograph *Maternal care and mental health* (Bowlby, 1951, 1952), in which he discussed the state of the art and most recent advances in studies on deprivation. He discussed the work of Bakwin, Goldfarb, and Spitz under the heading of 'direct' studies on evidence of effects of deprivation in which observations are made in institutions and in foster homes. Bowlby's own early study of forty-four thieves and the work of Lowrey and Edelston were categorized as 'retrospective' and 'follow-up' studies. These "[r]elatively few studies taken by themselves are more than suggestive, [b]ut when all the evidence is fitted together it is seen to be remarkably consistent", Bowlby (1952, p. 46) argued. And he reached the conclusion that:

the evidence is now such that it leaves no room for doubt regarding the general propositions – that the prolonged deprivation of the young child of maternal care may have grave and far-reaching effects on his character and so on the whole of his future life. (ibid., p. 46)

Bowlby's report was immediately and very favorably discussed by the editors of *The Lancet* (Editorial, 1951a). The editors considered his report "extremely impressive" and summarized Bowlby's discussion of the findings by Bakwin, Goldfarb, Spitz, and others. They concurred with Bowlby that the evidence in favor of the damaging effects of mother-child separations was remarkably consistent and impressive. Quoting Bowlby's words that "knowledge of truth is always partial, and that to await certainty is to await eternity", they concluded that "in this case, to await certainty may well be to await a spreading of our present social sickness until it is beyond all cure" (ibid., p. 1166). The editors of the *British Medical Journal* followed suit and praised the "remarkably interesting and valuable report". They, too, fully accepted Bowlby's findings and conclusions, and remarked that

happily in children's wards and children's hospitals there is now a tendency to allow daily visiting. Admittedly this presents great difficulties to the nurses, but even the small amount of carefully controlled work which Bowlby is able to report on this limited aspect of the subject shows how worthwhile the extra trouble is. (Editorial, 1951b, p. 1374)

The reception of Bowlby's monograph in the medical journals at the time was very positive, but later his views were critiqued. Though Michael Rutter (1972a, 1972b, 1979) stressed the importance of Bowlby work in the early 1950s as it "stimulated a wealth of research and led to a reconsideration of the care provided for children being reared in institutions" (Rutter, 1972a, p. 120) and stated that "the concept of 'maternal deprivation' has undoubtedly been useful in focusing attention on the sometimes grave consequences of deficient or disturbed care in early life" (ibid., p. 128), he also argued that "the term... has served its purpose and should now be abandoned" (ibid., 128). Rutter's main argument is that the experiences included under the term 'maternal deprivation' are too heterogeneous

and that the effects vary too much from child to child. Here we only emphasize the importance of Bowlby's work for the WHO for the development of his ideas on the consequences of early deprivation of maternal care. Much later, Bowlby himself would say that he had been greatly influenced by the work on maternal deprivation, because it resulted in him focusing more on separation and institutionalization (Senn, 1977).

### **A picture speaks a thousand words: films to support new views**

Both in the UK and in the USA the field of psychology was stirred in a similar way. In the USA, Spitz shook the ground with his silent, black-and-white film *Grief: A Peril in Infancy* (Spitz, 1947). Spitz filmed, amongst other children, a baby named Jane who within weeks after placement in a foundling home developed from a happy and approachable child into a distant and withdrawn one. Spitz himself described the cure for this child on one of the film's title cards: "Give mother back to baby". Jane is shown again, after her mother has returned after a three month separation, playing, clapping, and laughing. Reactions to Spitz's film were quite similar to the reactions Robertson would later receive for his film: those of shock and disbelief. Karen (1994, p. 25) described how after the film was shown to physicians and psychoanalysts at the New York Academy of Medicine, a "prominent New York analyst approached Spitz with tears in his eyes. 'How could you do this to us?' he said". Apparently, people were shocked by the sight of babies pining away from grief. It was something that they had not seen or been willing to see before.

In the UK, James Robertson, social worker with Bowlby at the Tavistock Clinic, made a similar film called *A Two-Year-Old Goes to Hospital* (Robertson, 1952) – black-and-white and silent, but with spoken commentary. In this film, an unusually controlled toddler named Laura leaves home for a period of nine days to be admitted at *Central Middlesex Hospital* for the operation of an umbilical hernia. She changes from a "ravishing little girl" (Hinde, in Van der Horst et al., 2007) to a silent and unresponsive one. Robertson's film was first shown at the Section of Paediatrics of the Royal Society of Medicine on November 28, 1952, before a large audience of doctors and nurses. The accounts of that meeting differ somewhat in their description of the way the film was received by the audience. The proceedings of the meeting (Bowlby & Robertson, 1952) just related that Bowlby and Robertson introduced the film, provided a synopsis of the film, and then added that the president of the pediatric section, Winnicott, spoke of a "highly successful first effort" that dealt with "a real problem". Winnicott continued that he himself had seen "irreversible change" as a result of "separation of small children from their mothers" and argued that "every time a child is to be taken into hospital there ought to be a careful weighing up of the value on the physical side against the danger on the psychiatric side". Both the editors of the *British Medical Journal* and *The Lancet* favorably discussed the meeting in their issues of December 6. The editors of the *British Medical Journal* agreed that "the 2-year-old girl depicted was unhappy and that possibly her unhappiness might have been prevented" (Editorial, 1952a, p. 1249). They mentioned that this was in line with the findings in Bowlby's (1951) report and that it would be a great risk to continue to neglect these matters. The editors believed that more "friendliness and consideration" would do the children much good, but added that "the part the mother should play, and how often parents should visit, may be

more controversial subjects” (Editorial, 1952a, p. 1250). The editors of *The Lancet* were equally positive but more detailed in their rendering of the reactions to the film by the audience (Editorial, 1952b). They stated that at first the audience frankly refused to admit the child was distressed at all and those who accepted that Laura was distressed were reluctant to believe it might cause long-term or even permanent emotional disturbances. These discussants argued one would need to film emotional upsets at home and hospital stays without an operation as a control. Robertson and Bowlby are said to have agreed that more research was needed but remained convinced that the child was upset, that such operations at this age should be avoided if possible, and that the isolation and lack of physical comfort in hospitals were positively bad. Interestingly, these contemporary accounts were rather more neutral than Robertson’s memory of the meeting. Robertson remembered that “the film encountered much resistance” and that various speakers said hotly that he had filmed “an atypical child of atypical parents in an atypical ward” (Robertson & Robertson, 1989, p. 44)<sup>5</sup>. The speakers supposedly also said that Robertson “had slandered paediatrics” and that the film should be withdrawn. We have no way to decide which account of the meeting is most correct, but according to Dr. Mary Lindsay (personal communication, April 7, 2008) “the film had a very hostile reception” and “the editor toned down the anger in the proceedings of the meeting”. The fact of the matter is that Bowlby and Robertson eventually decided to temporarily withhold the film from general release due to the massive resistance among the medical staff (Robertson & Robertson, 1989, p. 45).

Between parentheses, it should be said that the value of films as an argument in scientific debates is limited. Strictly speaking, they can only show that a certain phenomenon may take place, not that it generally takes place, and under which specific circumstances. Thus, Robertson’s opponents could always argue that Laura, her parents, or the hospital were somehow exceptional and that other children in (other) hospitals were perfectly fine. At any rate, they could argue with some justification that it remained far from proven that her distress was caused by the separation *per se* (cf. Bowlby, 1958a, 1958b; Edelston, 1955, 1958; Howells, 1958; Howells & Layng, 1955; Kräupl Taylor, 1958; Librach, 1956). Over the years, the opponents of flexible visiting in hospitals would exploit these possibilities to the utmost, with sometimes vehement debates as a result (cf. Herzog, 1958a, 1958b; Kidd, 1958; Robertson, 1958a, 1958b; Stephen & Whatley, 1958a, 1958b). Robertson and Bowlby’s adversaries would go to great length to disprove them: for example child psychiatrist Fred Stone was offered research money to “prove this Bowlby stuff to be nonsense” (Hinde, 1982a, p. 60). Such reservations about the methodological merits of films notwithstanding, Robertson campaigned the cause of better care of children in hospital throughout the 1950s and 1960s, showing his film to many audiences, and eventually managed to persuade many people that something needed be done. Mary Lindsay

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<sup>5</sup> This statement strangely contradicted what Robertson wrote in a letter to *The Lancet* in 1958. There he wrote that “when the film was shown to the paediatric section of the Royal Society of Medicine in November, 1952, it seemed generally agreed that (...) the behavior shown was common” (Robertson, 1958a).

remembers how Dr. MacCarthy, who would become a key figure in the debate, became converted:

Dr. MacCarthy went to this meeting [the first showing of *A two-year-old*] with his Ward Sister. Coming back in the car afterwards he went on at some length on how wrong Robertson was. However, Sister Morris said that Mr. Robertson was quite right and that these babies and young children did need their mothers; and that she used to let them stay in the ward with their children when he was not there. Dr. MacCarthy was startled by this idea, but he had great respect for Sister Morris. The next day he found that he could not walk down his children's ward without seeing Laura and her brothers and sisters. From then on, the mothers of children under five in his wards at Amersham and Aylesbury were routinely asked if they wanted to stay in the hospital with their children; most of them did, and visiting became unrestricted. (Mary Lindsay, personal communication, April 7, 2008)

In 1958 Robertson first showed his second film *Going to hospital with mother*, which followed the twenty-months-old Sally who was admitted to *Amersham General Hospital* for an umbilical hernia operation together with her mother. The film showed how mothers took care of their sick children and how Sally managed the hospital stay and operation without much anxiety thanks to the presence of her mother. As would be expected, the editors of *The Lancet* (Editorial, 1958) reviewed Robertson's favorably, noting that "even the most sceptical audience could hardly fail to be impressed by this second film". In sum, the evidence supporting the idea that the mother-child relationship was crucial to a healthy development of children was piling up.

### **Conclusion: Unexplained observations**

Taken together, the observations made in Child Guidance Clinics, during the evacuations, and in residential nurseries, the discussions surrounding visits to children in hospital, the hospitalization studies, and the films supporting these views yielded a consistent picture that was highly relevant for the proper way to deal with young children. The findings pointed out that separation from the parent is traumatic and potentially harmful, that children need strong emotional ties with a grown-up, and that they should be given a chance to form a new bond with a substitute parent in case they (temporarily) lose their own parents. In sum, children need to be loved, and when they lose this love, or believe they have lost it (e.g., in the case of separations they cannot comprehend), they feel very unhappy and may develop serious mental and physical problems. A growing group of psychiatrists and physicians was aware of these findings.

In all, slowly but surely, people – in hospitals, foundling homes, nurseries – were beginning to see the effects of separation and deprivation on young children. The evidence gathered led people to believe that the physical and emotional separation from a familiar environment was detrimental to the child's well-being. These new views were supported by films such as those by Spitz and Robertson (for a full overview of films on children's hospitalization and maternal deprivation, see Mason, 1967). Their cinematic contributions

were a way of conveying the message to a more general public. Unfortunately, because of the retrospective nature of the reported findings, there could only be speculation about the underlying mechanisms of the distress shown though. It was up to others to lead the way to a theoretical and experimental validation of the consequences of maternal deprivation – and this is where Bowlby entered the stage. The findings Bowlby had gathered for his WHO report were suggestive but not conclusive. What was missing were rigorous experimental investigations and, above all, a comprehensive theoretical framework from which to explain the findings (cf. Smuts, 1977). Firm experimental proof of his ideas Bowlby would eventually find in Harlow's experiments (see Chapter 4). For a theoretical framework for the explanation of the nature of the mother-child relationship he turned to ethology, the new science of animal behavior, eventually resulting in his *magnum opus*: the attachment trilogy (Bowlby, 1969/1982, 1973, 1980a).

Although Bowlby's acquaintance with ethology was rather coincidental, he had had an interest in nature from a very early age. As a child Bowlby had learned to value the life in the countryside during family holidays and he always remained a passionate naturalist. During long vacations in the Scottish highlands, it was Bowlby's mother May who "tried to pass on her love for nature to her children" (Van Dijken, 1998, p. 24) and who learned them "to identify flowers, birds and butterflies, to fish, ride and shoot" (Holmes, 1993, p. 17). As a naval cadet at Dartmouth, Bowlby was an enthusiastic bird-watcher and photographer (Van Dijken, 1998) – like many of the ethologists were (Roëll, 2000; Burkhardt, 2005). In one of his early publications, Bowlby (Durbin & Bowlby, 1939) already extensively cited studies on the social life of monkeys and apes. Another remarkable example of his interest in animal behavior and ethology comes from a travelogue in which Bowlby reported on his 1950 trip to the USA for the WHO:

I came across one [of Freud's] book[s] on the development of mind by Romanes which is all about animals and ethology which was carefully marked... by Freud, which rather pleased me, but I unfortunately have not yet confirmed that all the markings in this book were Freud's. That I am trying to do. But I was rather pleased. (Travelogue given by Bowlby, May 11, 1950; AMWL: PP/BOW/F.1/1)

Thus, Bowlby's choice for ethology as a framework was preceded by a life-long interest in nature: "His love for the out-doors and his keen eye for observation made him naturally responsive to the basic tenets of classical ethological theory and methodology" (Suomi, 1995, p. 185; cf. Van Dijken, 1998). Bowlby devoted the last years of his life to a substantial biography of Charles Darwin, which was published just three months before Bowlby's death. According to Ursula "the publication of the Darwin book... made the end of his life full of interest and enjoyment" (Ursula in a letter to Joan Stevenson-Hinde, September 24, 1990; private archive Stevenson-Hinde). In it, Bowlby put forward the thesis that "Darwin's long lasting troubles... can be understood as responses to stressful events... [and] as a result of a childhood shadowed by an invalid and dying mother" (Bowlby, 1990, pp. 1-2). By writing this biography, Bowlby completed the circle: he started with a passion for nature, turned to

clinical practice and mental health studies and finished with a clinically inspired work on “the most influential biologist to have lived” (ibid., p. 1).

In this chapter we have described how Bowlby found inspiration to follow up on the findings from early studies on deprivation by Bakwin, Goldfarb, and Spitz. To do so, Bowlby cast his net wide to get answers, for example from Dutch animal psychologist Adriaan Kortlandt<sup>6</sup>:

We are very happy to send you our reprints. Although hitherto they have not referred to ethological work we are shaping our studies increasingly in that direction, and we shall be very glad therefore to have reprints of your own work, which we already know from many references. (Bowlby in a letter to Kortlandt, dated April 28, 1954; private archive Kortlandt)

So, in the early 1950s, Bowlby and his colleagues were shaping their studies increasingly in the direction of ethology. For Bowlby it was clear that “the time [wa]s already ripe for a unification of psycho-analytic concepts with those of ethology” (Bowlby, 1953, p. 32). In the next chapter we shall describe how Bowlby was influenced by ethology, the new approach to the study of animal behavior that he would apply to human behavior and which he used as a theoretical basis for what later would be called ‘attachment theory’.

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<sup>6</sup> The correspondence between Kortlandt and Bowlby was very brief and consists of only nine letters. They stopped writing in 1957 and this may have had something to do with the fact that Hinde (1957) was very critical of Kortlandt’s (1955) publication on “aspects and prospect of the concept of instinct” (cf. A. Kortlandt, personal communication, April 8, 2006).

## CHAPTER 3.

# JOHN BOWLBY AND ETHOLOGY: AN ANNOTATED INTERVIEW WITH ROBERT HINDE

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**Abstract**

From the 1950s, John Bowlby, one of the founders of attachment theory, was in personal and scientific contact with leading European scientists in the field of ethology (e.g., Niko Tinbergen, Konrad Lorenz, and especially Robert Hinde). In constructing his new theory on the nature of the bond between children and their caregivers, Bowlby profited highly from their new approach to (animal) behavior. Hinde and Tinbergen in their turn were influenced and inspired by Bowlby's new thinking. On the basis of extensive interviews with Bowlby's colleague and lifelong friend Robert Hinde and on the basis of archival materials, both the relationship between John Bowlby and Robert Hinde and the cross-fertilization of ethology and attachment theory are described.

**Keywords:** attachment theory, ethology, animal behavior, history

## **Introduction**

The central figure of this special issue, John Bowlby (1907-1990), did not create his attachment theory overnight. Beginning from the late 1930s, he tried to combine different strands of thinking into one coherent theory that would explain the function and nature of the bond between children and their caregivers. Researchers have become increasingly interested in the different roots of attachment theory and in the way Bowlby merged them, but until now relatively few publications have specifically addressed the genesis of attachment theory (Bretherton, 1991, 1992; Newcombe & Lerner, 1981; Van Dijken, 1998; Van Dijken et al., 1998). In his contribution, attention will be paid to one particularly important influence on attachment theory, namely that of ethology, the new approach to animal behavior that emerged in the 1930s. Bowlby repeatedly stated that the ethological approach was of fundamental importance to his thinking and that it was Robert Hinde who introduced him to the finer details of ethology (e.g., Dinnage, 1979; Smuts, 1977). Likewise, Hinde himself has declared that working with Bowlby was immensely fruitful for his own thinking. Such claims raise the interesting question whether we can think of attachment theory and ethology in terms of cross-fertilization and, more particularly, to what extent Bowlby and Hinde influenced each other's thinking. In order to elucidate these and related issues the first author conducted two interviews with Robert Hinde at St. John's College in Cambridge UK, August 2005. What follows is an account of these interviews interspersed with explanatory passages and introductory remarks.

## **Before ethology**

Bowlby's interest in ethology was based on the hope that it seemed to provide a way of thinking about the nature and function of an affectional bond between a child and its caregiver (Bretherton, 1991, 1992). His interest in the caregiver-child relationship and its importance for the child's well-being had its roots in professional experiences and, perhaps, ultimately in his personal life. Van Dijken (1998) has argued that Bowlby's strong interest in the consequences of separation in childhood may be partly ascribed to experiences in his own childhood: the departure of his nanny when he was 4 years old, the absence of his father as a military surgeon during large parts of his childhood, the separation due to attendance at a boarding school at 11 years old and, finally, the unexpected death of his godfather during a game of football. Bowlby himself stated that his interest for the subject was aroused when he worked as a volunteer at Priory Gate, a school for maladjusted children:

I spent 12 months in one of the progressive and free schools, which was very valuable experience, because I saw a number of disturbed children at first hand, I lived with them, indeed I had to look after them, and I met there the first "affectionless character" of my career. (Tanner & Inhelder, 1971, p. 26)

Bowlby speculated that such affectionless characters were the result of separations from caregivers and subsequently tried to corroborate this view while working with juvenile delinquents at the London Child Guidance Clinic. He found that early separation and the

absence of an emotional relationship with a caregiver (usually mother) in the first years of life was indeed correlated with delinquency and affectionless behavior later on. This study was published as *Forty-four juvenile thieves* (Bowlby, 1944, 1946). According to Hinde this study had great influence on Bowlby's thinking:

Q: Bowlby was from an upper middle-class Victorian family, he was raised by a nanny, and his parents were not always physically present. Obviously, such circumstances cannot explain why Bowlby arrived at the idea of attachment between caregiver and child...

A: Why not? Though it would be wrong to assess his Victorian upbringing from a 21st century perspective. The real key is the forty-four thieves paper. He was studying adolescence and behavior disorders and he noticed that many of them had disrupted childhoods and that put him on the trail. Where his own childhood came in, I really can't say.

The study of the forty-four thieves ultimately led to Bowlby's assignment with the World Health Organization (WHO). Ronald Hargreaves, whom Bowlby had met during the war, had become Chief of the Mental Health Section at the WHO in Geneva. Hargreaves knew about Bowlby's work and in 1949 asked him to do a report on mental health problems of homeless children. Bowlby accepted the offer and he worked on the monograph *Maternal care and mental health* (Bowlby, 1951) for 6 months in 1950 (Smuts, 1977). The outcome of this research would greatly and decisively influence his further career and his research activities (Holmes, 1993). In his monograph Bowlby deviated from what was considered the orthodox view in psychoanalysis (Bowlby, 1951; cf. Van Dijken, 1998; Van Dijken et al., 1998). Trained in Kleinian psychoanalysis, Bowlby never accepted her explanation of the emotional relationship between mother and child. According to this theory, called the cupboard love theory or theory of secondary drive, this relationship ultimately depended on the fact that the mother feeds the child. Neither did he agree with what he saw as Klein's disregard for objective adverse circumstances in the child's environment. In a paper he read to the *British Psychoanalytical Society*, Bowlby stated that it was genuine, objective early experiences that influenced the child's development. Many years later, Bowlby commented that:

[M]ost of what goes on in the internal world is a more or less accurate reflection of what an individual has experienced in the external world... If a child sees his mother as a very loving person, the chances are that his mother is a loving person. If he sees her as a rejecting person, she is a very rejecting person. (Bowlby et al., 1986, p. 43)

The WHO report exerted tremendous influence, but it also raised a number of questions. Winnicott (1989; cf. Smuts, 1977), for example, criticized *Maternal care and mental health* because it lacked a discussion of how maternal care influences the child and what psychological processes play a role. According to Winnicott (1989, p. 425) "there is a poverty of treatment in [Bowlby's] theoretical chapter... It should be pointed out that there

are very complex internal factors [at work] that cannot be dealt with in a book like this at all". Bowlby himself could not yet answer these questions either: "I didn't know, and I don't think anyone else knew" (Smuts, 1977). It was in this period that his attention was first drawn to the new emerging science of ethology.

### Getting acquainted with ethology

Bowlby was first introduced to ethology in July 1951 by psychologist Norman Hotoph. He probably knew Hotoph through a group of Labour friends at the London School of Economics. Bowlby's closest friend Evan Durbin, his brother Tony and his brother-in-law Henry Phelps Brown were all in the same group (Smuts, 1977; Ursula Longstaff Bowlby, personal communication to Suzan van Dijken, April 29, 1996). Hotoph pointed out to Bowlby that Konrad Lorenz (1935, 1937) had worked on the principle of imprinting as a process underlying the formation of affectional bonds (cf. Bretherton, 1991) in the 1930s. During a summer holiday in Scotland, Julian Huxley, a friend of the Longstaff family and a prominent British ethologist, encouraged Bowlby to go into the matter in more depth after finishing the report on deprivation for the WHO (Ainsworth & Bowlby, 1991; Bowlby, 1969/1982; Bowlby et al., 1986; Hinde, 2005; Karen, 1994; Smuts, 1977). Bowlby indeed turned to Lorenz's work and in an interview in 1979 he ranked Lorenz's *Der Kumpan in der Umwelt des Vogels* [The companion in the bird's world] among the 11 books which had most influenced his thinking (Bowlby, 1979). One might wonder to what extent the concept of imprinting<sup>7</sup> as introduced by ethology and the concept of attachment were linked. Hinde's answer to that question leaves no doubts:

Q: Was Bowlby's concept of attachment new in ethological thinking and research on nonhuman primates?

A: Bowlby came up with the name of the concept, but it was in ethology long before that. Of course not with all Bowlby's connotations, only imprinting was there. Lorenz introduced imprinting in the thirties, and Heinroth described the process before then, but not with all Bowlby's connotations. That was really Bowlby's effort.

Q: How important do you think was Bowlby's introduction to imprinting for the history of attachment theory? Or was he inevitably going to come across ethology as a framework for his theory?

A: I don't know too much about that early stage. But imprinting was important and also Harlow's work was important, because it showed that attachment didn't depend on food, which was the prevailing view in psychoanalysis.

After Bowlby read Lorenz's work on imprinting, Huxley provided him with a proof copy of *King Solomon's ring* (Lorenz, 1952) for which he had written the foreword. Huxley also

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<sup>7</sup> American zoologist Charles Otis Whitman used imprinting for cross-breeding different species of pigeons. Dutch zoo man Frits Portielje had witnessed imprinting in in the South American Bittern [*Botaurus pinnatus*]. However, Lorenz coined the concept, emphasized its theoretical importance, and thus became its "discoverer" (Burkhardt, 2005).

mentioned Tinbergen's (1951) *The study of instinct* to Bowlby. It was partly because of Huxley's enthusiasm that Bowlby spent most of winter 1951-1952 reading his way in ethology. "From that day on," Bowlby remembered, "I was completely sold on ethology" (Smuts, 1977). In the following years, Bowlby and Lorenz met several times for academic discussion. Both attended all four of the WHO study group meetings between 1953 and 1956 in Geneva and London and visited each others laboratories: Bowlby visited Lorenz in Altenberg in 1954 (Zazzo, 1979, p. 56) and Lorenz visited Bowlby at the Tavistock Clinic in October 1957, where they had several discussions with each other (AMWL: PP/BOW/H.183).



Figure 3. WHO study group in Geneva in 1955. From left to right: Jean Piaget, Bärbel Inhelder, Konrad Lorenz, Julian Huxley, and Frank Fremont-Smith. Picture courtesy of the Wellcome Library, London (AMWL: PP/BOW/L.30).

Bowlby's fascination with ethology was obvious, but one might ask why he turned to ethology in the first place? One answer might be that he was seeking confirmation for views that he had held a long time already. From the very beginning of his career Bowlby believed that emotional relationships between parents and children matter a great deal, have long-lasting serious repercussions, and are independent from other factors such as providing food.

Q: Would you agree that Bowlby was stubbornly looking for evidence to buttress the view that early emotional relationships matter a great deal or do you think that

he was open to the idea that they may not have long-lasting effects? In other words, was Bowlby to some extent guided by a fixed idea?

A: You must remember where he came from, mainly his study on forty-four thieves and finding that the thing they had in common was a disrupted childhood. So I think he had the hunch from early on that early childhood relationships were very important in subsequent social development. What he was doing was working out why that should be and how it happened. The time at which I knew him, I would say he needed evidence. But, more importantly, he needed ways of convincing other people. He knew he was right, that it was emotionally important!

### **Hinde's influence on Bowlby**

In an interview in 1979 Bowlby said about the influence of ethology on his thinking:

Ethology I regard as immensely important. What I've been trying to do, really, is to rewrite psychoanalysis in the light of ethological principles. Hinde has had a particularly strong influence on me; I've known him since 1954 – he's vetted my work and criticized it ever since. (Dinnage, 1979, p. 325)

Bowlby and Hinde got to know each other in a rather curious and roundabout way. Bowlby suggested to Ronald Hargreaves, the organizer of the 1953 WHO study group, to invite Konrad Lorenz (Smuts, 1977). At this meeting, during their first conversation, Lorenz told Bowlby about a young ethologist in Cambridge UK by the name of Robert Hinde. Lorenz vividly remembered Hinde's performance at a symposium at his Max Planck Institute in Buldern, where Hinde "dropped a bombshell" (Burkhardt, 2005, p. 376) with a paper on the mobbing reaction of chaffinches [*Fringilla coelebs*] to owls. Lorenz was very enthusiastic about Hinde and Hinde's work.

One year later, in February 1954, Bowlby and Hinde met for the first time during a scientific meeting on ethology and psychiatry organized by the Royal Medico-Psychological Association (RMPA) in London. They actually first met by chance (Bowlby, 1991; Hinde, 2005; cf. Smuts, 1977). Hinde remembered that for "the 1954 [RMPA] conference in London... they had intended to ask Lorenz and Tinbergen and neither of them would come and so it was Bowlby and me who went" (interviews with Hinde, August 2005). Hinde and Bowlby both read a paper and afterwards had lunch together. Like Lorenz before him, Bowlby "was vastly impressed" (Smuts, 1977) by Hinde's expertise. He invited Hinde to join the weekly meetings at the Tavistock Clinic where theorists with wildly diverging views discussed case histories.

A: It's a long time ago now and they started before the Tavistock Clinic moved to its present quarters, in a sort of dingy basement in central London in Beaumont Street. I have a vague memory of a rather dark room with a dirty window under pavement level...

Q: How could you discuss these case histories with psychoanalysts and learning theorists given that in ethological eyes these were either just speculating or being simplistic?

A: Because we were willing to look at the facts and we talked about the facts and how best to explain them. There was Jack Gewirtz, a passionate learning theorist, who was trying to say that it was all learned. I mean, some people in that group would emphasize learning theory, some would bring in Piaget, but focused on the facts as presented. As I say, Bowlby was taking what he wanted...



Figure 4. Robert Hinde (left) and John Bowlby (right) at the Ciba-conference in 1963. Picture courtesy of the Wellcome Library, London (AMWL: PP/BOWL.9).

For Hinde it was a great experience to join the research seminars at the Tavistock Clinic: “It is difficult to describe the excitement of those meetings. Attendance at those meetings was for me a very important scientific experience” (Hinde, 1982b, 1991). Bowlby was very clear about the influence of Hinde on his own thinking. He mentioned Hinde as one of the persons who was crucial in his personal and scientific development in the 1950s and 1960s. Hinde succeeded, so to speak, Evan Durbin, who was influential in the 1930s, and wartime colleague and clinical psychologist at the Tavistock clinic Eric Trist, who was important for Bowlby in the 1940s. This explains why Bowlby dedicated his second volume of *Attachment and loss* (Bowlby, 1973) to those three friends (Smuts, 1977). Asked about his personal

relationship with Bowlby and whether that relationship should be seen as a friendship, Hinde said:

A: I would say that we became friends, yes. I've never quite thought about it in those terms. It is a curious thing in that we both met, as it were, as equals on the same platform, which put me in the position of being a colleague rather than being much younger. Of course the difference in age was always a factor, but one of the pleasures of being involved with Bowlby was that he was eager to learn and I think I can say I was eager to learn. We just talked a lot and I used to read all his manuscripts in the fifties and his books and articles. On the other hand he came from a different tradition from mine in that he was an Englishman of an earlier, more formal generation.

Q: So would you say that Bowlby was open to advice, even from much younger colleagues?

A: Oh yes, I would indeed. I mean, that's what the whole issue between me and him was about and that was what was so wonderful about this seminar with all these different curious people who came to it, including sometimes R. D. Laing, the antipsychiatrist. As I say, Bowlby was listening when we were discussing drafts of his papers or we were discussing case histories that the Robertsons and people brought up and so on. That was really fruitful to all of us, I think.

Q: But would you say that in matters of ethology you were Bowlby's tutor, so to speak?

A: I wasn't a tutor. The discussions that I remember having with Bowlby were very much joint discussions in which we talked things through. And, of course, he had much more experience with children. I had young children of my own, but that was all. It wasn't a tutor-pupil relationship, it wasn't exactly a colleague-relationship, but it was more a colleague-relationship and just talking things through and seeing what emerged... I don't know whether this is exactly what happened, but it might have been that I mentioned that baby ducks must stay near their mother otherwise a peregrine falcon [*Falco peregrinus*] or something might get them and he picked that up and wove it into the understanding of child behavior. I think that's a fair description... John Bowlby and I had long, long discussions, it went over years. As, you know, he came around to the view that what psychiatrists talked about as the irrational fears of childhood are not irrational at all, but had a functional significance.

Q: You said about your relationship with Bowlby that you were friends and colleagues and there was no tutor-pupil relationship...

A: Of course there was a tutor-pupil relationship, but to some extent it was both ways. Now there are certain people who have the ability to talk to people younger than themselves and make it a two-way conversation, as though you were colleagues exploring new territory; Tinbergen was one such, Bowlby another.

Bowlby appreciated Hinde's advice to the extent that he always asked Hinde to comment on his drafts (Smuts, 1977). That this was not always easy for him emerges from his private correspondence. In a letter to his wife Ursula, for example, he wrote:

Frank Beach has read *Separation anxiety* and seems interested. Both he and Robert Hinde, whose comments I read yesterday, make a number of criticisms. I suspect they are not of great substance, but they'll need careful insight and that takes time. Naturally I'm very grateful for them fundamentally, but I confess I hate them initially and feel anxious until I have grasped their full significance. (Bowlby in a letter to Ursula, June 3, 1968; AMWL: PP/BOW/B.1/20)

### **Bowlby's influence on Hinde and the study of animal behavior**

Previously it was suggested that one might think of attachment theory and ethology in terms of cross-fertilization. That would imply that ethology has been influenced by Bowlby's thinking as well. Hinde clearly saw the benefits of this cooperation with Bowlby:

A: I've been extraordinary lucky in lots of ways in my life. If I hadn't been in contact with him, I wouldn't have set up a rhesus monkey [*Macaca mulatta*] colony to study separation. I worked a lot with women colleagues and I do think women see some things that men don't see. Three women in particular, Thelma Rowell and Yvette Spencer-Booth, who both worked on rhesus monkeys here, and Jane Goodall, who worked with chimpanzees in Africa, all convinced me of the importance of individual relationships and individual differences in the animals. It was because of that and because I came to the view that people were more interesting than monkeys and because I had a research job which allowed me to do whatever I wanted to do, I turned from monkeys to studying children in human families. So in that way he had a very big influence on me, it influenced the subsequent course of my research.

Q You began as a biologist, devoted much attention to what many would see as psychological issues and now have focused on the psychological causes of war. How would you describe this development? Was Bowlby instrumental in this gradual shift?

A: That's partly what I've been saying. I started off as a bird watcher and my PhD thesis (Hinde, 1952) was on the Great Tit [*Parus major*]. It was a behavioral observation study in which I just wandered around the Wytham Wood with a notebook and a pencil and a pair of field glasses. David Lack was my supervisor and Niko Tinbergen had just come to Oxford from the Netherlands. Then I was lucky in that W. H. Thorpe was starting an ornithological field station here and various people, including Konrad Lorenz, turned down his offer of the job and eventually he came down the list to me and so I was in on the start of that enterprise. I worked on bird behavior and I happened to do a study on imprinting which was how Bowlby... well, I talked about that. Then I went on working in behavioral endocrinology through the fifties... In 1959 we set up the rhesus colony and through those years I was working with Bowlby and I worked more and more

with monkeys and less and less in behavioral endocrinology through the sixties. And then in the early sixties I got a Royal Research Professorship which allowed me to do whatever I wanted to do, which was super, it's really the most plummy job you can have. It only had one strict rule which was that you mustn't do anything you didn't like doing. You didn't teach anything you didn't want to teach, same for administration. And, as I've told you, then I focused more and more on monkeys and I was lucky that Louis Leakey, the anthropologist-archaeologist, thought that the secret of human evolution lay in studies of the great apes, so I got to supervise Jane Goodall and Dian Fossey and a lot of other people who worked with monkeys. Then I turned to working with human families with my wife [Joan Stevenson-Hinde]. I didn't do especially good work, she did much better, she's a real attachment theorist. Then I had to retire because of age. My brother was killed in the war and I lost a lot of friends and I was involved in it myself. So I did two things in return, one was to focus on war and its causes. During the 1970s and 1980s I was involved in the Campaign for Nuclear Disarmament [CND] and we... well, actually in the Ex-servicemen's Campaign for Nuclear Disarmament, because the media always used to portray CND as a hippie organization with torn jeans and that sort of thing. So we used to go along in suits with medals and grey hair and bowler hats, making CND respectable. And that turned into my heavy involvement with the issue. Quite early on in the war I was on a troopship coming back from Southern Africa, where I was trained as a pilot. We had to watch for submarines and I used to watch with another young man. When we started he was a passionate atheist and I was a mild Christian and when we got to England I was an agnostic and he was a Christian, we sort of converted each other having talked for 12 weeks. When I retired I thought it was time to come to terms with this issue, so I wrote a book on what religion gives people and another one on the sources of ethics and I've got another one in press on ethics.

Q: So after you retired you wanted to come to terms with some issues of the past: the causes of war and religion?

A: Yes, that's true. But they're also issues of the present.

Q: Do you believe that aggression, as a private feeling, has anything to do with the causes of war, as state conflicts? In other words, is psychological research by Freud or ethological research by Lorenz on aggression relevant in this context?

A: I don't think it is. I think that war induces aggression, but aggressiveness does not induce war. The word we use, aggression, covers violent actions by an individual or by a nation, but that doesn't mean that they have anything motivationally in common or that human aggressiveness, the propensity to show violence, is a thing that causes war. Anyway, not a thing that causes major international wars. I've written a lot about that, if you read that book *War no more* (Hinde & Rotblat, 2003), there's a chapter in there, that'll tell exactly how I think about that.

Interestingly, Hinde's later interest in the origins of war echoed an early interest of Bowlby. In 1939, together with friend and Labour politician Durbin, Bowlby published the book *Personal aggressiveness and war* (Durbin & Bowlby, 1939) in which he explained war and aggression by connecting Freud's views with evolutionary and anthropological thinking.

### **Bowlby and Tinbergen**

The existing literature is silent about the personal relationship between Bowlby and one of the cofounders of ethology, Niko Tinbergen. In the most authoritative work on the life of Tinbergen, Kruuk (2003) does not mention Bowlby even once. Hinde remembers that Bowlby and Tinbergen had no frequent scientific contacts; Tinbergen declined Bowlby's invitation to join the weekly meetings at the Tavistock Clinic and Hinde was asked in his place (Hinde, personal communication, March 31, 2006).

Q: Do you have any idea how the relation between Tinbergen and Bowlby was?

A: Tinbergen (Tinbergen & Tinbergen, 1983) came back in his late book on autism to views that very much emphasized contact comfort. But in the intervening years I don't think Bowlby... Well they did have some contact, I do know that. They did know each other and saw each other occasionally, but I don't think they had a lot of academic discussion.

Hinde's impression is confirmed by the personal correspondence between Tinbergen and Bowlby. In a letter to Bowlby, Tinbergen acknowledged that he had not been of much help in matters of ethology:

I often wonder, looking back, why I have in the past not been able to be of real help to you, as Robert [Hinde] has so outstandingly been. The truth is that my interest in human ethology has awakened only very recently. (Tinbergen in a letter to Bowlby, n.d.; AMWL: PP/BOW/B.3/22)

Tinbergen and Bowlby may not have had much academic discussion, but there now is evidence that they had some contact on a more personal level. For example, when Tinbergen had one of his depressions in Nairobi in 1967 (cf. Kruuk, 2003) and had to return from Kenya, he consulted Bowlby in his role as a psychiatrist. He subsequently explained this move in a nine-page letter to his doctor:

I was by then so off-balance, and upon return home decided, since Dr. Henderson was away on holiday, to turn to my good friend John Bowlby, who then started me on what has turned out to be the best course I could possibly have followed. (Tinbergen in a letter to his doctor, November 29, 1967; AMWL: PP/BOW/B.3/22)

Many years earlier, in the 1950s, Tinbergen had also consulted Bowlby, this time about the mental problems of one of his children. Apparently, the child suffered from something that

looked like an autistic disorder. In a letter to Bowlby, Tinbergen looked back to that episode and mentioned Bowlby's intervention:

And above all we [Tinbergen and his wife] were concerned about these children, once we had seen the entire syndrome, temporarily, in our own children (who are now well-balanced and integrated adults) and then some of our grandchildren. The one musical boy is the eldest son of our... [child], whom you were so kind to help years ago; [he/she] is now an extremely fine [schoolteacher], and a splendid [parent]. (Tinbergen in a letter to Bowlby, n.d.; AMWL: PP/BOW/B.3/22)

Apparently, these events stirred the interest of Tinbergen and his wife in the autistic syndrome and its possible cure. In their book *Autistic children: New hope for a cure* they advocated an ethological approach to the study of children with autism and strongly supported the so-called "holding therapy" defended by Martha Welch (Tinbergen & Tinbergen, 1983; cf. Kruuk, 2003). This therapy has now fallen into disrepute since it may endanger the physical and psychological health of the children and offers no clear therapeutic benefit (Chaffin et al., 2006; Lieberman & Zeanah, 1999; O'Conner & Zeanah, 2003).

### **Theoretical issues: Instinct and psychoanalysis**

In his reworking of psychoanalytical theory and the integration of ethological findings and concepts into attachment theory, Bowlby introduced a number of concepts such as "environmentally stable" and "labile" that led to subsequent debates. Since these ethologically based attachment concepts still play such a central role in attachment theory and have stirred so much debate one may wonder what Hinde thinks about their theoretical importance, origin, and intellectual authorship.

Q: Bowlby dismissed the concept of instinct and opted for the terms environmentally stable and environmentally labile. Would you say that he adopted these concepts and terms from you?

A: Yes, that I'm quite sure of. I remember discussing the pros and cons with him. It is very easy for me to claim more than I ought to claim, but I do know that those terms came from me.<sup>8</sup>

Q: You discussed those terms with Bowlby and also discussed with him why the concept of instinct wasn't useful?

A: I was an angry young man in the 1950s and only too glad to find things that were wrong with Lorenz's theory. The concept of instinct has been criticized since then

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<sup>8</sup> Hinde's impression that it was he who suggested to use the terms environmentally stable and environmentally labile finds additional confirmation in a much earlier letter to Bowlby: "I think you are right in attributing the terms 'environmentally stable and labile' as applied to behaviour to me (...), [though] they were used earlier in other contexts by Smallhausen" (Hinde in a letter to Bowlby, September 6, 1965; AMWL: PP/BOW/K.4/15).

much more effectively by my student Patrick Bateson. That's why I called my book *Animal behaviour* (Hinde, 1966) [and not *Animal instinct*];<sup>9</sup> I don't think you would find the term instinct in that book. Instinct is more or less out of use ever since Frank Beach (1955) wrote a wonderful paper in *Psychological Review*, called "The descent of instinct. Taking the stink out of instinct".

Q: You have sometimes said that in your discussions with Bowlby he devoted too much energy to the Freudian view. Later you claimed that you were mistaken. Why was that? Being an ethologist, what do you see of lasting value in Freudian theory?

A: Well, I'm not an expert on Freudian theory. That was almost a joke between John and me. When I was reading the manuscript of his books, I said, what do you want to say with all this stuff about psychoanalysis anyway? The point is that he was trying to push his version of psychoanalytic theory into the psychoanalytic world and he was in a very difficult position, because he was severely criticized by the *British Psychoanalytical Society* because of his renegade views on defense and all that. My view of psychoanalytic theory is that Freud started terribly important issues, but he was wrong about lots of things, about instinct and libido and all that. It's interesting how a lot of my colleagues here in the Arts are involved in it. When I discuss that with them, they say, well, you have to be *au fait* with psychoanalytic theory, because so many of the writers and poets and people have based their writings and poems on it and so it's a sort of circular self-reinforcing thing for them. The lasting value of psychoanalysis is the emphasis on the unconscious and what goes on in those levels. But I repeat, I'm not an expert on psychoanalytic theory and I tend to be biased about it, simply because when I was an angry young man I criticized libido models and all that.

### **Theoretical issues: The Environment of Evolutionary Adaptedness**

Another concept Bowlby introduced is the Environment of Evolutionary Adaptedness (EEA); this concept is central to the argument of attachment theory (Bowlby, 1969/1982; Hinde, 2005; Hrdy, 1999; cf. Sable, 2004). Bowlby used the concept of the EEA to explain how humans adopted attachment behavior as a survival strategy. Mary Main (personal communication, June 28, 2005) has suggested that it was not Bowlby but Hinde who came up with the idea of the EEA. Asked about this matter, Hinde comments:

I certainly think it was something that came up between us, but which of us actually coined the term, I don't know. If you look at the orienting attitudes of ethology, the environment to which the animal is adapted is critical for understanding its behavior and certainly I took that idea to Bowlby. Whether he or I thought of the term is another issue. It's a concept that's come in for a good deal of criticism, as you know, because human environments were diverse and so on, but that's another

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<sup>9</sup> Hinde mentions a change of title of the book in a letter to Bowlby, October 27, 1965; AMWL: PP/BOW/B.3/18.

issue. I think the criticism by Kevin Laland is misguided really, I mean, he doesn't understand the historical context in which it first arose.

Hinde is referring to Laland and Brown (2002), who wrote a critical review of the concept of the EEA. Their criticism primarily concerns the stereotypical description of the EEA as a Pleistocene African savannah. According to the authors, the environment in which humans lived during a large period of time was very different for different groups of hunter-gatherers. Hence, one cannot argue that humans adapted their behavior to one specific environment. Similar criticism had been voiced previously by evolutionary anthropologists (Foley, 1996; Irons, 1998). Hinde is not impressed by the critique:

The critics [of the EEA] are a pity actually, I have to say that. The point is this, Bowlby talked about the EEA primarily as those aspects of the environment of the young child which involve the mother. It was then used by other people and it was pointed out that environments are very different, but Bowlby's real point was that all babies need to be near their mother, all babies need to suckle, all babies need contact comfort. It was mainly the things that universally mattered in the mother-child relationship when he talked about the EEA. The fact that humans have lived in all sorts of physical environments is another issue.

Elsewhere, Hinde (1982a, 1987) has expressed the view that the generic concept of the EEA was of particular importance during the development of attachment theory, but that now that attachment theory has become established, the discussion concerning the EEA is no longer relevant: "[T]hat battle is now won: we are no longer concerned with broad principles but with the nature of individual differences between mother-infant relationships" (Hinde, 1982a, p. 72; cf. Irons, 1998). However, within evolutionary psychology the notion of the EEA is still relevant (Buss, 2004, 2005).

### **Ainsworth's contribution**

So far, the contacts between Bowlby and various ethologists have been discussed and how this influenced attachment theory and subsequent animal research. We do not want to give the impression, however, that in discussing the interchange between attachment theory and ethology we consider only Bowlby to have played a crucial role as the founder of attachment theory. Attachment theory as it eventually evolved owes also much to the empirical work of Mary Ainsworth with whom Bowlby collaborated over a number of years. According to Hinde, Ainsworth's contribution may have been somewhat underestimated by historians of science.

Q: Bowlby was primarily a theoretician and it was Ainsworth who provided the link between observational data and theory...

A: Not only Ainsworth, but Jimmy Robertson as well, who was a psychiatric social worker and made those very remarkable films of which the first was *A two year old goes to hospital* (Robertson, 1952). And by chance the 2-year-old he picked out turned out to be this ravishing little girl. That made the film much more effective.

Q: But don't you think it is a bit paradoxical that Bowlby who was not an empirical researcher himself so much emphasized the role of real and observable factors in child development?

A: Well, it is a curious thing... I think it is something that he learned from Jimmy Robertson and the seminars [at the Tavistock Clinic] and you must remember that Mary Ainsworth (1967) had done observational work in Uganda which was also of very much influence... She was in London in the early fifties. *Infancy in Uganda* was published in the sixties, but she made the observations in the mid-fifties.

Q: What do you think inspired Mary Ainsworth to try and get empirical validation of Bowlby's ethological notions when she left for Uganda in 1953 while she was quite skeptic about these views as an explanation for infant-mother attachment?

A: It might well be that Mary Ainsworth just did her research in Uganda and when she came back Bowlby and she linked it up with the work of attachment. I'm not sure there was a direct link prior to that.<sup>10</sup>

Q: So attachment theory was not a one-man job?

A: Not at all, I think Mary Ainsworth doesn't get enough credit for her contribution to attachment theory. You know, she made essential contributions with her observations in Uganda, but she may not get the credit for her role in attachment theory. So was it a one-man job? Certainly not.

Ainsworth (1967; cf. Van IJzendoorn & Sagi, 1999) linked her data from the observational study in Uganda, carried out in 1954-1955, to the new theoretical framework that Bowlby had been working on since the early 1950s, when they collaborated at the Tavistock Clinic. Over her lifetime, Ainsworth's contributions included: (1) the notion of the secure base (Ainsworth, 1963; 1967); (2) a method for assessing the quality of attachment (Ainsworth, Blehar, Waters, & Wall, 1978; Ainsworth & Wittig, 1969); (3) the original tripartite classification system of attachment relationships as avoidant (A), secure (B), and resistant or ambivalent (C) (Ainsworth et al., 1978); (4) research establishing the link between maternal sensitivity and attachment security (Ainsworth et al., 1978); and (5) acknowledgement of the fact that the mother needs to be "free enough of preoccupations and anxieties of her own" (Ainsworth, 1967, pp. 397-398) to foster the establishment of a secure attachment relationship. Perhaps just as importantly, Mary Ainsworth was herself a secure base from which to explore for many students who went on to make important contributions to attachment research. These themes have been central in research on individual differences in attachment ever since.

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<sup>10</sup> Bowlby states that Ainsworth "must have known a bit about it [ethology] before she left [for Uganda in 1954], because I was getting enthusiastic about it in 1951 and 1952 when she was here [at the Tavistock Clinic]; she must have shown quite a lot of interest in it (...) I remember having quite prolonged debates on paper (...) and that's how she became ethologically oriented" (Smuts, 1977).

**Conclusion**

In this contribution, on the basis of interviews with Robert Hinde, we explored the cross-fertilization of attachment theory and ethology. More specifically, we have taken a closer look at the influence of John Bowlby and Robert Hinde on each other's thinking and research. From archival materials and from personal accounts by various contemporary informants, we may conclude that from the 1950s Bowlby was in personal and scientific contact with leading European scientists in the field of ethology, namely Niko Tinbergen, Konrad Lorenz, and especially the rising star of ethology Robert Hinde. Using the viewpoints of this emerging science and reading extensively in the ethology literature, Bowlby developed new explanatory hypotheses for what is now known as human attachment behavior. In particular, on the basis of ethological evidence he was able to reject the dominant "cupboard love" theory of attachment prevailing in psychoanalysis and learning theory of the 1940s and 1950s. He also introduced the concepts of environmentally stable or labile human behavior allowing for the revolutionary combination of the idea of a species-specific genetic bias to become attached and the concept of individual differences in attachment security as environmentally labile strategies for adaptation to a specific childrearing niche. Alternately, Bowlby's thinking about the nature and function of the caregiver-child relationship influenced ethological research (see Suomi, 1995), and inspired students of animal behavior such as Tinbergen, Hinde, and Harlow. Bowlby spurred Hinde to start his ground breaking work on attachment and separation in primates (monkeys and humans), and in general emphasized the importance of evolutionary thinking about human development that foreshadowed the new interdisciplinary approach of evolutionary psychology. Obviously, the encounter of ethology and attachment theory led to a genuine cross-fertilization.

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**INTERMEZZO.**

**FROM THEORETICAL CLAIMS TO EMPIRICAL  
EVIDENCE**



In the previous chapter we have seen how Bowlby from the mid-1950s, “with Robert Hinde's generous and stern guidance” (Bowlby, 1980b, p. 650), was introduced to the theoretical principles of ethology and studies in animal behavior. It became clear that from that point in time it was Bowlby's goal “to rewrite psychoanalysis in the light of ethological principles” (Dinnage, 1979, p. 325), because, according to Bowlby, “the theory was a mess – there was no suitable theory really” (Senn, 1977). At the same time it was Bowlby's “belief that problems of method and theoretical interpretation are best approached from a firm base in empirical data” (Bowlby, 1961d, p. xiii). However, his problem was that the empirical basis to support his new view of the mother-child bond was exactly what he was lacking.

Therefore, Bowlby started collecting evidence that could support his new views. The interdisciplinary approach he took to accomplish this fact is reflected in four symposia held in the late 1950s and early 1960s. For these symposia it was Bowlby's goal to invite a small number of people

who were already engaged in first-hand studies of the behaviour of infants and young children in a social setting, ... representatives of those making similar studies in animals, ... [and] a number of clinicians who could contribute from their experience of what seems pathologically and therapeutically relevant. (Bowlby, 1961d, p. xiv)

During these meetings Bowlby emphasized the need for an exchange of ideas between different fields of study and the fact that “these meetings have been convened in the belief that an understanding of mother-infant interaction in humans will come soonest if the knowledge and skills of several different groups of workers are pooled” (Bowlby, 1965, p. xiii).

During these so-called Ciba-conferences it was important that priority was given to “empirical studies, especially those that utilize first-hand observations of what actually happens between infant and mother. In the past these have been scarce, but an increasing number of investigators are now awakening to their interest and value” (Bowlby, 1963b, p. xi). After the symposia, Bowlby recapitulated that it was interesting to see “how the work... reported had been influenced or even initiated as a result of discussions that had occurred at... previous meetings or as a result of visits or correspondence that had been started at them” (Bowlby, 1969, p. xiv).

In an interview Bowlby once stated that “in 1957 I started tackling theory” (Senn, 1977). In that year he proposed “a new theoretical framework for understanding problems of personality development and pathology,” (Bowlby, 1980b, p. 650) but “because this framework [wa]s radically different to the frameworks adopted by psychoanalysts and learning theorists, it remain[ed] controversial” (ibid.). The controversiality of the theory would diminish though, “thanks in large part to the related studies of rhesus monkeys undertaken by Harry Harlow in the US and Hinde over here” (ibid.). Harlow and Hinde were to carry out the experimental developmental research that Bowlby needed for the empirical validation of his ideas. The cross-fertilization of the work and ideas of Harlow and Bowlby is the subject of

the next chapter: attention will be drawn to what happens “when strangers from strange disciplines first meet” (Bowlby, 1969, p. xiii).

## CHAPTER 4.

# **“WHEN STRANGERS MEET”: JOHN BOWLBY AND HARRY HARLOW ON ATTACHMENT BEHAVIOR**

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**Abstract**

From 1957 through the mid-1970s, John Bowlby, one of the founders of attachment theory, was in close personal and scientific contact with Harry Harlow. In constructing his new theory on the nature of the bond between children and their caregivers, Bowlby profited highly from Harlow's experimental work with rhesus monkeys. Harlow in his turn was influenced and inspired by Bowlby's new thinking. On the basis of the correspondence between Harlow and Bowlby, their mutual participation in scientific meetings, archival materials, and an analysis of their scholarly writings, both the personal relationship between John Bowlby and Harry Harlow and the cross-fertilization of their work are described.

**Keywords:** attachment theory, animal psychology, ethology, animal behavior, infant-mother relations, history

## Introduction

Today, one can pick up almost any introductory, general, or developmental psychology textbook (e.g., DeHart, Sroufe & Cooper, 2004; Cole & Cole, 2005) and find references to British child psychiatrist John Bowlby (1907-1990) and American animal psychologist Harry Harlow (1905-1981). Quite often their work is discussed in tandem. Bowlby was a clinician by training and Harlow an experimentalist. Despite these rather different backgrounds, the two men had several things in common. One of them was that they showed no hesitation in expressing views that went against the prevailing *Zeitgeist*. In the 1950s and 1960s, both Bowlby and Harlow formulated new ideas on the nature of the bond between child and caregiver. They defied the prevailing psychoanalytic and learning theoretical views that dominated psychological thinking from the 1930s. Although it has been argued (Singer, 1975) that Harlow's experimenting had no influence on Bowlby's theorizing, here it will become clear that Bowlby used Harlow's surrogate work with rhesus monkeys as much needed empirical support for his emerging theory of attachment in which he explained the nature and function of the affectional bonds between children and their caregivers (Bowlby, 1958c, 1969/1982). In his turn, Harlow was influenced by Bowlby's thinking and tried to model his rhesus work to support Bowlby's new theoretical framework (e.g., Seay, Hansen & Harlow, 1962; Seay & Harlow, 1965).

The theories of Harlow and Bowlby are well-known but so far little was known about the personal and professional relationships between these two giants in the field. In this contribution, on the basis of the correspondence between Harlow and Bowlby,<sup>11</sup> their joint participation in scientific meetings, archival materials, and an analysis of their scholarly writings, an attempt is made to delineate the cross-fertilization of their work during the most active years of their acquaintance from 1957 through the mid-1970s. It will be demonstrated that Bowlby and Harlow's interests converged as Harlow shifted his focus to a developmental approach shortly before the two met. Their introduction at a distance by British ethologist Robert Hinde was the beginning of an exchange of ideas that resulted in groundbreaking experimenting and theorizing that affects the field of developmental psychology to this day.

### **Bowlby's early career (1938-1957): from Kleinian psychoanalysis to real life**

John Bowlby, who received a Master's degree from Cambridge University and an MD from University College Hospital in London, was trained in psychoanalysis. He practiced as a clinician and joined the staff of the Tavistock Clinic in London in 1946, where he spent the remainder of his professional career (cf. Van Dijken, 1998; Van Dijken et al., 1998). There is no doubt that he will be remembered in history as “the father of attachment theory”. Bowlby's career evolved on the basis of a single theme, the relationship between mother and infant, and the effects of the pattern established early on upon the developmental outcome of the offspring. He mounted a scientific challenge to dominant psychoanalytical views in British psychiatry, such as those held by Anna Freud and Melanie Klein (Berrios & Freeman, 1991).

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<sup>11</sup> The correspondence between Harry Harlow and John Bowlby (thus far twenty letters were recovered) resides with Helen A. LeRoy.

In an interview with Robert Karen (1994, pp. 45-46), Bowlby described an influential experience in 1938, while training under the supervision of psychoanalyst Melanie Klein. Contrary to Klein, who believed all behavior was motivated by inner feelings, Bowlby felt that external relationships, e.g., the way a parent treated a child, were important to consider in understanding the child's behavior. At the time, he was seeing an anxious, hyperactive child as a patient five days a week. The boy's mother would sit in the waiting room, and Bowlby noticed that she too seemed quite anxious and unhappy. When he told Klein he wanted to talk to the mother as well, Klein refused adamantly, dismissing the mother as a possible causal or related factor in the child's behavior. Bowlby was thoroughly annoyed and gradually distanced himself from the Melanie Klein school of thought.<sup>12</sup> Later, in 1948, through the work of Tavistock social worker James Robertson, with whom he would work closely over the years, Bowlby became interested in recording the responses shown by children between the ages of 12 months and 4 years upon separation from their mothers or attachment figures (Bowlby, 1960a).

In 1950, as part of a WHO project, Bowlby (1951) undertook a literature survey in order to test the hypothesis that "separation experiences are pathogenic" (Bowlby, 1958c). Homeless children had become a major problem after World War II, and in his WHO report, Bowlby warned that children deprived of their mothers were at risk for physical and mental illness. After surveying the literature, Bowlby (Bowlby, Robertson, & Rosenbluth, 1952, p. 82) concluded:

It became clear that this hypothesis is well supported by evidence and the team is now planning to concentrate on understanding the psychological processes which lead to the grave personality disturbances – severe anxiety conditions and psychopathic personality – which we now know sometimes follow experiences of separation.

We may conclude, then, that Bowlby was convinced at the time that (repeated) separation experiences may seriously harm the mental health of children and that the existing literature (e.g., on hospitalization) proved his point of view. He valued empirical studies and emphasized the importance of objective observation of real-life experiences. However, he still lacked the theoretical apparatus to understand the causal mechanisms behind the phenomena he observed. Also, he knew of no experiments that manipulated the potentially relevant variables in the domain of attachment formation. It was in this situation that he chanced upon the emerging science of ethology and the experimental work of Harlow.

In the subsequent years Bowlby made increasing use of ethological findings and theorizing guided by British ethologist, colleague and life-long friend Robert Hinde (Van der

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<sup>12</sup> Even fifty years later, Bowlby still became angry when relating his conflict with Klein over the relevant factors in the explanation of a young boy's anxiety. Klein replied to Bowlby's request to see the mother: "Dr. Bowlby, we are not concerned with reality, we are concerned only with the fantasy" (Kagan, 2006, p. 43).

Horst et al., 2007). Bowlby (1957, 1960c) acknowledged a deep and pervasive interest in ethology beginning about 1951, which was sparked by Konrad Lorenz's (1935, 1937) gosling work. His talk to the members of the *British Psychoanalytical Society* on June 19, 1957 (published as Bowlby, 1958c) testifies of his growing confidence in the relevance of ethology.

### **Harlow's early career (1930-1957): from conditioning rats to studying monkey love**

Harry Harlow received a PhD in psychology from Stanford University in 1930 and spent the remainder of his academic career as a professor at the University of Wisconsin-Madison. Harlow was educated in the psychological tradition of the 1920s and 1930s, a time when psychology was making an effort to become a 'real' science. Studying behavior was a case of controlling the environment and varying one particular condition. It was a time when behaviorist views carried the day and the conditioned responses of Norwegian rats were the key to understanding mental life. So, when Harlow was appointed at Wisconsin in 1930 and found that the psychology department's chairman had the rat laboratory dismantled and it was not about to be replaced, he was greatly inconvenienced (Harlow, 1977, p. 138-139; Suomi & LeRoy, 1982).

It was only at the suggestion of the chairman's wife that Harlow decided to study primates at the local zoo and he soon found out that the intellectual capabilities of the monkeys were far greater than those of rats (*ibid.*). To study these capabilities more rigorously and effectively Harlow developed the Wisconsin General Test Apparatus (WGTA; Harlow & Bromer, 1938) by which it was possible to present the monkeys with a large number of learning tests in a highly standardized way. With it he tested the monkeys with discrimination learning and memory tasks (e.g., Harlow, 1943, 1944). Harlow's next step was to study cortical localization of learning capabilities by doing lesion studies with monkeys (e.g., Harlow & Dagnon, 1943; Harlow & Settlege, 1947; Moss & Harlow, 1948). By lesioning different areas of the brain, Harlow noted that each of the operated monkeys performed differently on the WGTA tests. This work was basically similar to the work done by Lashley (e.g., Lashley, 1950).

In the late 1940s, Harlow achieved “a major conceptual and methodological breakthrough” (Suomi & LeRoy, 1982, p. 321) by identifying the formation of learning sets in monkeys (Harlow, 1949). Harlow demonstrated that his monkeys “learned to learn” and that they acquired a strategy for problem-solving. As methods of studying processes underlying monkey learning were exhausted, Harlow in the early 1950s turned to studying motivation and the ontogeny of learning. This type of developmental research required the establishment of a breeding colony of rhesus monkeys. It was at this point that Harlow's attention was drawn to the phenomenon of affection.

Harlow had always had problems importing monkeys: apart from being very expensive, they were often ill upon arrival and infected the other monkeys in the laboratory (Harlow, 2008). In 1956, following Van Wagenen's (1950) procedures, he decided to raise his own rhesus monkeys, and thus the Wisconsin lab became the first self-sustaining colony of monkeys in the US. The monkeys were kept separated at all times to avoid any spread of disease. The results of this procedure were remarkable for those who could see it: the

monkeys Harlow raised were physically perfectly healthy, but their social behavior was very awkward. They were simply unable to socially interact with each other. Another striking observation Harlow made was that the infant monkeys “clung to [the diapers on the floor of their cage] and engaged in violent temper tantrums when the pads were removed and replaced for sanitary reasons” (Harlow, 1958, p. 675). Harlow wondered whether these observations could mean anything for the needs of human children.

Just two months prior to Bowlby's *British Psychoanalytical Society* address which discussed in great depth the child's tie to the mother, Harlow spoke on April 20, 1957 at a conference in Washington, D.C. The title of his address was the “Experimental Analysis of Behavior” and it included a discussion of trends in this area. Harlow began his address by stating that “no behavior is too complicated to analyze experimentally, if only the proper techniques can be discovered and developed” (Harlow, 1957, p. 485). He went on to emphasize the importance of a developing trend toward longitudinal studies (psychology had traditionally been concerned with a cross-sectional approach), and he told how:

I have followed with interest the changes in my own research programs and the development of these programs. The experimental S that has consumed almost all my research time has been the rhesus monkey. When I initially approached the experimental analysis of this animal's behavior, I approached it in the classical, cross-sectional manner... If it had not been for the fact that my monkey Ss continued to live after they had solved a problem and that they were not expendable in view of the available financial support, I might still be engaged in cross-sectional studies of the monkey's behavior. (Harlow, 1957, p. 487)

These comments clearly indicate that Harlow was moving towards experimental developmental research, the type of research that Bowlby so badly needed at the time. Harlow was now on the threshold of the affectional studies, for which he would become famous. He explained that:

More recently we have planned and initiated much more extensive longitudinal studies in which we have separated infant rhesus monkeys from their mothers at birth and raised them under the controlled conditions of the laboratory. We have been successful in raising over fifty of these young animals, and we have obtained data on their learning development from birth through three years of age... We have found the longitudinal approach to the experimental analysis of behavior interesting and even exciting, and we are now extending this type of analysis to other areas than learning, perception, and motivation... [W]e are planning and conducting systematic longitudinal studies on the development of emotional responses. (Harlow, 1957, p. 488)

Just like Bowlby before his fellow psychiatrists of the *British Psychoanalytical Society*, Harlow (1957, p. 490), before an audience of clinical psychologists, stressed the

importance of observational methods in this process, something that was of course very obvious to him.

At the present time... we are interested in tracing the development of various patterns of emotional behavior... We began by looking for response patterns which might fit... But this observational study... is gradually taking on the characteristics of an experiment. As we gain sophistication about the monkey's emotional responses, we become more selective in the patterns which we observe.

It was because of their mutual interest in this area of emotional behavior and responses that Harlow and Bowlby became acquainted. In Harlow's words: "It is an understatement to add that we have research interests in common" (Harlow in a letter to Bowlby, January 27, 1958).

### **Ethology and animal psychology: contrasting approaches to animal behavior**

It was not self-evident for a British ethologically oriented psychiatrist and an American animal behaviorist to meet in those days. In the 1950s, there was a great barrier between ethologists (who were mostly biologists by training) and students of animal behavior (mostly psychologists). Ethologists emphasized observation of animals in their natural habitat, whereas comparative psychologists relied on rigorous experimentation in the laboratory. The culmination of this debate was a 1953 critique by Theodore Schneirla's student Daniel Lehrman of Lorenz's concept of instinct, at that time the central theoretical construct of ethology (Lehrman, 1953). But in contrast to what might be expected, when Lehrman visited Europe in 1954 and met with leading ethologists, he was very well received. Just like many of the ethologists, Lehrman had a background in evolutionary biology and ornithology and this may have been essential in bridging their differences. Although Lorenz never acknowledged Lehrman's ideas, they later became mainstream ethology (Griffiths, 2004). Eventually Hinde (1966) wrote his authoritative book *Animal behaviour* which was essentially "a synthesis of ethology and comparative psychology" (cf. Van der Horst et al., 2007, p. 9-10).

In this climate of contrasting views, Hinde and Harlow met for the first time in Palo Alto in early 1957 at a conference organized by Frank Beach that was intended to bring together a group of European ethologists (Niko Tinbergen, Gerard Baerends, Jan van Iersel, David Vowles, Eckhardt Hess and Robert Hinde) with a group of mainly North-American comparative and experimental psychologists (Frank Beach, Donald Hebb, Daniel Lehrman, Jay Rosenblatt, Karl Lashley and Harry Harlow). Hinde has good memories of the event: "It was a wonderful conference, about three weeks, [where you had] nothing to publish, and if you did not finish what you had to say today there was always tomorrow" (Robert Hinde, personal communication, March 14, 2007). After their first encounter, Hinde and Harlow met several times in the late fifties and sixties. Although they influenced each other and their relationship was very cordial in the days they interacted, Hinde in retrospect remembers that at that time their approaches were still rather far apart:

I must have next met Harry when I visited Madison and was appalled by this room full of cages with babies going “whoowhoowho” and Harlow had no sensitivity at that point that he was damaging these infants. At that time I was beginning to work on mother-infant relations in monkeys myself, but I already knew enough about monkeys to know that that “who”-call was a distress call. These experiments had their restrictions, but they did show certain important things. After that I saw him at least once a year for a while as he asked me to join his scientific committee. Of course his results influenced my way of thinking, but I was then an ethologist and not keen on his laboratory orientation. And I could never have attempted to do the sort of research that he did because our colony only had six adult males and two or three females in each group. We attempted to create an approximation to a normal social situation: it was a long way off, of course, but at least it was social. (Robert Hinde, personal communication, August 22 and 26, 2005; March 14, 2007)

Despite these differences in theoretical orientation, it was Robert Hinde who would eventually establish contact between Bowlby and Harlow. At the Palo Alto conference, Hinde and Harlow had a discussion on motherhood and after returning home Hinde informed Bowlby that Harlow was interested in Bowlby’s recent work on this subject (Stephen Suomi, personal communication, September 27, 2006; Karen, 1994; Hrdy, 1999; Blum, 2002; Van der Horst et al., 2007).

### **Harlow and Bowlby become acquainted in 1957**

It was just several months later that Bowlby and Harlow introduced themselves by letter. The written record of their relationship commenced with a letter dated August 8, 1957 in which Bowlby expressed his interest:

Robert Hinde tells me that you were interested in my recent paper when he showed it to you at Palo Alto and at his suggestion I am now sending you a copy. I need hardly say I would be most grateful for any comments and criticisms you cared to make. I shall be at the Center at Palo Alto from mid-September and will be preparing it for publication then. Robert Hinde told me of your experimental work on maternal responses in monkeys. If you have any papers or typescripts I would be very grateful for them. If there were a chance, I would try to visit you next Spring when I hope to be moving around U.S.A. (Bowlby in a letter to Harlow, August 8, 1957)

The paper which Bowlby sent to Harlow at the time was a draft of “The nature of the child’s tie to his mother” (Bowlby, 1958c). Harlow replied by return of post, thanking Bowlby for the paper, which he several years later (in a letter to Bowlby, March 25, 1959) would refer to as a “reference bible”:

[Y]our interests are... closely akin to a research program I am developing on maternal responses in monkeys. I certainly hope that you can pay a visit to my

laboratory sometime during this forthcoming year. At the moment our researches are just getting underway, and I hope to use these materials for my American Psychological Association Presidential Address in September, 1958. This address will be the first formal presentation of these researches. (Harlow in a letter to Bowlby, August 16, 1957)

### **Mutual referencing after 1957**

It was only after the two men began corresponding in August, 1957, that they began referring to each other's writings. A review of Bowlby's publications from 1951-1957 (Bowlby, 1951, 1953, 1957; Bowlby & Robertson, 1952; Bowlby et al., 1952, 1956; Robertson & Bowlby, 1953) yields no mention of Harlow's work. Likewise, we find no reference to Bowlby's work in Harlow's first developmental writings (Harlow, 1957).

The early correspondence resulted in the planning of mutual visits and in the exchange of reprints.<sup>13</sup> Seven of Bowlby's publications (Bowlby et al., 1952; Bowlby et al., 1956; Bowlby, 1958c, 1960a, 1960b, 1960c, 1961c) have been found in Harlow's reprint collection, in addition to two volumes on *Attachment and Loss* (Bowlby, 1969/1982, 1973). It is especially interesting to see Harlow's notes jotted in the margins of Bowlby's papers.

As a result of the interchange, the first reference to Harlow's work appears in Bowlby's work (Bowlby, 1958c). This paper was an expanded version of an address Bowlby gave before the *British Psychoanalytical Society* on June 19, 1957. The paper is concerned with conceptualizing the nature of the young child's tie to his mother, the dynamics which promote and underlie this tie. Bowlby described four alternative views found in the psychoanalytic or psychological literature at the time. He then went on to present his own theoretical perspective. He emphasized that his view was based on direct observation of infants and young children, rather than on retrospective analysis of older subjects as was the typical base for psychoanalytic theorizing at the time. Bowlby (1958c, p. 351) went on to state:

The longer I contemplated the diverse clinical evidence the more dissatisfied I became with the views current in psychoanalytical and psychological literature and the more I found myself turning to the ethologists for help. The extent to which I have drawn on concepts of ethology will be apparent.

The four then contemporary views he described were first of all the cupboard-love theory of object relations, according to which the physiological needs for food and warmth are met by the mother, through which the baby gradually learns to regard the mother as the source of all gratification and love. Secondly, primary object sucking, which states that the

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<sup>13</sup> Note that reprints at the time had to be typed anew, because the Xeroxing machine was still a luxury of the future. In order to make multiple copies to exchange their writings, researchers had to resort to having papers typed several times or to reproducing them by mimeograph. On the mailing list for Harlow's papers we find among others the names of Mary Ainsworth, Gerard Baerends, John Bowlby, Julian Huxley and René Spitz.

infant has a built-in need to orally attach to a breast and subsequently learns the breast is attached to the mother and then relates to her also. Thirdly, primary object clinging, according to which the infant has a built-in need to touch and cling to a human being, independent of food, but just as important. And finally, primary return-to-womb craving, which holds that the infant resents its removal from the womb at birth and wants to return there.

Bowlby then described his own hypothesis, one of much greater complexity and quite controversial at the time (Karen, 1994; Hrdy, 1999), as “Component Instinctual Responses”. He believed that five responses comprise attachment behavior – sucking, clinging, following, crying, and smiling – also acknowledging that many more may exist. He explained that his theory was “rooted firmly in biological theory and requires no dynamic which is not plainly explicable in terms of the survival of the species” (Bowlby, 1958c, p. 369).

A main point of Bowlby's argument was that no one response was more primary than another. He believed it was a mistake to emphasize sucking and feeding as the most important. Pointing out the inadequacy of human infant studies to date in terms of illustrating his hypothesis, Bowlby turned to observation of animals. It was in this context that Bowlby first cited Harlow's research. He clearly used Harlow's findings to undermine the psychoanalytic idea that all attachment develops through oral gratification. Harlow had specifically investigated the importance of clinging. Bowlby cited Harlow's yet unpublished nonhuman primate data “on the attachment behaviour of young rhesus monkeys” (later published as Harlow, 1958):

Clinging appears to be a universal characteristic of primate infants and is found from the lemurs up to anthropoid apes and human babies... Though in the higher species mothers play a role in holding their infants, those of lower species do little for them; in all it is plain that in the wild the infant's life depends, indeed literally hangs, on the efficiency of his clinging response... In at least two different species... there is first-hand evidence that clinging occurs before sucking... We may conclude, therefore, that in sub-human primates clinging is a primary response, first exhibited independently of food. Harlow... removed [young rhesus monkeys] from their mothers at birth, they are provided with the choice of two varieties of model to which to cling and from which to take food... Preliminary results strongly suggest that the preferred model is the one which is more 'comfortable' to cling to rather than the one which provides food. (Bowlby, 1958c, p. 366)

### **Harlow and Bowlby finally meet in 1958**

After the first two letters in August, 1957, eight additional letters were exchanged during the period Bowlby spent at the Palo Alto Center from mid-September, 1957 through mid-June, 1958. In these letters, the two men discussed their mutual interests and made arrangements for Bowlby to visit Harlow's lab in Madison as Bowlby was finally able to carry out the plans of a visit he had mentioned in his first letter. Bowlby attended one of Harlow's lectures on

April 26, 1958 and visited his laboratory for two days in June of that same year (Smuts, 1977; Zazzo, 1979). In a letter to his wife Bowlby shows his enthusiasm after their first encounter:

You may remember I went to hear the final paper of the [Monterey] conference – an address by Harry Harlow of Wisconsin on mother infant interaction in monkeys. His stuff is a tremendous confirmation of the Child's Tie paper, which he quoted. Afterwards Chris[toph Heinicke] heard him remark, in very good humour, to a friend: “You know, I thought I had got hold of a really original idea [and] then to find that bastard Bowlby had beaten me to it!” This is not really true [and] I think we can say it's a dead heat – [and] the work of each supports the other. We had a very aimable chat [and] arranged to meet in June. (Bowlby in a letter to Ursula, April 28, 1958; AMWL: PP/BOW/B.1/20)

The lecture Bowlby attended was a presentation Harlow gave at the meetings of the *American Philosophical Society* (published as Harlow & Zimmermann, 1958) on the development of affectional responses in infant monkeys. There Harlow touched upon, in contrast with Bowlby's earlier in-depth analysis of the same matter, the various psychoanalytic theoretical positions concerning the bond of the infant to the mother. Referring to their personal contacts, Harlow (Harlow & Zimmermann, 1958, p. 501) mentioned that “Bowlby has given approximately equal emphasis to primary clinging (contact) and sucking as innate affectional components, and at a later maturational level, visual and auditory following”. This was Bowlby's first appearance in a Harlow publication.

Bowlby visited Madison in June, 1958 and wrote to Harlow on the 26th, thanking him for his hospitality, and adding: “I shall look forward to keeping in touch... I hope too you will put me on your list to send mimeograph versions as and when your stuff goes further forward. We will reciprocate”. By June of 1958, the earlier formal salutations and closings “Dear Professor Harlow” and “Yours sincerely” or “Dear Dr. Bowlby” and “Cordially” had changed to a much more informal tone, becoming “Dear Harry” and “Yours ever, John”, or “Dear John” and “Best personal wishes, Harry”.

Two months later, on August 31, 1958, Harlow delivered his famous presidential address on “The nature of love” to the *American Psychological Association*. “The recent writings of John Bowlby” are mentioned in the published paper (Harlow, 1958, p. 673), to the effect that he recognized the mother's importance in providing the infant with intimate physical contact, as well as serving as a source of nutrition. Harlow also positively mentioned Bowlby's notion of ‘primary object following’, i.e. the tendency to visually and orally search the mother. The fact that Bowlby is mentioned twice in the presidential address is of some significance given that Harlow mentions but six names of researchers and hardly discusses their ideas.

### **Ethology further emphasized in Bowlby's work**

It was in July, 1959, that Bowlby (1960c) read a paper on ethology before the Congress of the *International Psychoanalytical Association* in Copenhagen. Bowlby began his paper by

remarking that eight years had now passed since his interest in ethology had been aroused, initially by Lorenz's gosling work.

From this time forward the further I read and the more ethologists I met the more I felt a kinship with them. Here were first-rate scientists studying the family life of lower species who were not only making observations that were at least analogous to those made of human family life but whose interests, like those of analysts, lay in the field of instinctive behaviour, conflict, and the many surprising and sometimes pathological outcomes of conflict... A main reason I value ethology is that it gives us a wide range of new concepts to try out in our theorizing. (Bowlby, 1960c, p. 313)

At the same time, Bowlby was cautious about extrapolating or generalizing from one species to another. He shared this restraint with Harlow who often reiterated that "monkeys are not furry little men with tails". Both, however, were convinced of the importance of animal research in providing a better understanding of human social behavior. Bowlby (1960c, p. 314) expressed his view thus:

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... [W]e share anatomical and physiological features with lower species, and it would be odd were we to share none of the behavioural features which go with them.

Carrying the notion further, Bowlby explained his efforts to use ideas gleaned from ethology in order to understand the ontogeny of what psychoanalysts called 'object relations'. For a specific example of instinctual response systems present in the young, which facilitate the attachment of the infant to a mother figure without the mother's active participation, Bowlby (1960c, p. 314) referred to and cited Harlow's surrogate mother research: "a newborn monkey will cling to a dummy provided it is soft and comfortable. The provision of food and warmth are quite unnecessary. These young creatures follow for the sake of following and cling for the sake of clinging".

Several pages later, in discussing the consequences of disrupting the mother-infant bond, Bowlby mentioned the substitution of one behavior for another due to frustration when the normal event was blocked, e.g., thumb sucking or overeating when denied maternal access. He drew a parallel with nonnutritive sucking in chimpanzees and rhesus monkeys:

In Harlow's laboratory I have seen a full-grown rhesus female who habitually sucked her own breast and a male who sucked his penis. Both had been reared in isolation. In these cases what we should all describe as oral symptoms had developed as a result of depriving the infant of a relationship with a mother-figure... May it not be the same for oral symptoms in human infants? (Bowlby, 1960c, p. 316)

In his conclusions, Bowlby once again stated that an understanding of biological processes is required in order to understand the psychological concomitants of biological processes. Two months later, in September, 1959, the first symposium organized by Bowlby was held at the Tavistock Clinic and Harlow was an invited participant.



*Figure 5. Ciba-conference group photograph 1965. From left to right: Jay Rosenblatt, unknown, Martin Richards (at back), unknown, Mavis Gunther, Harriet Rheingold, unknown, David Hamburg (in centre), unknown, Jack Gewirtz (at back), Harry Harlow, Mary Ainsworth, unknown, Tony Ambrose (in centre), Dorothy Heard, unknown, unknown, unknown, unknown, John Bowlby, unknown. Picture courtesy of the Wellcome Library, London (AMWL: PP/BOWL.31).*

#### **Mutual contacts: the Ciba-symposia from 1959 to 1965**

The initial introduction by Hinde and Bowlby's visit to Harlow's laboratory led to a fruitful cooperation during the following years. Just prior to a Chicago meeting, Harlow invited Bowlby to visit the University of Wisconsin again, but Bowlby replied with regrets on March 30, 1961, stating that he was already booked up with engagements relative to a forthcoming Chicago trip and would hope to visit Harlow's lab in 1962 or 1963 during a "more leisurely trip in the States. Looking forward to seeing you in the Autumn" (Bowlby in a letter to Harlow,

March 30, 1961). Bowlby was undoubtedly referring to the second of four so-called Ciba-symposia<sup>14</sup> to be held in London in the fall of 1961.

The Ciba-symposia followed the design for interdisciplinary discussion Bowlby had first experienced during the meetings of the WHO on the psychobiological development of the child, which he attended in the early 1950s (Tanner & Inhelder, 1971; cf. Foss, 1969). Bowlby was impressed by the series' innovative format: the meetings brought together a small group of researchers from different countries and disciplines for the purpose of promoting the knowledge of the subject matter and enhancing a mutual understanding of each other's work and views.

Thus, following this model, Bowlby convened and chaired the Tavistock study group on mother-infant interaction, a series of four meetings at two-year intervals, held in the house of the Ciba foundation in London between 1959 and 1965. Harlow was a major participant of and contributor to the Ciba-symposia in 1959 (Harlow, 1961), 1961 (Harlow, 1963), and 1965 (Harlow and Harlow, 1969), but was unable to attend the third session in 1963. In his introduction to the proceedings of the last meeting, Bowlby contended that his early hopes had come true:

As the series of meetings proceeds, reserves and misconceptions, inevitable when strangers from strange disciplines first meet, will recede and give place to an increasing grasp of what the other is attempting and why; to cross-fertilization of related fields; to mutual understanding and personal friendship. (Bowlby, 1969, p. xiii)

It is clear that both Harlow and Bowlby shared these positive feelings about the effectiveness of the symposia and that Bowlby was very pleased with the way things worked out. During the second study group, on September 7 and 9, 1961, Bowlby wrote to his wife Ursula:

There is widespread enthusiasm at the way the study group is going, regrets we have so little time, [and] shows demand we meet again in [two] years time – (after our holiday next time). The atmosphere is much less tense this time – Jack Gewirtz no longer a problem child – [and] communication is quick, spontaneous [and] effective. The two year gap, I'm sure, is better than one year. It has given plenty of time for everyone to digest the lessons of the first meeting, [and] there has been much private visiting [and] private communication between the members since. The result is that this time it is the atmosphere of a house-party. Harry H[arlow] has got to London last night so missed the first two days but is now with us... Tomorrow he is on the platform [and] we should probably have some firework. I confess I feel

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<sup>14</sup> The four Ciba-symposia (organized in 1959, 1961, 1963 and 1965) were funded by the Ciba foundation, a foundation formed in 1949 by the Swiss company Ciba (now Novartis) that promotes scientific excellence by arranging scientific meetings. The four meetings are often also referred to as meetings of the Tavistock study group.

rather proud of this party, both as a convener [and] chairman, I can take much credit for the party atmosphere, [and] also because so much of the work reported owes its origin to my stimulation. We have had [three] excellent presentations (Mary Ainsworth, Peter Wolff [and] Heinz Prechtl) [and] two that were too long (Jack Gewirtz [and] Tony Ambrose). In addition, Robert [Hinde] has shone [and] Rudolf Schaffer did very well in a brief contribution. They say Thelma [Rowell] is on the best of things [and] presents her Cambridge monkeys tomorrow. (Bowlby in a letter to Ursula, September 7, 1961; AMWL: PP/BOW/B.1/24)

The study group is over [and] has been a tremendous success. Everyone has enjoyed it [and] feel they have profited from it. It has been extremely friendly [and] intense, together with cautious and effective discussion. We managed to cover a lot of ground without hurry. It is striking how far [and] fast people have developed in the two years since we last met. In a sense it has become a kind of club [and] seems likely to have far reaching effects. (Bowlby in a letter to Ursula, September 9, 1961; AMWL: PP/BOW/B.1/24)

After the last of the Ciba-symposia, Bowlby wrote to Harlow that he was "very glad indeed that you were able to be with us last week and to give us such a stimulating account of your work" (Bowlby in a letter to Harlow September 21, 1965). Bowlby's sentiments concerning the ultimate success of the four-part series are echoed in a letter Harlow wrote to Bowlby:

It was my personal opinion that the last [Ciba-symposium] was more informative than the first two... I was impressed by the fact that the people who reported both in formal papers and in discussion were far more sophisticated about the problems... and I think I can include myself within this generalization. Furthermore, I thought that members of the conference communicated with each other far more effectively than they had... and I believe that this was a result of increasing sophistication in the nature of the problems attacked and in the development of adequate measurement and techniques. I personally believe that the Tavistock series... achieved a great deal. (Harlow in a letter to Bowlby, October 18, 1965)

There is no doubt, then, that the Ciba-symposia achieved their goal. By bringing together major figures in the field, such as Mary Ainsworth, John Bowlby, Jack Gewirtz, Harry Harlow, Robert Hinde, Harriet Rheingold, and Theodore Schneirla, they were able to further the mutual understanding of animal psychologists, ethologist, and learning theorists, and to advance the understanding of infant behavior (Foss, 1961, 1963, 1965, 1969). In particular, they allowed Bowlby and Harlow to meet on a regular basis and to discuss each other's ideas thoroughly.

**Bowlby's writings in the early 1960s: using Harlow's empirical findings as a secure base**

In the early 1960s, in several papers, Bowlby (1960a, 1960b, 1961a, 1961b, 1961c) expanded upon the theme of separation anxiety. He intended it as a corollary to his earlier treatise on the child's tie to the mother (Bowlby, 1958c). In a review of the literature (Bowlby, 1961a; cf. Bowlby, 1960a), he presented his new conceptualization of separation anxiety in the same detailed manner as he elaborated on the nature of the child's tie to the mother in that previous paper. Before presenting his own theory, Bowlby delineated five different theories of anxiety related to the child's attachment to the mother. First, he described 'transformed libido' theory, a view held by Freud until 1926, where he attributed anxiety to a child's unsatisfied libido upon separation from an attachment figure. Second, he mentioned the view that separation anxiety may mirror birth trauma and is the counterpart to the craving of the infant in the 'return-to-the-womb theory' met before. The third view Bowlby discussed was that of 'signal theory', which held that anxiety behavior has a function and results from a safety device to ensure that the separation will not be long and implied that the child's tie to the mother derives from a secondary drive. The fourth view presented was that of 'depressive anxiety', after Melanie Klein, who suggested the infant felt responsible for destroying his mother and believed he had lost her forever. Finally, Bowlby discussed 'persecutory anxiety', also after Melanie Klein, where the young child feels the mother has left him, because she is angry with him.

Bowlby then described his own theory as 'Primary anxiety' theory, defining anxiety as:

a primary response not reducible to other terms and due simply to the rupture of the attachment to his mother. / The child is bound to his mother by a number of instinctual response systems, each of which is primary and which together have high survival value... I wish to distinguish it sharply from states of anxiety dependent on foresight. (Bowlby, 1961a, pp. 253/267)

Bowlby (1960a) emphasized that his theory involved a new and ethologically inspired approach:

The heart of this theory is that the organism is provided with a repertoire of behaviour patterns, which are bred into it like the features of its anatomy and physiology, and which have become characteristic of its species *because of their survival value to the species* [original italics]. (Bowlby, 1960a, p. 95)

But Bowlby now also clearly relied on the careful experiments by comparative psychologists such as Harry Harlow. In discussing fright and an animal's escape from a fearful situation to a secure situation, he referred to the latter as a "haven of safety", a term which he took from Harlow and Zimmermann (1958). Bowlby quoted Harlow and Zimmermann as follows:

In describing their very interesting experiments with rhesus monkeys they write: ‘One function of the real mother, human or sub-human, and presumably of a mother surrogate, is to provide a haven of safety for the infant in times of fear or danger.’ (Bowlby, 1960a, p. 97)

Later in the same paper, Bowlby compared the behavior of the young child, Laura (filmed by Robertson, 1952), who pretended to be asleep when a strange man entered her room, to the behavior of the rhesus infants, who froze in a crouched posture when introduced to a strange situation in the absence of the surrogate mother. That remarkable comparison too was a reference to Harlow and Zimmermann’s paper. Bowlby also discussed the infants’ rushing to the mother (if she was present) as a source of security, describing the response as so strong “it can be adequately depicted only by motion pictures” (Bowlby, 1960a, p. 101). He was no doubt referring to Harlow’s film, *The nature and development of affection* (Harlow & Zimmermann, 1959), a film that has been shown to thousands of introductory psychology classes over the years and received an award for excellence at a European film festival in 1960.



Figure 6. John Bowlby lecturing at the 117th Annual Meeting of the American Psychiatric Association in Chicago, 1961. Picture courtesy of the Wellcome Library, London (AMWL: PP/BOW/L.5).

In three other papers Bowlby (1960b, 1961b, 1961c) of that period discussed maternal separation and the processes of grief and mourning: according to his views separation from the mother-figure would lead to separation anxiety and grief and would set in train processes of mourning. Bowlby described the three stages of protest, despair, and detachment. One of the papers was based on a lecture Bowlby (1961c) read at a meeting of the *American Psychiatric Association* in Chicago in May, 1961 (see Figure 6). There Bowlby once again presented his new ideas to an audience of psychiatrists. He stressed the importance of observation instead of using retrospective evidence, described the analogous course of grief and mourning in children and adults as well as in animals, and finally pointed to the evolutionary basis of the process of mourning. To buttress his claim that “in the light of phylogeny it is likely that the instinctual bonds that tie human young to a mother figure are built on the same general pattern as in other mammalian species” (Bowlby, 1961c, p. 482), Bowlby referred once again to the work of Harlow (Harlow & Zimmermann, 1959). There was no discussion of Harlow's work beyond that, but Bowlby's own description of the stages of protest, despair, and detachment was to greatly influence Harlow's experimenting.

### **Harlow's research in the 1960s: seeking empirical evidence for Bowlby's theoretical claims**

Bowlby's influence on Harlow's work becomes evident after the first two Ciba-conferences. In two studies on mother-infant separation Harlow modeled his experiments with rhesus monkeys on the human separation syndrome described by Bowlby (Stephen Suomi, personal communication, August 27, 2006). In his experiments Harlow either physically (Seay et al., 1962) or totally (not just physically but also visually and audibly) separated (Seay & Harlow, 1965) the infant rhesus monkeys from their mothers for three and two weeks respectively. In both studies, the rhesus infants initially responded with “violent and prolonged protest” and then passed into a stage of “low activity, little or no play and occasional crying”. These stages were similar to the phases of protest and despair described by Bowlby. The third phase of detachment was not found in either study, presumably because of the relatively short period of separation. Overall, Harlow reported considerable similarity in the responses to mother-infant separation in human children and infant monkeys, explicitly referring to Bowlby's (1960b, 1961a) studies on the subject.

### **Bowlby's continuing interest in Harlow's work**

Ten years after their first publications on the mother-child bond (Bowlby, 1958c; Harlow, 1958), Bowlby (1968) published a paper on the effects on behavior of the disruption of an affectional bond. In this paper he stated that “[t]here is now abundant evidence that, not only in birds but in mammals also, young become attached to mother-objects despite not being fed from that source...”, and referred to Harlow's work with rhesus monkeys (Harlow & Harlow, 1965). This statement made clear that there no longer was any empirical support for psychoanalytic and learning theorist explanations for attachment behavior.

In 1969, four years after the fourth and last Ciba-symposium, the first volume of Bowlby's trilogy on *Attachment and Loss* was published. In that volume, Bowlby draws heavily on the results of Harlow's experiments as an empirical confirmation of his ideas.

Throughout this book, Bowlby makes ample use of animal evidence and biological theorizing (e.g., Lorenz, Tinbergen). Among the students of animal behavior Bowlby referred to, Harlow figured prominently. We shall mention but a few examples.

In discussing motor patterns of primate sexual behavior, Bowlby (1969/1982, p. 165) claimed there is clear evidence that they are subject to a sensitive developmental phase and pointed out Harlow's extensive series of experiments in which rhesus infants were raised in differing social environments, “all differing greatly from the environment of evolutionary adaptedness”. Pointing out the deficits in adult heterosexual behavior displayed by the Wisconsin isolate-reared monkeys, Bowlby cited a personal communication in which Harlow wrote he was “now quite convinced that there is no adequate substitute for monkey mothers early in the socialization process” (Harlow in a letter to Bowlby dated October 18, 1965).

A chapter on the nature of attachment behavior contained a reiteration by Bowlby (1969/1982, p. 178) of the four principal theories of the child's tie to the mother that he had disputed in his earlier paper (Bowlby, 1958c). This time he prefaced his own view with the interesting phrase: “Until 1958, which saw the publication of Harlow's first papers and of an early version of the view expressed here, four principal theories regarding the nature and origin of the child's tie were... found”. With that phrase, Bowlby seemed to at least implicitly make two points: first, that he and Harlow simultaneously and independently arrived at similar views, and, second, that Harlow's findings were of fundamental importance for attachment theory and hence for his own thinking.

In a discussion of primate infant and mother roles in their joint relationship, Bowlby (1969/1982, p. 194) referred to the tenacity of primate infants brought up in human homes to cling to their foster parents and added: “Of the cases in which an infant has been brought up on an experimental dummy the best-known reports are those of Harlow and his colleagues (Harlow, 1961; Harlow and Harlow, 1965)”. The next sub-topic (Bowlby, 1969/1982, p. 195) was the infant's ability to discriminate the mother, and Bowlby again cited Harlow and Harlow (1965) pointing out that Harlow believed a rhesus infant learned attachment to a specific mother during the first week or two of life.

In his chapter on the nature and function of attachment behavior, Bowlby connected Lorenz's work on imprinting to Harlow's rhesus monkey work. To support his views on the nature and function of attachment behavior, Bowlby (1969/1982, pp. 213-216) used Harlow's experiments to undermine “the secondary drive type of theory”. He meticulously described Harlow's (Harlow & Zimmermann, 1959; Harlow, 1961) experiments with the cloth and wire mother illustrating “that ‘contact comfort’ led to attachment behaviour whereas food did not” and that “typical attachment behaviour is directed to the non-feeding cloth model whereas no such behaviour is directed towards the feeding wire one”.

In developing a control systems approach to attachment behavior, Bowlby (1969/1982, p. 239) applied Harlow's (Harlow & Harlow, 1965) views on the object and social exploratory behavior of young monkeys to that of human children: just as infant monkeys, human children have an exploratory system that is “antithetic to [their] attachment behaviour”, because it takes them away from their mother.

From these few examples, it becomes clear that in the first volume of his magnum opus *Attachment and Loss*, Bowlby used Harlow's empirical data on rhesus monkeys as uncontested evidence for his own views on the nature and development of the attachment relation which is formed between children and their caregivers in the first year of life. Harlow's findings provided Bowlby with independent empirical evidence, which he could use to argue the superiority of his ideas over and above those of psychoanalysts and learning theorists.

### **Conclusion**

In this contribution, we have taken a closer look at the cross-fertilization of the work of John Bowlby and Harry Harlow. We have demonstrated Harlow-Bowlby ties through correspondence and mutual presence at professional meetings. They wrote dozens of letters and met at least five times between 1958 and 1965. Instances in which Bowlby cited Harlow's work in order to make a point, or as illustrative documentation of a behavior or phenomenon, have been noted. We may conclude that Harlow's scientific influence on Bowlby has been demonstrated beyond reasonable doubt: Harlow's experiments showed in a remarkable way what Bowlby had been theorizing about since his introduction to ethology in the early 1950s. Our findings make abundantly clear that Singer (1975) was completely wrong in asserting that Harlow's findings had no impact on Bowlby's theory whatsoever. A careful analysis shows that Harlow provided an important part of the solid empirical foundation for Bowlby's theoretical construction.

In his turn, Harlow was influenced by Bowlby's new theorizing. We have described how in two studies on separation (Seay et al., 1962; Seay & Harlow, 1965) Harlow modeled his experiments on Bowlby's ideas. Harlow's own assertion that he and his colleagues used one of Bowlby's paper as something of a "reference bible" (see above), his frequent requests in their correspondence for offprints of Bowlby's papers, and his references to Bowlby's ideas make it clear that he regarded Bowlby as one of the major theoreticians. It was Harlow's student Suomi (1995) who acknowledged Bowlby's major influence in three areas of animal research: 1) descriptive studies of the development of attachment and other social relationships in monkeys and apes, 2) experimental and naturalistic studies of social separation in nonhuman primates, and 3) investigations of the long-term consequences of differential early attachments in rhesus monkeys.

The scientific and personal contact between Bowlby and Harlow that started in 1957 lasted through the 1960s and early 1970s until Harlow's retirement in 1974. They kept each other informed about their work and cited each other's work extensively. Although they came from widely diverging backgrounds and differed in many respects they found a common denominator in their interest in the origin of affectional bonds. Together they reached the introductory psychology textbooks and influenced the lives of many children around the world.

**INTERMEZZO.**

**HISTORICAL VIEWS AND CURRENT RESEARCH**



So far, we have argued that from the 1950s John Bowlby was in close personal and scientific contact with Robert Hinde, who introduced him to the finer details of the emerging science of ethology (Chapter 3). The theoretical implications drawn from this new approach for animal behavior eventually led him to “rewrite psychoanalysis in the light of ethological principles” (Dinnage, 1979, p. 325). At the same time, as we have shown, Bowlby’s position was confirmed by Harlow’s early research on separation with rhesus monkeys (Chapter 4). Also, it became clear that Bowlby’s influence on students of animal behavior was immense. Encouraged by Bowlby, Hinde shifted his focus from song-learning in birds to studying mother-infant interactions in rhesus monkeys. Harlow modeled his experiments with rhesus infants on Bowlby’s theoretical ideas and thus sought and found empirical confirmation for Bowlby’s views as to the consequences of separation in human infants. But these reciprocal influences are not just a thing of the past. To this day researchers of attachment theory and animal behavior are profiting from each other’s work and their research is intertwined.

On the basis of the findings presented in the previous chapters, we decided to invite a leading expert in the field to discuss these issues in an in-depth interview as to further understand and clarify the cross-fertilization of ideas. We decided upon Dr. Stephen J. Suomi and conducted an interview with him on September 27, 2006 at the Centre for Child and Family Studies, Leiden University. The verbatim record of this interview was subsequently annotated by Frank van der Horst and edited several times by both him and Dr. Suomi. Questions that came up in the process were dealt with through email correspondence. The result of this extensive process of revision is presented in the next chapter as a running text.

In the general introduction we have addressed the importance of ‘oral history’ for the historical and theoretical research conducted in this thesis. The interview presented here is an illustration of this importance for the history of the cross-fertilization of ethology and attachment theory. First, the chapter nicely illustrates the relevance of oral histories to historical and theoretical research and, secondly, it shows that the interchange between attachment theory and studies of animal behavior bears fruit to this day. Before turning to this extensive illustration of merging attachment research with studies in primates, Suomi’s work shall be briefly introduced.

Dr. Suomi has received international recognition for his research on biobehavioral development in rhesus monkeys and other primate species. From 1968 he was a graduate student with Harry Harlow at the University of Wisconsin-Madison, receiving his PhD in Psychology in 1971. His initial research showed that reversal of the adverse affects of early social isolation, previously thought to be permanent, is possible in rhesus monkeys (e.g., Harlow & Suomi, 1971; Suomi & Harlow, 1972; Suomi, Harlow & McKinney, 1972; Suomi, 1973). Subsequent research led to his election as Fellow in the American Association for the Advancement of Science “for major contributions to the understanding of social factors that influence the psychological development of nonhuman primates”. Since joining the National Institute of Child Health and Human Development (NICHD) he has identified hereditary and experiential factors that influence individual biobehavioral development (e.g., Suomi, 1981, 1987, 1997, 2004), described both behavioral and physiological features of distinctive rhesus monkey phenotypes (e.g., Champoux, Coe, Schanberg, Kuhn, & Suomi, 1989;

Roma, Champoux & Suomi, 2006; Suomi, 1990, 1996), and demonstrated the adaptive significance of these different phenotypes in naturalistic settings. His present research at the Laboratory of Comparative Ethology focuses on three general issues: "first, the role of specific genetic and environmental factors (and their interactions) in shaping individual developmental trajectories; second, the issue of developmental continuity vs. change and the relative stability of individual differences throughout development; and third, the degree to which research findings from monkeys studied in captivity generalize not only to monkeys living in the wild but also to humans living in different cultures" (Suomi, personal communication, September 6, 2006).

## CHAPTER 5.

# **RIGOROUS EXPERIMENTS ON MONKEY LOVE: AN ACCOUNT OF HARRY F. HARLOW'S ROLE IN THE HISTORY OF ATTACHMENT THEORY**

This chapter is based on the verbatim record of an interview with Dr. Suomi conducted on September 27, 2006 at the Centre for Child and Family Studies, Leiden University by Frank van der Horst and René van der Veer, who subsequently edited and annotated the text.

A shortened version of this chapter was published as:

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**Abstract**

On the basis of personal reminiscences an account is given of Harlow's role in the development of attachment theory and key notions of attachment theory are being discussed. Among other things, it is related how Harlow arrived at his famous research with rhesus monkeys and how this made Harlow a highly relevant figure for attachment theorist Bowlby.

**Keywords:** attachment theory, affectional systems, ethology, animal research, Harlow, Hinde, Spitz, Suomi, history of psychology, biography

**Suomi's background and relationship with Harlow**

I grew up in Madison, Wisconsin, where Harry Harlow became famous for his research on surrogate monkey mothers (Harlow, 1958), attracting widespread international public attention when I was in primary school. After secondary school I became an undergraduate at Stanford University, where I began studying psychology. I was initially a pre-medical student, but I took my first psychology course and my first organic chemistry course during the same academic term, and I did very well in the former and not so well in the latter. I decided at that point I was really interested in psychology. It turned out that the very first question on the very first exam in my Introductory Psychology course was about Harlow's isolation studies, and I answered it well because by then I already knew Harlow's work by heart. As my undergraduate studies progressed I was accepted into an honors program in psychology and began doing research in social psychology, and I just absolutely fell in love with it. This probably kept me in school, because I also was getting interested in other things at the time.

For holidays I would usually go back to my parents' house in Madison. My father [Verner E. Suomi] was a long-time faculty member at the University of Wisconsin. He was also a noted scientist in his own right, a very prominent researcher in the field of meteorology who, among other things, had basically created the weather satellite system that we now have today. Prior to the spring break during my junior year at Stanford, he and Harlow ended up on the same airplane and found themselves sitting next to each other – at the time they were mutual acquaintances but not close friends. Sometime during the flight my father told Harlow that he had a son studying psychology at Stanford, which is where Harlow had gone to school himself, and he asked him if there was any information or advice Harlow might want to pass on to his son. So when I returned home for my spring break, there was a message waiting for me saying Harry Harlow wanted to see me. Well, I certainly knew who Harlow was, and I certainly made that appointment!

When I arrived at Harlow's office, he immediately sat me down and asked me what I had been doing at Stanford and what my plans were. I told him that I was very interested in social psychology and had started carrying out research in that area – and that I really wanted to go on to graduate school in that field. But what I did not tell him was that I had already checked out Wisconsin as a potential place to go to graduate school and had rejected the idea for two reasons in particular. One was I did not like the winters in Madison – and since I had discovered by that time that it was not necessary to nearly freeze to death every winter, my desire to return to the American Midwest was about zero. Secondly, I had already checked out the social psychologists in the Wisconsin psychology department and although most were very prominent, they were studying things I was not particularly interested in at the time. So I replied to Harlow: "Yes, I am seriously looking at going to graduate school in the field of social psychology." He reacted by saying: "Well, that is interesting. But if you do that then you will end up with a pretty narrow background. Why don't you come and work with me instead?" That is how I got into the monkey business, because at the time I was not about to turn down his offer!

When I went back to Stanford for my spring term I had one elective opening in my class schedule, and it ultimately came down to a choice between two courses. One

possibility was to take a course in physiological psychology from Charles Hamilton, who at that time was carrying out cortical lesion studies with monkeys. I knew that Harlow had conducted some pioneering research involving cortical lesions in monkeys, so it seemed like that course might be relevant for me. The other possibility was to take an advanced seminar from the noted developmental psychologist Eleanor Maccoby. I had never taken a developmental psychology course before, but the title of her seminar – Attachment and Dependency – sounded intriguing to me. Many years later Maccoby told me that she had somehow obtained a proof copy of John Bowlby's first book on attachment (Bowlby, 1969/1982), and that is what she essentially based the seminar upon. So it turned out that my initial exposure to Bowlby and attachment theory occurred even before his first volume had been published – and before I started working with Harlow.

When I returned to Wisconsin to begin graduate school the following year [in February, 1968] I initially found Harlow to be very different from the person with whom I had met the previous spring. I subsequently learned that he had just found out that his wife Margaret had terminal cancer and that he had taken the news very badly – he had become clinically depressed. At any rate, I had only been in the lab for maybe two or three weeks when Harlow suddenly pulled me into his office one afternoon and told me: “Go find somewhere else to study. I am about to go to the Mayo Clinic for extended treatment. I do not know how long I am going to be away from here, and you might want to re-consider some of those other places you have applied to.” I very quickly made my decision: No, I do not want to do that, I will stay around and see what happens. In the meantime a brilliant, active, enthusiastic, and newly tenured Associate Professor named Jim [Gene P.] Sackett, took me under his wings and in the ensuing 3-4 months taught me just about everything I know about experimental design and the observation of behavior. Sackett easily convinced me to do some research with him, and after we finished that experiment I conducted a follow-up study using the same apparatus. I wrote up the results, and when Harlow finally came back to the lab and read the manuscript, he told me: “Congratulations, you have just done your Master's thesis. Now let's go study something serious.” That paper was my first scientific publication, with both Harlow and Sackett as co-authors (Suomi, Sackett, & Harlow 1970).

When I subsequently met with Harlow to discuss possible topics for my dissertation research, he told me: “There are two topics I am especially interested in these days. One involves the study of cognitive development, using tests like cross-string tasks to assess some advanced cognitive capabilities in young monkeys,” but at the time I was not really interested in that. “The second involves developing a monkey model of depression.”

After Harlow had been treated for his depression, he decided that he wanted to try to model it in monkeys, and he spent some time consulting with his good friend Bill [William] Lewis, who at that time was Chair of the Department of Psychiatry at Wisconsin, regarding the plausibility of developing a monkey model. Lewis was enthusiastic about that prospect, and Harlow proposed that I start the ball rolling by surveying what previous efforts to model human psychopathology in monkeys had yielded. He added that “there are some things in the literature that might help”. It turned out that Harlow and his students had carried out some monkey experiments involving maternal separation in the previous decade, basing

their studies on reports of the depressive consequences of maternal separation for human infants. He told me: "There are two people that you need to read: one of them is René Spitz and the other is John Bowlby, whom I know personally." So first of all he gave me all of his copies of Bowlby's reprints, which were not only autographed by Bowlby, but more interestingly, Harlow had written notes in the margins of the reprints. He later talked to me extensively about his relationship with Bowlby. So I knew about Bowlby and attachment theory before I met Harlow, but more importantly Harlow was the one who encouraged me to read Bowlby thoroughly and who started telling me about his work.

### **Harlow and Bowlby**

Harlow was introduced to Bowlby by the British ethologist Robert Hinde, who of course knew Bowlby well. What is interesting is that at the time that Bowlby was starting to develop his theory of attachment, Hinde was shifting his area of interest from studies of song-learning in birds to studies of mother-infant interactions in rhesus monkeys. The suggestion is that one of the reasons Hinde changed his area of interest was because he had visited Harlow some years earlier. So Harlow influenced Hinde, who then got Bowlby's attention, and then Hinde introduced Harlow to Bowlby – and they hit it off right away. They subsequently corresponded extensively, and Bowlby invited Harlow to several conferences at the Ciba-foundation that Bowlby, Hinde, and Harlow all attended (Foss, 1961, 1963, 1965, 1969).

I think the best indication of the importance of these Ciba-conferences for Harlow's work is that Harlow insisted that Bowlby invite some of his best students and postdocs to the second and subsequent conferences. Harlow wanted his students to absorb both what was happening at the human level and where these people were coming from in terms of not only the empirical work they were carrying out but also the theoretical foundation upon which they were basing their studies. I am sure that Harlow had recognized long before his interactions with Bowlby that one could use monkeys to study behavioral phenomena that would be relevant for human development but that could not be done with rats and was not feasible, for ethical and/or practical reasons, to carry out with human subjects.

You could not carry out those studies with rats because rats do not have the all the advanced cognitive capabilities that the primate cortex makes possible. If all you are studying is conditioning, you do not need an organism with a well-developed cortex. However, if you limit yourself to studying conditioning processes, you are basically ignoring all the advanced cognitive capabilities that emerge during development that the primate brain provides. So Harlow thought that he could study aspects of human cognitive development and social behavior using monkeys where it was possible to rigorously control environments and vary the conditions and the stimulus presentation – and he could test those monkeys every day. It is all but impossible to do that with human subjects, especially children, because most parents and teachers are appropriately unwilling to have an experimenter show up in their house or their classroom every day. So Harlow realized that it is possible to collect much more complete information on individual monkeys than is typically the case with human subjects.

Bowlby visited Harlow's lab at least once, and that is how their relationship became well-established. If you look at Bowlby's (1958c) first monograph on attachment, you will find

in one of the footnotes a reference to Harlow's not yet published surrogate mother studies. Harlow was about to present his initial findings from that research publicly for the first time in his presidential address to the *American Psychological Association* in the summer of 1958. That address, which Harlow entitled "The nature of love," turned out to be an absolutely remarkable presentation, which became famous (at least among psychologists) not only for its scientific content but also for its style of presentation – I have numerous older colleagues who were in the audience when Harlow delivered that address who still remember the occasion. At any rate, Harlow apparently sent a copy of a draft of the talk to Bowlby before he published it in the *American Psychologist* (Harlow, 1958). Bowlby included a reference to that paper as a footnote in his original 1958 monograph on attachment. Of course when Harlow gave me his copy of that paper, he had circled the footnote and said: "Pay attention to this!" So right from the beginning of attachment theory there was a biological component, and it was heavily influenced not only by Bowlby's previous interest in ethology, but also by his concurrent interest in the mother-infant studies that Harlow was modeling with his surrogate research and that Hinde was beginning to study in more naturalistic circumstances.

A few years later, shortly after I got my degree, Harlow introduced me to Bowlby at a meeting in New York. At that meeting, which involved a relatively small number of very prominent ethologists, psychiatrists, and comparative and developmental psychologists (including Bowlby, Hinde, and Mary Ainsworth, among others) Harlow insisted that I present the latest findings from the lab, saying "Steve, you are going to give this talk, not me." The conference began with that presentation (Suomi, 1976), and Bowlby gave the talk that followed (Bowlby, 1976) – and that is where we got to know one another. Shortly thereafter, Bowlby invited me to come to England and visit him at the Tavistock. That is how my own relationship with Bowlby got started – but Harlow's interactions with Bowlby predated that conference by almost two decades. Indeed, from the very beginning of his research with surrogates, Harlow was acutely aware of Bowlby and appreciated the importance of what he was trying to do with his ideas about attachment.

Regarding their personal relationship, I would say that they respected one another enormously. Harlow was a rebel in his own field who delighted in destroying theories as much as he could, and his initial experiment with surrogate monkey mothers all but demolished two of the most prominent contemporary theories at the same time. First of all, it knocked the socks off of the classic psychoanalytic view of how infants establish their initial relationships with caregivers, namely through oral gratification associated with nursing. It also clearly contradicted the prevailing psychological theory of primary and secondary drive reduction, which had at its heart the idea that an infant's desire to be with its caregivers stems from the reduction of the primary drive of hunger through feeding, i.e., this desire for the caregiver represents a secondary drive. Thus, both the prevailing psychoanalytic and behavioral views at the time held that relationships between parents and infants developed initially as a consequence of nursing. And Harlow's surrogate research, in which he demonstrated convincingly that rhesus monkey infants overwhelmingly preferred to be with cloth-covered surrogates that provided no source of milk to wire-covered surrogates that provided them with all the milk they could ever drink, showed that neither of those views

could be correct. Bowlby of course spent much of his entire career fighting the classic orthodox psychoanalytic view. So I think they both saw that rebellious spirit in one another and had plenty to talk about regarding theories and data. And they also listened to each other's advice.

As one example of this, Harlow told me about a visit Bowlby once made to his lab after Harlow had finished his initial surrogate studies and was next trying to design a surrogate that would physically reject an infant, presumably to block the infant's development of an attachment to the surrogate. At the time of Bowlby's visit Harlow had already pilot-tested a variety of different models of "rejecting" surrogates. One model shook the infant off, another had a little catapult that would throw the infant off, a third surrogate that had little spikes that would come out of its body to discourage physical contact by the infant – and none of them worked. That is, every time the infant was physically rejected by each surrogate mother, as soon as the surrogate went back to its "normal" condition, the infant would immediately return to the surrogate. Harlow discussed with Bowlby his problems in trying to get this research going, expressing considerable frustration because he was trying to produce psychopathology so he could study it rigorously, scientifically, and systematically – and the infant monkeys were clearly not cooperating! According to Harlow's account to me, Bowlby listened patiently to his complaints, and then he said: "Well Harry, unfortunately not every experiment works, not even yours – and by the way, can I go see your lab?", so Harlow had one of his students give Bowlby a tour of the lab.

At that time, and actually unfortunately for many years thereafter in most other primate facilities, the standard way of housing monkeys was to put them in cages by themselves and keep them socially isolated where they could see and hear other monkeys, but not physically interact with them. This was done largely for veterinary purposes. The veterinarians were afraid of disease being spread, and they thought they could prevent that by physically isolating the monkeys from one another – at the time their biggest concern was simply to keep the monkeys alive. Bowlby saw all of these monkeys housed in single cages exhibiting weird stereotypic behaviors, sucking their fingers and toes, and rocking back and forth, which is how rhesus monkeys reared with a lack of physical contact opportunities routinely behave. After his tour Bowlby came back to see Harlow in his office and told him: "Harry, I do not know what your problem is. I just toured your lab and you have more crazy monkeys here than probably exist in any other place on the face of the earth! You do not have to produce psychopathology – you already have it!" Harlow later would say that this just goes to show that one can not have a psychosis unless there is a psychiatrist around to diagnose it. Many years later, when I related that story first time I gave a talk at Cambridge, Robert Hinde came up to me afterward and said: "You have the story right, but you have the wrong person. I am the one who told Harry that." But I have a feeling they both did.

At some point Harlow and Bowlby stopped interacting. I think one of the main reasons was that Harlow retired in 1974, around the time I began corresponding with Bowlby. Maybe Bowlby thought I was the vehicle through which that tradition would keep going – and when Harlow retired, he really retired. He remarried his first wife, moved out of Madison, and went to southern Arizona with her. He had Parkinson's disease at the time, and he later had a stroke and passed away shortly thereafter [in 1981]. The last time I saw

him was in late 1980, and I could tell by then that his memory was starting to fade. So it was not that Harlow and Bowlby no longer liked each other but instead that Harlow basically took himself out of the picture.

### **Harlow's work and the influence of Bowlby and Spitz**

I do not think it was Harlow's original intention to refute psychoanalysis. He initially designed his surrogate studies probably more to refute classic drive reduction theory, which was absolutely the prominent behaviorist theory at the time, championed by people like Clarke Hull and Herbert Spence. This theory held that primary drives would lead to secondary drives through associations with stimuli that produced the primary drives. So if a mother reduces a child's hunger she becomes a secondary reinforcement object as a result. Harlow hated that theory. His second wife [Margaret] had come out of Spence's lab, and I think that among other things he wanted to show that her mentor was wrong. But Hull was also a major figure in the Department of Psychology at Wisconsin when Harlow first showed up back in 1930. In the years that followed Harlow was discovering all sorts of things that his monkeys could do, such as learning based on curiosity without reinforcement and observational learning that they were not supposed to be able to do according to the basic principles of drive reduction theory. These activities did not require either traditional drive reduction or any other kind of reinforcement – the monkeys would just do these things out of an inherent curiosity.

A second series of insights occurred when Harlow started breeding monkeys [in the early 1950s]. He was especially interested in studying learning phenomena at this time, and one of the things he wanted to do was to understand the development of learning capabilities: how do monkeys learn to learn, how do their cognitive abilities change as they get older? In order to answer those and other questions he needed to test infants, and he wanted infants that were not being cared for by their mothers, because if they were living with their mothers he could not test those infants individually without major disruption. So he separated them from their mothers at birth and developed a neonatal nursery – and he started raising the infants in the nursery. The infants had diapers on the floors of their cages, and Harlow noticed, as had Gertrude van Wagenen (1950)<sup>15</sup> several years before, that when the infants had their diapers taken away to be cleaned, they got really upset and they kept clinging very strongly to the diapers.<sup>16</sup> Harlow thought about this for a while and discussed it extensively with his students. At that time, Bill [William A.] Mason was a postdoc in Harlow's lab, and he was very interested in many of these same learning issues himself – he had carried out some of the original studies investigating learning in these infants as they were growing up. Mason, like Harlow, recognized that these infants spent a lot of time clinging to

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<sup>15</sup> Van Wagenen (1950, p. 25) noted that the “clinging reaction, undoubtedly initiated by the grasp reflex in the newborn, is unrelated to it physiologically – rather it is an expression of infantile emotional dependence”.

<sup>16</sup> Harlow (1958, p. 675) used “folded gauze diapers to cover the hardware-cloth floors of the cages. The infants clung to these pads and engaged in violent temper tantrums when the pads were removed and replaced for sanitary reasons”.

the diapers and he said: "Let's formalize this, let's make something that is more tangible, that they can hang on to, something more permanent." Mason was interested in creating the surrogate as a way of providing that tactile stimulation directly affected the infants. Harlow had the same interest. They had gotten to the point where they had decided to pit surrogates with different types of surfaces against one another: the same wire mesh that was on the floor and sides of the cages versus the cloth in the diapers that the infants seemed to love. The infants spent considerable time hanging onto the cloth, but they did not spend any time hanging onto the wire. So they then said: "Let's make a couple of dummies, and we will put one with food but no cloth and one with cloth but no food in each infant's cage and see what happens."

Harlow's recollection of the next step is that while returning from a speaking engagement, he was flying over Detroit when all of a sudden there appeared a surrogate with a face sitting in the seat next to him. He went back to the lab the next morning with the inspiration: "Let's put a head with a face on the dummy." So I think that although both Mason and Harlow had the idea using the surrogates to pit food versus tactile contact, it was Harlow who wanted to put a head with a face on the body of the surrogate. Mason did not want to do that – he was very adamant about not putting a head on the surrogate, let alone one with a face, because he did not want to get into the area of affection or anything like that. Instead, he just wanted something that would functionally serve as a vehicle for providing a test of food versus tactile stimulation. Indeed, Mason argued that adding a head with a face would muddy up the situation and make the research sloppy, so when Harlow insisted on adding the head, Mason backed out of the surrogate project. Harlow eventually found a graduate student, Bob [Robert R.] Zimmermann, who agreed to take on the project, and rest is history.

I really think that the insight of adding a head with a face to the surrogate is what suddenly opened up a whole new area of research, allowing Harlow to take something that was initially a test of basic theoretical issues into a whole new research arena that presumably had real relevance for real mothers and real kids. At the time when Harlow met Bowlby for the first time, this was what Bowlby was dealing with in his own mind, and although Harlow did not call it attachment theory *per se*, it certainly did not hurt to have that kind of empirical foundation showing the strength of the ties that Bowlby was talking about and was starting to develop from his human work. I mean, Harlow was sufficiently creative that he could come up with that insight *de novo* and immediately recognized what he might be able to do with this research, but I think even he was surprised by how the results of his initial surrogate research took off.

I think it may have been Bowlby who also pointed out to Harlow that those infant monkeys being raised in the nursery were in fact being isolated socially – and in this way may have well provided the impetus to begin formal study of the social and emotional consequences of being reared in social isolation. Harlow's lab was already carrying out studies of the effects of social isolation on the development of cognitive capabilities in monkeys (Mason, Blazek & Harlow, 1956, was the first of a series of publications on that topic), but the idea to focus on the social and emotional consequences came later, perhaps initially on Hinde's suggestion but almost certainly reinforced by Bowlby. Harlow himself both in public and privately to me said: "It is Bowlby who really got me into this business."

Harlow and his students had actually been studying monkeys reared in functional isolation for some time before that, because it turns out that simply by rearing animals from birth in a nursery and not putting them in with other monkeys, they were doing *de facto* isolation. What they did subsequently was make the isolation more extreme by putting the infants into tin boxes where they could not even see or hear any other social stimuli, because the previous infants otherwise were growing up in rooms where they could see and hear the other monkeys in the room, even though they could not physically contact them. I am certain that it was Bowlby's influence that taught Harlow to pay attention to things other than the infants' learning capabilities, because that is all that they were studying prior to the time that Harlow began interacting with Bowlby.

Bowlby may have pointed out to Harlow: "What you see in these monkeys is what we see in human children raised in institutions," as was reported in studies by Spitz (1945, 1946). There followed the first formal studies of the social effects of isolation, in which Harlow and his students deliberately put newborn infant monkeys into these isolation units and then kept them in the units for varying periods of time (0-3 months, 0-6 months, 6-12 months, 0-12 months); those studies provided the basis for several PhD dissertations. From Guy Rowland's (1964) dissertation, which looked at six-month-isolates versus 1-year-isolates versus monkeys that were growing up in single cages where they could at least see and hear other monkeys, it became pretty clear that the isolation-reared monkeys were developing grossly abnormal patterns of behavior. When these monkeys were subsequently placed in a playroom with other monkeys of the same age, they were just completely blown away in terms of their total lack of emotional regulation and any sort of normal social repertoires and the appearance of extremely abnormal self-directed behaviors that mother-reared monkeys, and even most single-cage-reared monkeys, simply did not show.

All I can say about the suggestion that Harlow modeled his monkey experiments on the human work done by Spitz is that Harlow once told me: "If you really want to get into this depression business, well, start with Spitz and Bowlby." So I do not know for certain if his initial isolation studies were done as a consequence of reading Spitz – indeed, I doubt that was the case because in the initial isolation studies, the clear motivation was to study learning in a "pure" environment uncontaminated by other social experiences and things like that. At that time, Harlow and his students were convinced that they were going to study these learning process "right", that is in settings where mothers could not be teaching their kids anything since the infants were being kept by themselves and where it was possible to control their environment to the extent that only the experimenters would be presenting the infants with the stimuli that they would be going to remember or forget. Only later, after Bowlby (and most likely Hinde as well) pointed out to Harlow that these monkeys had some real social and emotional problems, did Harlow begin studying those phenomena systematically – and when Harlow went after a problem first thing he usually did was get one of his students to do a literature review. Did he know about Spitz's work before then? He certainly knew about those reports by the time he started carrying out those formal studies of the social and emotional consequences of prolonged social isolation.

With respect to the study of the effects of short-term maternal separations, phenomena that in children had clearly been a long-term topic of interest for Bowlby, Harlow

was either the first or one of the first to investigate these phenomena systematically in monkeys. I believe Gordon Jensen in Colorado actually beat him to the first publication on this topic by two weeks with a much more limited study (Jensen & Tolman, 1962), but Harlow was certainly one of the first to study mother-infant separation in monkeys, that is taking away an infant from its mother for a certain amount of time after an attachment bond has clearly been established and then putting it back with the mother.<sup>17</sup> Two years later Hinde did essentially the same thing in a slightly different setting, and indeed maternal separation studies are still being carried out today, but if one goes back to the very first published studies carried out in Harlow's lab (Seay et al., 1962; Seay & Harlow, 1965), in the Introduction and in the Discussion sections of those papers there is nothing but Bowlby. Those monkey studies were modeled exactly on Bowlby's published accounts of the effects of maternal separation on children, including the use of exactly the same terms – "protest, despair, and detachment" – that Bowlby had employed in describing the reactions of children following separation from and reunion with their mothers. So the monkey separation paradigms were a direct consequence of the Bowlby and Robertson (Bowlby, Robertson & Rosenbluth, 1952; Robertson, 1953) hospitalization studies, and they are still being employed as experimental manipulations today, forty-five years later. The questions of what does separation from an attachment object do to the physiology, to the biochemical systems, to gene expression, in an infant remain relevant today, largely because that manipulation is a powerful enough stimulus to elicit significant changes in those and other biological systems. Bowlby was the first, at least from Harlow's standpoint, to recognize this fact. So absolutely yes, Harlow modeled his monkey separation research on the human clinical reports that Bowlby and his colleagues had put together.

### **Animal psychology**

You could say that for the study of attachment-related phenomena it was in a way sheer luck that Harlow was working with rhesus monkeys. In the 1930s he started off like most primatologists at the time: you could either watch monkeys at a zoo or you could have an importer bring them in as pets in order to study them. The primate researchers back then did not know much about how to take care of primates, so most of their monkeys did not survive very long in laboratory settings. Now, if you end up purchasing expensive animals and they die within the first two weeks, they are not going to do you much good. If you look at Harlow's published studies over about the first 10 years of his career, they focus on topics such as object learning in orangutans, gibbons, guenons, langurs, rhesus, and capuchin monkeys, that is, reports of multiple species being tested under different circumstances. If you look more carefully, these other species start dropping out of citations and pretty soon it is only rhesus and capuchin monkeys that are being reported upon. These were the two species that seemed to be able to survive life in those primitive laboratories where they could routinely be maintained for months if not years.

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<sup>17</sup> Earlier Hersher, Moore and Richmond (1958) studied separation of goat mothers from their newborns and concluded that separated mothers nursed their own kids less and other kids more than nonseparated mothers.

Ultimately, the most interesting part of that history from my standpoint is that in the late 1930s and 1940s Harlow developed a technique for testing the learning capabilities of monkeys using something called the Wisconsin General Test Apparatus (WGTA). This is a device that once you have trained the monkeys to get used to the apparatus, they can be sitting in a cage adjacent to the WGTA, and you as the experimenter have a stimulus tray with two or three shallow wells bored into it hidden from the view of the subject by a movable barrier. On each test trial you put a treat in one of the wells, and you cover it with one type of stimulus and cover the empty well or wells with a different stimulus object or objects, and then you raise the barrier and present the monkey with the baited stimulus tray. The subject has to push aside what it thinks is the correct stimulus object and either obtain a reward or not. So this is a very systematic form of testing that one can carry out over hundreds of trials for each subject over multiple sessions, but quite frankly it is boring as hell. Ever since I was a graduate student I have been much more interested in social aspects of primate behavior. When I began training in Harlow's lab, virtually everybody had to do WGTA-testing, but somehow I managed to go all the way through graduate school without ever running a single monkey in a WGTA even once. The testing is clearly boring for the experimenter and takes time up for the monkeys as well. At any rate, Harlow soon discovered that whereas rhesus monkeys would sit still and do this hour after hour, capuchin monkeys, even though they were clever, would not settle down and go through these long-term rigors, and so Harlow eventually concluded: "My choice is between a factory worker and an artist and I am going to choose the factory worker."

Harlow was influenced by the work of the American comparative psychologist Robert Yerkes and his European colleague Wolfgang Köhler. Virtually all the early primatologists knew each other back then and if they did not know each other personally, they were well aware of one another's work. As a graduate student I was shown an old movie that Köhler and Yerkes made of chimps stacking boxes on top of each other to be able to reach a reward. When Harlow first saw that movie [probably back in the 1930s] he said: "If chimps can do it, then why can't capuchins?" So he tried that and eventually made his own movie showing one of his capuchin monkeys stacking boxes and climbing poles to obtain out-of-reach bananas. Harlow absolutely knew about this work involving tool-using by chimps, and he was interested also right from the beginning of his career in studying the complex cognitive capabilities of primates, again because of this notion that monkeys can master complex tasks that rats can not, and can utilize abstract learning processes rather than simple reinforcement chains.

Harlow's interest in characterizing abstract learning processes in monkeys culminated in his discovery of learning sets (Harlow, 1949) and that ground-breaking finding probably is what got him elected into the National Academy of Sciences in 1951. This was the finding that if you give monkeys the same discrimination learning task for six trials, initially they get better with each trial and finally by the sixth trial they usually have solved that particular task. After a few hundred different 6-trial tasks, they can solve each new task perfectly on the second trial, because if they make the right choice the first time they just stick with that choice and if they make the wrong choice on the first trial, they shift and pick

the other stimulus consistently, and therefore they will always solve the problem – and this is viewed as evidence of higher learning, of insightful behavior.

The only sabbatical Harlow ever took was to go to Columbia in 1940, where in one of his lectures the famous German neurologist Kurt Goldstein<sup>18</sup> stated forcefully that humans are the only ones capable of solving abstract problems. When Harlow returned to Wisconsin he went back to his lab and said: “I will get rhesus monkeys to do this.” And he did get the rhesus monkeys to do it. So he later claimed that he was probably the only person who cared about this finding and he was quite sure that Goldstein did not care anything about monkeys – but Harlow sure did. In a way he was involved in the debate between Wolfgang Köhler and Edward Thorndike regarding insightful versus incremental learning. Once he started working with primates, he said: “I should not waste my time studying the old classic conditioning theories, let’s get at this insight business.” He had what for most scientists would constitute an entire career studying what we would today call cognitive processes or cognitive development long before he ever began looking at the social, affectional, and emotional capabilities of monkeys – and it was his studies with surrogate mothers that changed all of that.

### **Harlow’s influence on Bowlby, Ainsworth, and attachment theory**

I think at the very least, Harlow provided Bowlby with the empirical backbone for the theoretical foundation of the biological contribution to attachment. He provided evidence that was supportive of a biological basis for attachment, and if that is all he did, that would have been quite enough. I am pretty sure that Harlow’s work *per se* did not really influence Mary Ainsworth’s characterization of different attachment styles – I think that her ideas about that were well-developed without any involvement with biology. On the other hand, the notion of a secure base was very clearly supported by Harlow’s surrogate findings, especially as depicted in a movie that Harlow made that was eventually shown on national television in the US. I have often said that the finding most people remember from the original surrogate studies was the difference between the cloth-reared and the wire-reared surrogates in terms of the amount of time infants spent in contact with each surrogate type. I think the much more dramatic example of secure-base behavior came when Harlow put these monkeys into a playroom filled with toys and other interesting devices, as depicted in that movie. When an infant was in the playroom with a cloth surrogate present, it typically would initially hang on to the surrogate, clinging to it like crazy, and then after a few seconds the infant would climb off the surrogate, move a short distance away from the surrogate, and then run back to the surrogate for a quick touch, after which it would then leave the surrogate again to explore a little bit more, and then run back to the surrogate, etc.

During some of the test sessions an unfamiliar object would be placed inside the playroom in the presence of the infant – the object that was used in the above-mentioned movie was a small toy bear that mechanically played a drum. This particular stimulus initially terrified the infant – it immediately ran back to the surrogate and clung to it for dear life. But

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<sup>18</sup> Goldstein had done research on ‘concrete’ and ‘abstract’ learning in brain-damaged soldiers after World War I.

after a while, the infant left the surrogate and went over to the toy bear and began to manipulate and then play with it. Indeed, some infants in this situation actually began ripping the toy bear apart after their initial exposure to it. But the manner in which these monkeys initially sought refuge and security by holding on to the cloth surrogate in this novel situation and then used the surrogate as a secure base from which to go out and to explore and even while exploring frequently look back at the surrogate was striking. And the reactions of infants when they were placed in the playroom in the presence of a wire surrogate instead of the cloth surrogate was even more dramatic – most infants would not try to contact the wire surrogate or engage in any kind of exploratory behavior. Instead they would typically run to the corner of the playroom and roll up into a ball, screaming all the while, and then remain there for the rest of the test session. I can not imagine that Bowlby would not have been greatly impressed by the infants' vastly different reactions in the playroom depending on the type of surrogate that was present at the time. I am sure that the behavior of those infant monkeys in the playroom solidified his notion of a secure base, of the attachment-like role these surrogates were really providing. So Bowlby may well have had the concept of a secure base before Harlow carried out his surrogate studies, but those studies provided compelling empirical support that was biological in nature, indeed that was coming from another species. It is hard to imagine that Bowlby would not have either felt very satisfied with Harlow's findings or even become inspired to say: "Well, let's put a little more emphasis on this secure-base phenomenon."

### **Harlow and Bowlby as persons**

It might seem at first glance that Harlow and Bowlby would have very different personalities: Bowlby as a typical upper-middle class Englishman with a stiff upper lip and Harry Harlow as having a much more outgoing personality. Bowlby may have been formal and stiff-upper-lipped in public, but in private he apparently was more engaging. In my interactions with him, which were universally positive and indeed, extremely memorable to me, we would typically start talking about various topics and freely exchange ideas and insights. He often would get terribly excited about some particular point, and any reticence or pretence would quickly disappear under the circumstances. He was also very self-effacing and humble in person. Mario Reda, an Italian cognitive therapist who simply revered Bowlby, once told me that his fondest memory of Bowlby was him saying: "I am just a simple man with simple ideas and I do not have any big notions, I just want to pursue my interests."

Harlow, on the other hand, grew up in a small town in the middle of Iowa, and when he was growing up he was a very shy person, who nevertheless was very smart, quick on his feet, and interested in all sorts of things. He was determined to wear the latest fashion, he was an above-average tennis player (one of his brothers played tennis professionally), and he was an avid and expert bridge player. Harlow was also basically a frustrated English major, which may be one reason why poetry appeared in some of his papers. He grew up with a speech impediment, which initially made public speaking very difficult for him, but when he went to Wisconsin and began teaching introductory psychology to three hundred students at a time three days a week – well, that experience quickly took care of any kind of fear of public speaking, and he even got over his speech impediment. In fact, over the years

he became one of the best and most sought-after public speakers of his time. His scientific presentations were just remarkable, indeed often spellbinding. Harlow had a real appreciation of the power of humor, and he knew how to use it. In public, he could be very critical of contemporaries, but if you could get him in a room by himself he would become very humble and self-effacing – and in that way not all that different from Bowlby. I mean, the public appearance is one thing, but if you get either of these guys in a room without anyone else around...

Harlow could put things rather bluntly and he prided himself on that. He liked to get attention and that was one way to do it – and he loved controversy and did not shy away from it. He expressed ideas in terms other scientists would be afraid to use, would be wary of, or be too careful to want to try. So despite his original shy personality, he often turned to shocking people in his public pronouncements. He discovered that he liked being on stage, and he found out that if you say things that are controversial, you will get asked to be on stage more often – and if you can present your work in ways that focus more on human relationships than its basic theoretical foundations, you get invited to more places.

### **Freud and psychoanalysis**

Let me put into perspective the fact that Bowlby was a psychoanalyst who never really rejected many of Freud's ideas, whereas Harlow was not and hence looked at Freud in a somewhat different light. First of all, I would not say that Harlow knew all aspects of psychoanalysis thoroughly, but he certainly knew about the basic ideas of Freud. I inherited most of Harlow's personal library, and I still have some of Freud's original volumes that Harlow had obtained over the years – and I must say that the extensive notes he wrote in the margins of many pages of these books are just really interesting. At any rate, he was well aware of many aspects of psychoanalytic theory and he knew specifically of the writings of Anna Freud and *Bulldogs Bank's* children<sup>19</sup> during World War II. In fact, I believe her observations probably provided the inspiration for the peer-only rearing procedures he developed in the early 1960s. If nothing else, he was aware of what he called the “cupboard theory” of the bond between the infant and its mother, in which the infant's bond was thought to be derived the feeding process and the oral gratification provided by the maternal breast.

One of my favorite papers that Harlow (1964) ever wrote was based on an address he gave to the *American Psychoanalytic Society*. In that paper he essentially argued that Freud was right, but for all the wrong reasons. What Harlow pointed out was that at that time probably the most solid empirical evidence in support of Freud's observations and some basic psychoanalytic principles that were the foundation of his theory (psychosexual stages, the notion of regression under stress, the notion of fixation in various points in development, etc.) actually came from Harlow's own monkey research, because he could demonstrate every single one of those phenomena in crystal-clear fashion with his monkeys. For

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<sup>19</sup> After World War II Anna Freud studied children who survived concentration camps at an orphanage called *Bulldogs Bank home*. Based on these observations Freud published a series of studies (Freud, 1973) on the impact of stress on children and the ability to find substitute affections among peers.

example, with respect to the notion of psychosexual stages, Harlow pointed out that initially an infant spends great deal of time at its mother's breast, both when it is nursing and when it is not. Interestingly, virtually all of the monkey infants who were reared without mothers began sucking their thumbs and toes in their initial weeks of life. In the anal stage these same monkeys would take their feces and smear them all over their diapers and even themselves, whereas monkey mothers will not let infants soil themselves. Harlow provided comparable monkey examples for each of Freud's other three psychosexual stages in this paper. To paraphrase his concluding arguments: "You have to appreciate Freud's gifted and very inspired observations. They are basically correct, at least for monkeys anyway. So, where does this leave you? This means the monkeys either have little *ids*, *egos* and *superegos* floating around in their tiny brains or there must be another explanation for these phenomena" – and that is where he ended the paper. The original address was before a psychoanalytic audience, so you can imagine what kind of impact that argument must have had. Recall my earlier point about his interest in being provocative... my guess it was probably the first time those psychoanalysts had ever heard anything like that before!

Actually, in retrospect you can argue for either one of the alternative conclusions that Harlow put forward in that paper. Now that we know more about rhesus monkeys and the complexity of their social behaviors, it is clear that they have specific behavioral predispositions and very strong tendencies to react to certain stimuli in quite specific ways that are almost certainly the product of evolution – there is your *id* component. In the wild these monkeys grow up in complex social groups where in order to remain in the group they have learned to adhere to strict social rules, and ultimately must be able to internalize these rules if they are going to survive in the group – there is your *superego* component. Finally, they are smart and have good memories, and most are capable of making judgments that seem to reflect complex decision-making processes – there is your *ego* component. So maybe they actually do have little *ids*, *egos* and *superegos* in one form or another, or maybe there is another explanation – neither of these has to be mutually exclusive of the other.

At any rate, Harlow was basically an empiricist, and although he was well-schooled and familiar with all the classic theories in the field, he could find faults in all of them – and he delighted in finding and demonstrating their shortcomings. That is one reason I think he was so proud of his surrogate work, because in one fell swoop he had basically taken out two of the biggest theories of his time. But being an empiricist he was very eager and willing to look at and examine data, not only his own data but also findings from other studies and fields of investigation – and that is where Bowlby came in. So Harlow did not care whether Bowlby was a psychoanalyst or a behaviorist or anything else. Bowlby was studying things that were just damn interesting to Harlow, and my guess is they did not get into deep discussions of theory when they were together. Instead, they probably talked a great deal about what Bowlby could see in his kids and what Harlow could do with his monkeys. In the end Harlow was keenly interested in Bowlby's studies and his ideas about these studies.

In one of our interactions Bowlby said just out of the blue: "You know Steve, the one thing in my entire career that I regret more than anything else is the fact that I have had to spend so much time dealing with my colleagues who did not believe in what I was trying to do and trying to convince them that I was not crazy and that my approach was indeed

legitimate. I spent so much time doing this, and it really sapped my energy.” And that did say a lot. Of course he was talking about his psychoanalytic colleagues, particularly those who had trained him and who worked in his institute. I have heard that some of those theoretical battles were legendary. Bowlby could not understand why his psychoanalytic colleagues did not think it was legitimate to actually observe behavior and see what is going on outside of the psyche – and how that might help one infer what might be going on inside the psyche. In point of fact, although Bowlby was a psychoanalyst by training, he was really an ethologist at heart. He had a love for animals and was always interested in their behavior – and that most likely influenced a lot of his thinking.

Bowlby did not conduct much empirical research himself, possibly because it was not part of his formal medical or psychoanalytic training. I doubt that the teaching of advanced experimental design or data collection and statistical analytic techniques was a high priority in the schools of medicine and psychoanalysis during Bowlby's student days, a situation that remains largely the same today. I mean, most MD's who do research do research in spite of their MD – empirical research methodology is not usually part of their normal training, except in MD-PhD programs. I do not want to characterize the whole field, but take a typical class of MD's coming out a typical medical school, and maybe two or three percent will go on to become basic researchers down the line, but most of them go on to do what they were trained to do. They may turn out to be extraordinarily skilled and competent in diagnosis and treatment and otherwise make major contributions to society, but unless they have either a research background before they go to medical school or a long and abiding interest in a particular research topic, or they encounter an exceptional mentor, a research-oriented career usually does not come about spontaneously from medical and/or psychoanalytic training *per se*. The nice thing about Bowlby was that he was curious enough to follow the field of ethological research and to be interested in the findings from that area of research – and then to factor those findings into his own thinking and to articulate it in ways that made researchers want to keep coming to interact with him, especially researchers like Harlow.

### **Ethology and animal psychology**

I do not know exactly how Robert Hinde and Harry Harlow first came into contact with each other, but their relationship was special in light of the philosophical, theoretical, and methodological differences between American animal psychologists and the European ethologists at the time. In general, these two groups of investigators were working in different universes – they came from different traditions, and they had different research agendas. I am not sure that I can articulate all aspects of the ethologists' agenda, other than the 'four whys' and a basic interest in studying animals in their natural habitats. In that day and age, field studies were basically an afterthought for most psychologists doing animal experimentation, whose training almost always included instruction in rigorous experimental design and control, advanced and sophisticated statistical analyses, and a desire to eliminate all extraneous variables in any one study, which typically focused on a single variable.

In fact Harlow was one of the few animal psychologists of his era who routinely carried out follow-up studies over the long term. More often animal research at the time meant getting a group of rats and running them through a single problem under rigorously controlled experimental conditions, then analyzing your findings, and then getting another set of rats and running another test, based on what you found first time around, either replicating the finding or extending or otherwise varying the experimental manipulation. So the last thing these psychologists wanted to do was to study their subjects in their natural habitat, and if they ever did talk about the ethologists, they probably would say something like: “Well, they are just watching their animals without understanding their behavior. How can you understand something if you are not manipulating some aspect of their environment?” So that was their bias.

From my perspective, there apparently is still a little bit of this going on in current interactions between primatologists who do field studies and primatologists who do lab studies. Ethologists will say: “If you do not study primates in their natural habitat, how can you learn anything meaningful, given that you are not studying them in the habitat in which they evolved? Whatever you may find may be interesting, but from our standpoint it is meaningless, because you can not extrapolate from findings in highly experimental conditions to a situation in the wild.” So you can that if these basic differences are still evident even today, it is not so surprising that ethologists and animal psychologists back then were not all that interested in talking to one another, especially given that at that time they did not have e-mail or easy access to overseas flights, so they probably did not have many opportunities to talk to each other on a regular basis even if they had wanted to. If they happened to be in the same institution, they would most likely be in different academic departments: ethologists would be in a biology or zoology department and the experimental animal researchers would be in a psychology department – and oftentimes they would even be in different colleges. So these groups would not usually come together spontaneously, even though they were both studying animals. Moreover, the ethologists would more likely be studying their animals in their natural settings for the animals’ own sake, whereas most of the experimental psychologists would be using animals to study psychological processes or to demonstrate more general psychological principles, usually independent of the actual species they had in their labs.

If you put into perspective the role Konrad Lorenz played and the role Harlow played in Bowlby’s thinking, it is difficult for me to say who had the greater influence. I think it is a matter of comparing apples with oranges. When I met Bowlby, I did not talk with him that much about Lorenz and Tinbergen and their ethological research, but we all know that Bowlby was very familiar with their work and that he was very interested in that literature right from the beginning. Some of the first publications of Lorenz had been translated into English before Bowlby published his first paper with the Robertsons (Robertson & Bowlby, 1952), so by that time he already probably knew Lorenz’s work cold. So I think that one difference is that Bowlby was well aware of the writings of Lorenz and the other ethologists long before he ever met Harlow – but maybe that made Bowlby more likely to be interested in somebody who was studying monkeys than he would have been had he never been exposed to those writings. Also, the basis of one of Lorenz’s early areas of work – his

studies of imprinting in geese – involved a certain degree of isolation rearing, and maybe that made any connection with Harlow's monkey studies seem more relevant as well. But I am not sure if the three of them were ever in the same place at the same time – and at any rate, Harlow clearly came from another academic world and worked in a different academic field than did Lorenz and the other pioneering ethologists.

### **The four whys of ethology**

Although one can never fully understand anything really complicated completely or maybe even sufficiently, I think there have been significant advances in the research and overall knowledge about attachment phenomena with respect to all of the 'four whys' central to ethological investigation: the function, causation, ontogeny, and evolution of behavior. If you start with function, my main question would be: why do you not see attachment relationships in all primates – or even all animals – if its purpose is to promote the survival of the individual and make sure that the next generation is well taken care of? I guess there are a couple of relevant issues, and again I am not as well versed in the classic ethological literature as I would like, but for starters you have the issue of K-strategies versus r-strategies, an issue that has been around for a long time. An r-strategy has you producing a lot of kids with little parental investment in any one of them, and a K-strategy means you have few kids but invest a lot in each – and attachment obviously falls into the latter category.

You can find evidence of increased parental care of offspring in some species relative to other closely related species all over the place. Some of the most elegant work has been carried out with voles. There are both monogamous and non-monogamous vole species, and the investment that one or two parents make largely depends on species differences and the habitats in which they normally reside. Vole species that live in habitats such as meadows, where they may experience frequent floods that can wipe out entire litters overnight, typically follow an r-strategy. Other vole species, like prairie voles, typically live in relatively stable and predictable environments, where one can afford to spend a lot of time and energy caring for a few offspring, especially if both parents are involved. So, with respect to the issue of differential parental investment, mother-infant attachment in primates seems to represent one of the extremes of parental involvement.

The particular feature that I believe is unique about attachment in primates is the specificity of the relationship, and I think part of that comes from the fact that mothers in most primate species have single rather than multiple births or litters, so they can afford to spend much more time and effort with one offspring in any given year. In primates you also have a relatively extended pace of development, so there is a much longer period of immaturity on the part of the offspring, meaning that there is sufficient time to establish a long-term relationship. You end up with a single infant that is dependent on parental care for a long time, but that infant also needs that time to prepare itself for life in a very complicated social environment. Moreover, if the rearing environment encompasses a large physical area consisting of a good deal of basically unrestricted space, which is the case for most terrestrial primate habitats, there must be some sort of motivation for the offspring to stay in proximity to the mother for extended periods, hence the notion of a secure base. And even

when rhesus monkey youngsters are spending most of their waking hours away from their mother, typically after they are six months of age, they still go back to her whenever they become frightened or get tired.

Thus, what basically differentiates attachment from all other types of primate social relationships, including those with peers, is the strong and intimate one-to-one bond between one individual and another – in contrast to, for example, peer relationships where one typically has rather loose bonds with several individuals. In terms of ontogeny, an attachment bond starts out very strong and eventually wanes somewhat throughout development, although it can be reconstituted very quickly under stress, that is, it is responsive to major changes in the environment. Peer relationships by contrast start out relatively weak and increase in relative strength during the childhood years. The mother-infant attachment bond is not symmetrical with respect to specific patterns of behavior, in that what the mother is doing and what the infant is doing are very different – in contrast to peer relationships, which are behaviorally much more reciprocal, and it ultimately has very long-term and even cross-generational consequences. I do not think that all of these features are present in the other types of relationships that rhesus monkeys develop.

On the other hand, there are some other primate species such as capuchin monkeys [*Cebus apella*] who are really smart and quite capable of doing all sorts of things that are clearly adaptive, that don't seem to form "real" attachments. These New World monkeys do not really develop the kind of attachment bond between mother and infant that one sees in rhesus monkeys – or in baboons or in any of the great apes or, of course, in humans. Capuchin monkey infants do not typically exhibit secure-base behavior when they initially leave their mother to start exploring their environment, although in their early months there is clearly a lot of interaction between capuchin infants and their mothers – but it is not the same as what one sees in rhesus. Rhesus monkey infants spend virtually all of their first month of life in intimate physical contact with their mother, usually clinging to her ventrum, and then in the next two months they use their mother as a secure base, repeatedly going back and forth from their mother during brief exploratory forays. During this time and thereafter they gradually establish relationships with peers and others in their natal social group, spending less and less time with their mother, but they almost always return to her between their interactive bouts with others. Capuchin infants essentially stay on their mother's back (rather than on her ventrum) for their first three months or more before they finally start to leave – and then they are largely gone. A capuchin infant can stay away from its mother for as much as an hour at a time while it is exploring its environment, never going back to her at any point during that period. Furthermore, whenever a capuchin infant becomes frightened while it is away from its mother, it is almost as likely to run over and hop onto the back of a different monkey as it is to seek out its own mother. So there is much less specificity to the mother-infant relationship and no real secure-base behavior in this species. Is there a long period during which a capuchin infant is dependent on its mother for survival? Yes, but that is not really the same as an attachment, at least not as attachment was originally conceptualized by Bowlby!

So what is so special about attachment – why would something like attachment be so adaptive for some primate species such as rhesus monkeys but not for others? It may

have to do in part with maintaining or even strengthening family ties across successive generations. In the wild, rhesus monkeys live in large social groups (troops) that are always organized around several multigenerational female-headed families or matriline. Females stay in their natal troop for their entire life, whereas most males emigrate around the time of puberty. It may be that having a strong attachment bond with one's mother helps insure that one will stay relatively close to her and other family members throughout the formative years for males – and well beyond that for females.

Of course, the problem with talking about function is that in many cases any discussion may be little more than simply coming up with stories that seem to make sense. I think one can come up with a number of stories regarding the possible function(s) of attachment that seem at least somewhat plausible. For example, for species in which offspring mature slowly, where most individuals develop an extremely rich social repertoire and spend most if not all of their lives in a large, complicated social group, in order to survive, let alone thrive, (a) those individuals are going to need some strong social support at various times, especially early in life, and attachment will all but guarantee that, and (b) individuals should be able to profit from experiences that the previous generation has accumulated, and attachment can certainly facilitate that process.

Beyond any issues regarding the possible function(s) of attachment, I think Harlow cared a great deal about causation, the second of the 'four whys'. His research was largely devoted to the study of proximal causation: you manipulate a variable or situation, you see what happens as a result of that manipulation, and then you try to draw some inference regarding what might be causing the outcome that you have just observed. This is basic experimental design, and that is how one can demonstrate proximal causation, given the appropriate control conditions. This experimental research strategy was seldom utilized by the ethologists of Harlow's time, but that may be less true today.

In most cases Harlow was not really that much concerned about answering the ultimate "why" question, even he was obviously very interested in making comparisons with humans. That of course was one of his basic rationales for carrying out research with primates – not so much to tell you what monkeys can do but what their behavior can tell us about humans. Instead, the whole of his career was devoted to carrying out well-controlled experiments with primates and in the process to look for possible proximal causes. To be sure, he never used that specific terminology, and even though he must have been aware of what the term "proximate causes" meant to ethologists, he apparently did not care. He never adopted the standard ethological terminology in describing his research – he could have, but he did not. But again: he was talking to a basically different audience than were the ethologists of the time.

The third "why" concerns ontogeny. Obviously my colleagues and I are very interested in development, and I think one of the nice things about attachment theory is that it has brought an ethological perspective to developmentally oriented research that has been in place for almost a half-century and now represents the mainstream view of the field. I think this is an interesting development. Piaget had been "discovered" by most American developmental scientists only shortly before attachment theory also began attracting their attention – but that was many years after Piaget had carried out all of his empirical research.

Piagetian approaches to the study of developmental phenomena seem somewhat less relevant today than before, but attachment still remains largely at the forefront of contemporary developmental psychology.

In the 1970s there was an interesting conflict between Mary Ainsworth (Bell & Ainsworth, 1972; Ainsworth & Bell, 1977) and Jack Gewirtz (Gewirtz & Boyd, 1977a, 1977b),<sup>20</sup> who was (and still is) an effective champion of the behavioral modification view of mother-infant interactions. In a wonderful back-and-forth series of exchanges that was carried out across several issues of *Child Development* they got into an argument over the effects of punishment in bringing up kids. From an attachment perspective you want to limit situations in which punishment occurs, whereas Gewirtz argues that you had to establish reinforcement contingencies one way or another. Michael Lewis, whom I consider to be one of the most pre-eminent developmental psychologists of our time, has often said: "Historically, attachment theory won that argument, because people are still talking about attachment and the things underlying attachment-related phenomena, but most no longer care that much about reinforcement issues."<sup>21</sup> In Lewis' eyes attachment theory has largely superseded ideas about reinforcement for understanding certain social and emotional, if not cognitive, developmental phenomena. So the ethological link that attachment theory brought to issues regarding development has had a powerful and lasting influence and remains strong to this day.

As to the last of the four whys, evolution, most developmental psychologists really have never been that much interested in evolutionary issues, although today we now have evolutionary psychology emerging almost as a separate field. I strongly believe in the theory of evolution and all that it might entail, but I am decidedly not an evolutionary psychologist. It seems to me that the so-called explanations put forward by evolutionary psychologists often seem somewhat shallow. In fact, I think that in many ways evolutionary psychology is to psychology what sociobiology (cf. Wilson, 1975) was to real, serious evolutionary biology and what early psychoanalysis was to biological psychiatry. In evolutionary psychology there are many explanations of human behavior that appear to be exceedingly attractive but that are also unfortunately virtually impossible to falsify. As a result, accounts about possible

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<sup>20</sup> In their study on the relation between infant crying and maternal responsiveness Bell and Ainsworth (1972, p. 1185-1188) concluded that "the more responsive [the mother] is the less likely [the baby] is to cry" and thus that "the processes implicit in a decrease of crying must be more complex than [the] popular extrapolations from learning theory would suggest." The critique by Gewirtz and Boyd (1977a, 1977b) focused on statistical procedures and on the assumption that maternal responding to crying is the inverse of maternal ignoring of crying. In all the discussion seems to be a "cross-paradigm controversy (...) [in which] neither partner can convince the other – unless either one or the other is prepared to abandon his paradigm" (Ainsworth & Bell, 1977, p. 1208).

<sup>21</sup> In a replication study by Hubbard and Van IJzendoorn (1991) the results of the Ainsworth and Bell (1972) study were not supported. Hubbard and Van IJzendoorn use the concept of differential responsiveness to explain that severe distress calls need a prompt reply whereas mild distress vocalizations do not.

evolutionary origins of specific patterns of behavior often turn into story-telling contests, with the one who can tell the most compelling story usually prevailing. There is nothing wrong with story-telling – it certainly can be interesting and even entertaining – but my basic problem with most evolutionary psychologists is twofold: first, they are not all that interested in studying individual differences. In fact, because they are looking for what has evolved and hence already been selected for, if there exists substantial inter-individual variation, then presumably the selection process must still be ongoing. Secondly, most evolutionary psychologists do not really seem to be very interested in development. It just so happens that those are the very two issues that I am personally MOST interested in, and what I center my research around: development and individual differences! So I think evolutionary psychologists like David Buss may be very smart people with very nice insights, but one does not have to buy their entire story – and I think evolutionary psychology has been largely a story-telling enterprise.

### **Cross-fertilization of attachment theory and ethology**

The key concepts in attachment theory drawn from ethology are the five basic drives or dispositions that Bowlby (1958c) put forward in his first monograph about attachment, including contact-seeking and security – and most importantly and underlying all of those dispositions, his view that attachment is a product of evolution, that it has been selected for. As a consequence it appears to be a universal human characteristic – and if it is not present in any particular caregiver-infant dyad, something is probably wrong, either on the part of the parent, the infant – or both. To the best of my knowledge, there are no human societies in which some sort of attachment relationship does not spontaneously appear.

Among the higher primates [Old World monkeys and apes] there likewise are no species in which attachment-like bonds do not similarly emerge between mother and infant, no matter what the subsequent social organization might be. For example, among the apes, chimpanzees live in multi-male, multi-female groups with female dispersal, gorillas live in harem groups, i.e., one silverback male, with male dispersal, whereas orangutans are basically solitary throughout adulthood, except for mothers with immature offspring, and gibbons and siamangs are basically monogamous. In every one of those species of apes there is an obvious attachment relationship between mother and infant – and if that relationship is not present the infant almost certainly will not survive. If you look at all of the Old World monkey species you see essentially the same story. For example, across the macaque genus the different species have slightly different social systems: in some cases the matrilineal families are tighter than others, in some cases the mothers are more willing to have other females both within and outside the family handle their kids – but they all have attachment relationships. With New World monkeys the picture is not as clear: you have the capuchins, for which “true” attachment relationships probably do not exist according to Bowlby’s original formulation, but there are many other New World primate species for which I do not know that much about their characteristic mother-infant relationships.

Regarding the issue of infanticide, a phenomenon that also occurs among chimpanzees as well as in many other primate species, at first blush it does not appear to be very adaptive, but then one must distinguish between two types of infanticide. One type is

the infanticide carried out by intruding males when they take over a group, something that you often see in langurs for example. You almost never see that type of infanticide in macaques, and if you do, it most likely will take place only in a captive situation – and this is probably because most macaque social groups are female-dominated, and they would not tolerate any male killing their kids. If a rhesus monkey male ever tried to take out a female's young infant, the female's family and the rest of that troop would probably attack and mob that male in no time at all. So whenever male infanticide has been reported in the field literature it almost always has involved species characterized by male-dominated social groups.

The other type of infanticide is female-initiated, in which the mother kills her own offspring. Female-initiated infanticide is relatively common in some species of rodents, in canines, and in a number of other carnivores, but to the best of my knowledge it does not typically happen in primates except under very unusual circumstances. Whenever maternal infanticide does occur in macaques, it is usually a consequence of gross maternal incompetence or disturbance – it is clearly not a common event in the wild. It can be argued that this type of infanticide is not adaptive and perhaps that is one reason that one does not see it very often.

Indeed, there are some wonderful anecdotal stories, as well as some fairly comprehensive research (Fedigan & Fedigan, 1977) looking at handicapped primate infants in the wild, and in most cases the mothers appear to go out of their way to compensate for their infants' physical limitations. Among rhesus monkeys for example, if a mother has an infant that is a bit slow in its development, she may end up skipping an entire breeding season and thereby have an extra year to spend with that infant before having another birth. So instead of having a sibling that is one year younger, this kid will have its closest sibling be no less than two years younger – and when one considers that rhesus monkeys grow up about 4 times faster than do humans, that would in human terms be the difference between having eight years of time with your mother without any competition from a younger sibling and only having the “standard” four years. I think this is quite remarkable, especially in light of the reports throughout human history of children being born with handicaps who are then killed by their parents or left alone to die. Another anecdote: when a rhesus monkey infant is stillborn or dies within its first few days, it is not uncommon – but absolutely heartbreaking to observe – for the mother to carry her dead infant around for three or four or more days, not letting go of the corpse until it decomposes. Infants that are severely handicapped often survive in natural settings for remarkably long periods of time, and not only does the mother compensate but in some cases other family members also compensate, and in a few cases individuals outside the family may compensate as well.

What about orphaned infants? First of all, if they are orphaned before they are weaned, then their survival is dependent upon somebody else in that infant's family being in a lactating state – and because this typically occurs in the middle of the group's birth season, usually there are other infants within the orphan's matriline who are still suckling, and sometimes older sisters, female cousins, or even maternal aunts will adopt that infant and nourish it. If the infant becomes orphaned following weaning, then the infant is likely to survive nutritionally on its own – but it usually will still remain in the family and be physically

adopted by another female relative. For rhesus monkeys alloparenting under these circumstances is a quite common and expected outcome. There are other macaque species such as Barbary macaques and bonnet macaques in which mothers routinely pass their kids around not only to other members of the family but to non-family members as well. In those species where there is much more alloparental care even when the mother is present, whenever an infant is orphaned there is usually no major question regarding its survival – somebody else will almost always adopt it. Personally, I do not know the relevant data regarding orphan adoptions in any of the ape species, but I am sure there are primatologists who have studied such phenomena extensively.

Neophobia – fear of the unfamiliar, as well as xenophobia – the tendency to attack anything that looks strange – was commonly observed in the Harlow lab when I first began working there, because at that time the researchers were trying to socialize isolate-reared monkeys by putting them into a playroom with socially normal, same-age peers. What would happen almost every time in these playroom sessions was that the normal peers would start physically attacking the poor isolates as soon as they entered the playroom and continued to do so throughout the playroom sessions. Clearly, the isolates must have seemed very strange to their normal age-mates, so it should not be surprising that they were repeatedly attacked by them. Obviously this was not a very therapeutic situation for the isolates. A few years later, Harlow and I were able to significantly rehabilitate isolate-reared monkeys by putting them into the playroom with monkeys who were much younger than the isolates. These younger “therapist” monkeys, as we called them at the time, did not yet have aggression in their behavioral repertoire – in effect, they were too young to bully anyone. So the isolates essentially grew up interacting with these younger monkeys instead of with someone their own age and that worked, in large part because what those younger monkeys initially did was to physically cling to the isolates rather than attacking them. Moreover, when they were first introduced to the isolates, their play behavior was very simple and did not seem to overwhelm the isolates. In this more benign setting it became relatively easy for the isolates to be brought out of their self-imposed social shell by these very socially active but otherwise nonthreatening youngsters. But what seemed most striking to me when I first came to Harlow’s lab was the degree to which the rhesus monkeys of all ages (except infants) seemed almost predisposed to attack any unfamiliar individual they might encounter.

This is also largely the case for rhesus monkeys growing up in species-normative social settings. If a stranger is introduced to a troop of wild rhesus monkeys, most if not all troop members will instantly identify that individual as a stranger, and if that stranger does not get its act together and immediately begin displaying submissive behavior, it is likely to get literally torn to pieces. These monkeys are also very sensitive to what constitutes aberrant or unfamiliar behavior within their social group, and as was suggested earlier, the basic strategy of most rhesus monkeys can be summarized as: “When in doubt, attack!” Indeed, in this context most impulsive individuals growing up in their natal troop do not necessarily start out being overly aggressive, but they do frequently exhibit socially inappropriate behavior and they do get punished for doing so. They seem not to know how to respond appropriately to such punishment, or they are unwilling to do so, or perhaps they do not care. For whatever reasons, they often persist in these inappropriate behaviors, and

as they mature and get stronger – and become physically capable of causing injury to others through their aggressive responses, they first get shunned by other group members and eventually either get expelled from their natal troop or actually killed by other group members if they do not leave.

Members of rhesus monkey troops generally react to outsiders who do not display appropriate submissive behavior in a similarly strong fashion, probably because these are likely behavioral characteristics that have been selected for in this species. Just like it is possible to have selection for attachment behavior, I think it is similarly possible to have selection for xenophobia, especially in animals that naturally form tight-knit groups that persist generation after generation. That is what rhesus monkey troops are like – and that is what presumably many human communities are like, or at least how they most likely started off. On the other hand, societies in the U.S. these days tend to be much more mobile than in previous decades, and our communities are much less stable, in part because most families are no longer living in the same place generation after generation.

I think Bowlby would have very much liked the gene X environment interaction studies that we and several groups of investigators studying human longitudinal development are currently carrying out – and what we have been finding. I think he would have especially appreciated these new findings, because (a) he absolutely believed in evolutionary principles, including genetic selection, and (b) he certainly knew, especially after his work with Mary Ainsworth, that there clearly exist different types of attachment relationships – they are not always the same across different mother-infant dyads. Instead, there is variability – and where does that variability come from? At least some of it must come from genetic differences among different mothers and infants. On the other hand, Bowlby was certainly an environmentalist in many respects, even though he often talked in terms of selection. Very crucially, he strongly believed that the kinds of experiences you grow up with are going to have lifelong consequences. So he was as aware of the importance of experience and emphasized it as much as any dye-in-the-wool behaviorist might, even though his theoretical background and training were obviously very different. I have to believe that the recent demonstrations that early experiences can have quite different consequences depending on what one's genetic background happens to be would have been particularly attractive to Bowlby, and I am sure he would have accepted those findings without any problem at all. I obviously can not speak for him now, but in my experience he seemed open-minded enough that I can not imagine that he would not have been responsive, indeed enthusiastic, to these demonstrations of gene X environment interactions involving different attachment relationships in monkeys.

### **Generalization of attachment behavior and culture in animals**

The extent to which it is possible to generalize attachment phenomena from humans to nonhuman primates depends of course on the species of primate. In cases for which the behavioral parallels are obvious, you can generalize a great deal. However, there are few areas where generalizations can become somewhat more problematic, no matter what the primate species might be. My favorite example of this is the notion of working models: the presumed way in which attachment experiences become internalized in humans. The idea is

that as you are growing up and developing a particular attachment relationship with your mother (and/or father), those experiences induce you to generate a “working model” in your mind that then guides you through the rest of your social development, affecting the way you interact with peers, influencing the way you later select a mate or partner and the type of relationship you establish with that person – and ultimately the way you raise your own kids. And what is this self-reflection? According to current views regarding working models, it is the going over and over again and again in your mind of what you remember experiencing with your parent(s) earlier in life – and which you presumably then use to generate your own personal views of life in general and your own personal relationships with others in particular – and those perceptions persist throughout the rest of your development and perhaps even the rest of your life. The idea that working models provide the basis for the long-term effects of early attachment experiences has been a big deal among attachment theorists over the last fifteen or twenty years. However, there is one basic problem with this view that I believe is generated by the data coming from long-term studies with monkeys. The problem is this: I do not think monkeys do much self-reflecting as they are growing up. In fact, to follow that phrase, they can barely recognize themselves in a mirror.

The question of whether monkeys or any other nonhuman primates are capable of self-reflection or indeed any form of self-awareness has been the subject of considerable debate for some time. During this time, the “gold standard” for demonstrating such capabilities has been the so-called “mirror test”. The mirror test basically involves anaesthetizing a subject, painting a red dot on its forehead, and then when it awakens, placing it in front of a mirror and seeing if it touches the dot as soon as it views its reflection. A number of investigators, most notably Gordon Gallup (1970), have reported that some chimpanzees and some of the other great apes consistently “pass” the mirror test but interestingly, not all apes can do this, particularly ones who were reared in socially deprived environments. In contrast, virtually all monkeys tested to date have “failed” this task, leading most investigators to conclude that even if apes have this capability, monkeys do not.

In point of fact, this conclusion may be somewhat premature. One of the problems with using the mirror test on monkeys is that because they tend to be neophobic, their usual initial response to seeing their reflection in a mirror is to threaten the reflection and then avoid any additional eye contact with the mirror. I mean, you have to carry all sorts of extensive manipulations to get any monkey to be willing to look at a mirror for any extended period of time. My colleague Melinda Novak did just that – she trained rhesus monkeys to get used to mirrors and then she had them perform the mirror test. What she found I think is really interesting – in every case the monkey would stare at the mirror, sometimes threatening the image, look away, stare at the mirror again, look away, and then stare at the mirror and briefly touch the red dot on its forehead – and then look away. So it appeared that these monkeys had at best a fleeting recognition that something was on their forehead based on what they saw in the mirror, but they apparently could not maintain that insight for any appreciable period of time. It was as if that capability was right on the edge of their consciousness, which I think is a really interesting phenomenon.

Nevertheless, even in light of these presently unpublished findings, it seems obvious (at least to me) that monkeys do not normally engage in a great deal of self-

reflection and at best are barely capable of identifying themselves in a mirror. On the other hand, they are REALLY good at identifying relationships among other individuals – within their own social groupings they know who is related to whom and where all the other monkeys around them fit into the dominance hierarchies of their group and even where they themselves fit in relative to those other individuals. So they are really good at that – but they are apparently incapable of prolonged self-reflection.

Now, for generation after generation, monkey infants become attached to their mothers, and as they go through life, their other social relationships are affected by the nature of that initial attachment relationship – and when the females have kids of their own they tend to reproduce the attachment style or regenerate the attachment relationship that they experienced with their own mother as infants. The data on cross-generational transmission of specific maternal behavior patterns that come from studies of monkeys are very compelling – they are actually much more solid than are the extant human data, even though adult attachment theory is a big deal right now. So here you have monkeys who exhibit virtually all the behavioral phenomena associated with cross-generational transmission of attachment styles that humans are presumed to be doing – but I do not believe that the monkeys who exhibit these behavioral patterns ever sit back and reflect on their attachment experiences, let alone form a working model and act on it. It reminds me of the story about Harlow's speech to the psychoanalysts: in terms of their apparent ability to transmit particular styles of maternal behavior, especially those associated with attachment, across successive generations, monkeys do everything at least as convincingly as do humans, but they apparently can do this without relying on any sort of working model.

So what does this all mean? I think it means that the basic biological foundation of attachment is shared by monkeys and humans alike – but that humans have additional cognitive capabilities overlaying the behavioral propensities and biological underpinnings associated with attachment. These additional cognitive capabilities enable us to reflect on our previous experiences and to take account of them as we enter into other social relationships and accumulate additional social experiences. Once they have established their initial attachment relationship with their mother, monkeys apparently do not need these “extra” cognitive capabilities in order for that relationship to be able to shape their subsequent relationships with other monkeys or to guide their social activities throughout the rest of their life – those phenomena clearly take place despite the apparent absence of any cognitive reflection on the part of the monkeys throughout the process.

But we are humans and obviously we do reflect on our experiences, and what this means is that we are probably more aware of what is going on, or are certainly able to articulate what is going on, than any monkey, even though it may end up on a comparable developmental trajectory. Moreover, under certain circumstances such as in therapeutic interventions, we can take advantage of those reflections and perhaps alter the trajectory, essentially concluding that we do not like that particular pathway, and decide to try to do something else instead. I am not sure that a monkey could ever be able to do that. So this is how the flexibility that our unique cognitive capabilities provide can be used to advantage. But there is also a potential disadvantage: if you obsessively dwell on your previous experiences to the point of excessive rumination, those cognitive activities might literally

destroy you emotionally, whereas a monkey would probably continue merrily along its particular developmental pathway. So that is one major difference between monkeys and humans – and that is probably why the primate data regarding intergenerational transmission of attachment patterns tend to be clearer than the human data. This does not mean that the biological underpinnings for these attachments are grossly different in monkeys and humans, or that they are more or less important in either species. It is just that we are lucky (or unlucky) enough to be able to perceive and even act on our feelings about relationships, either positively or negatively. But the basic biology is there in both instances – and in that sense Bowlby was correct right from the beginning. The biological processes accompanying attachment behavior that we are now able to see reflected in hormonal systems, in neurotransmitter systems, and most likely in gene expression, are probably all happening in humans, just as we have been able to demonstrate in our monkeys.

Whether monkeys or other primates (or other animals) have actual cultures in the human sense largely depends on how one defines culture. If one defines culture as “the transmission of certain characteristics, values, rules, and ways of behaving from one generation to the next within the same group”, a definition with which I am quite comfortable, then the answer is: “Absolutely yes!” – and as far as I am concerned, that is no longer an issue. But for purists who want culture to require a written record or perhaps “only” an oral history documenting that intergenerational transmission, no nonhuman species can ever develop a culture. On the other hand, the transmission across generations not only of attachment styles, but also particular forms of tool use, and very specific patterns and sequences of social interaction clearly takes place in many primate groups. Why do you think strangers are identified immediately by members of a rhesus monkey troop? It is not simply the stranger’s physical appearance – from the troop members’ vantage, it may be that the stranger does not approach other monkeys with exactly the “right” gait or sit next to them at exactly the “proper” angle, or expresses a slightly different dialect in its vocalizations toward them – subtle deviations from the behavioral patterns that have characterized that particular troop across multiple generations which make it clear to the troop members that the stranger is really not one of them. So I think culture *per se* can encompass not only the transmission of ideas, values, behavior patterns, communicative patterns, or whatever technology might be passed from generation to generation, but also to some degree a sense of “us versus them” as well, a sense of having something that is part of “us”. Rhesus monkeys and chimpanzees surely have that, and I am certain that there are other species that have it as well. So again, according to a definition of culture with which I am comfortable and which I believe is acceptable to many people, primates certainly have culture.

### **The Environment of Evolutionary Adaptedness (EEA)**

The EEA is a concept Bowlby used to explain attachment behavior as a survival strategy. The concept has apparently come in for some criticism lately, although I do not know what all of the perceived problems might exactly entail. Of course, I can make some guesses. In general, I absolutely believe that attachment phenomena are a product of evolution and that various behavioral and biological characteristics have been selected for over many, many millennia. I think one need only go as far as considering different aspects of parenting –

each aspect probably has had a different selection history, that is, each aspect has independently been subjected to different selective pressures. Of course, I am a strong believer in the basic notion of evolutionary selection, but I think that given Bowlby's characterization of the EEA, it actually may not make that much difference what specific environment you grow up in today – if you are a primate infant you are going to need some sort of long-term relationship with a specific caregiver or caregivers who can nurture you for a long enough period for you to develop the emotional regulation and social skills that are required for life in a complex and dynamic social environment, to develop strategies for dealing with the demands and coping with the problems that over time are part and parcel of that environment. So in that sense it does not make much difference what environment that individual came out of, because those selective pressures would have been there in any environment.

What was the specific environment in which humans evolved? Many people think we originally all came out of Africa, but that is still open to some dispute. If you are talking about attachment *per se*, the selection almost certainly began long before there were any humans, probably around 35-25 million years ago, sometime during the period when the evolutionary ancestors of the great apes and Old World monkeys of today began to split off from the ancestors of today's New World monkey's species. So whatever environments those ancestral primates were living in back then is probably the so-called EEA with respect to attachment. A second point is if you take a species like humans or a species like rhesus monkeys, what is their natural habitat? Today that question would be difficult to answer for humans. Is it in the cities, is it in the countryside, is it where hunter-gatherer societies are currently living at this particular point in time? For rhesus monkeys, is it the savannah regions of the Indian subcontinent, is it the various forested areas of that subcontinent, is it in the Himalayas, is it at the edges of Indian deserts – or is it in the middle of India's largest cities? Rhesus monkeys can be found in all of those places today, and they appear to be able handle life in each place quite well.

Does it make any difference where they first came from? Some characteristics probably have served them well in every one of those environments and all that preceded them, and I think attachment is clearly one of those characteristics. No matter what environment you happen to be born into, you still need to be fed, you still have to be protected, you still must be kept warm. One criticism of EEA is that Bowlby presumably was imagining a hunter-gatherer society living in a savannah environment in which one of the roles of the attachment figure would be to protect the infant against predators. He was probably thinking of small groups protecting themselves and that is true for many primates, especially chimpanzees, gorillas, and the other ape species who all live in, by our standards, small groups that never contain more than 20 or 25 individuals. In contrast, rhesus monkeys often live in groups that have 200 to 300 individual members, which clearly is not a "small group".

Bowlby initially thought that human infants formed an attachment with just one caregiver, and one of the criticisms was that in a group that would not be the best thing to do – it might be better to have multiple relationships. If you look at rhesus monkeys, they basically have single, one-to-one attachments between mother and infant, with probably

fewer cases of “secondary” attachments or other alloparental arrangements than one sees in most other primate species, including other macaques – and they have certainly fared rather well compared to those other species. But what rhesus monkeys also have are many other kinds of social relationships – as do humans and most other primate species, even the ones in which mothers routinely pass their infants around to other adults. Those other social relationships are fundamentally different from attachment relationships, a point I keep making over and over again.

For example, consider peer relationships: the basic characteristics of peer relationships are different from those of mother-infant attachment relationships with respect to just about every dimension one can imagine. They are different in terms of the specific behaviors that are most predominant: rhesus monkey attachment is characterized by high levels of ventral contact between mother and infant and very low levels of play – the one thing these mothers do not do with their kids is play a lot, and their offspring probably play less with them than with any other group members, except perhaps other adult females. In contrast, the most predominant behavior in peer relationships by far is social play. With respect to relative reciprocity, peer relationships tend to be highly reciprocal, whereas the relationship between a mother and her infant is basically asymmetrical, especially in the infant's initial months of life, when mother is clearly giving more to the infant, and the infant is taking a lot more from its mother than vice versa. In terms of exclusivity, attachment represents a strong and highly exclusive bond between an infant and its mother, whereas peer relationships feature relatively loose ties with multiple partners. With respect to the time course of the relationship, an infant's attachment to its mother is strongest during the first month of life and thereafter begins to wane thereafter, especially after weaning and following the birth of a younger sibling. By contrast, peer relationships start off with relatively few mutual interactions, but those interactions increase dramatically following weaning and end up dominating social activities during rest of the childhood years.

Harlow recognized these differences as well as anybody when he introduced the concept of different affectional systems (Harlow & Harlow, 1965), and his work in this area predated what are now called social networks. The point is that most primates develop and maintain a variety of complex social relationships throughout life. I believe that Bowlby was basically wrong when he said that the attachment relationship provides the prototype for all subsequent social relationships. It is not a prototype; it is, in point of fact, quite unique. But Bowlby was absolutely right when he argued that an effective attachment relationship is crucial for the normal development of these other types of social relationships, because if you have a messy situation with your mom, it is likely going to mess up your ability to interact with peers, and ultimately it is likely to mess up your ability to deal with partners. So I think the kind of environment that he was talking about with reference to EEA is relevant not only for attachment but also for all these other kinds of social relationships that come to dominate the lives of humans and the lives of rhesus monkeys throughout development and beyond.

If you look at the everyday life of rhesus monkeys living in the wild, what do they spent their time doing? They usually do not have to worry too much about getting enough food because they can eat just about anything, so in relatively few places is obtaining food a

major daily problem, especially in locations where there they are being provisioned. They usually do not have to worry about predators, except for the few individuals living on the periphery of a wild troop who risk getting picked off by a raptor or a leopard – within the core of the troop itself there is no predator that is likely to be successful if it attacks, because the troop members will immediately mob and either quickly destroy that predator or at least drive it away. So predation is seldom a major problem for these monkeys. What they do have to worry about – and they have plenty of time to do so – is social relationships and social interactions with other troop members. These monkeys spent most of their time during the day dealing with one another in both positive and negative – affiliative and agonistic – ways.

The nature of this situation becomes obvious whenever one visits the free-ranging colony of rhesus monkeys who have been living on Cayo Santiago, a small island off the southeastern coast of Puerto Rico, since the mid-1930s. This island has a population of approximately 1200 rhesus monkeys residing in seven different troops. Human visitors to Cayo Santiago can stand almost anywhere on the island and watch the different troops pass by, or even walk right through the middle of any of the troops – and be largely ignored by all of the monkeys. Why? – it is because most of these monkeys usually can not afford to spend any time watching any human. Instead, they are too busy looking over their shoulder to see what Uncle Bill and Aunt Mary might be doing over here or who is getting into a fight over there or what might be brewing across the way that might lead to other problems down the line. So their daily life is largely spent engaging in multiple interactions with multiple individuals, and underlying most of these interactions are the multiple relationships they have established with family, friends, and other monkeys in their troop.

You could imagine that if you were an infant monkey on this island you would be attached to your mother, but you also could have long-term relationships with your Uncle Bill and Aunt Mary – and then if your mother happened to get severely wounded or become gravely ill, or even die, your chances of survival would be much higher if you were able to count on them for social support. You could have relationships with other individuals both inside and outside of the family – they would be familiar and your relationships would likely involve predictable sequences of behavior and predictable types of behavior. So, for example, your Aunt Mary might often contact, cuddle, and groom you, but not as much as your mother – and when the chips come down, you are going to run to your mother instead of your aunt or your older sisters or your peers – unless there is something seriously wrong with your mother.

Thus, there appears to be a difference between familiarity and having a long-term relationship, as Hinde (1978) has beautifully described in his article on what constitutes a relationship. The attachment relationship is special, but Bowlby may have put too much emphasis on when he said that you can not substitute it or that things become troublesome when you try to substitute it. This view led him to criticize day-care programs, which at one point caused him some problems. Yet, we do not know if day care will ultimately cause our own society its own long-term problems – that will probably take at least a couple of generations to find out one way or another. But I do not think these different social relationships are entirely mutually exclusive. Rather, I believe that the beauty for advanced primates is that they can deal effectively with social complexity because they are able to

develop and maintain multiple relationships of different types with different qualities and different intensities.

### **Influence of Bowlby and attachment theory on Suomi's work**

Bowlby and the attachment theory he developed clearly influenced my own thinking and research right from the very beginning, because I knew about Bowlby's work even before I started working with primates. When I began carrying out separation studies under Harlow's tutelage, Bowlby of course was the inspiration, just as he had been the inspiration for Harlow. The very first time that I met Bowlby at that afore-mentioned symposium in New York, Mary Ainsworth went after me in her public commentary on my presentation, because in my characterization of peer-reared monkeys I talked about "attachment between peers", and she argued that peers can never become attached to each other – attachment is only for infants and their mothers. From that day on, whenever I talked to Bowlby he would always emphasize: "Do not listen to Mary – I am very interested in the relationships those peer-reared monkeys have myself. What can they tell us about attachment and in what sense can we consider them more like mother-infant relationships as opposed to the kinds of relationships peers usually develop with each other?" So he inspired – well, I do not know if "inspired" is exactly the right word because Harlow was already talking with me about this – but Bowlby certainly reinforced the view that there were other relations than with the mother that might be important, although they were very likely different. We actually spent almost all of our time together asking each other what we were doing, discussing what was we were each interested in, and what I might do with the monkeys that might be helpful to him in his own research and thinking, and he basically asked on several different occasions: "What have you been doing – and what do you think you would find if you did this to the monkeys or what if you did that – that I could incorporate into my own work." Here was this true giant in the field asking a young researcher like me questions like that – it was really something quite special for me personally. But I think a common thread throughout all of our discussions was the basic notion of the importance of social relationships. Social relationships are really the things that make us humans and make rhesus monkeys rhesus monkeys... it is not so much how smart we are or how good we are at finding food, or how well we can avoid predators – it is how we get along with those around us, and what might go wrong in those relationships and why they might be going wrong – and how much of that might be attributable to early attachment experiences. I think the work he was doing with Ainsworth, especially the characterization of different kinds of attachment – and the idea that differences in these early relationships are really meaningful and have long term consequences, was very, very important. When I was talking with him about long-term consequences, we were talking only in terms of social capabilities and emotional regulation, because at that time nobody was looking at possible physiological correlates. It was only when William Mason (Wood, Mason & Kenney, 1979) and Seymour Levine (Mendoza, Smotherman, Miner, Kaplan & Levine, 1978; Mendoza, Coe, Lowe & Levine, 1979; Gunnar, Gonzalez & Levine, 1980) and others started collecting physiological data in attachment and separation studies a few years later that the influence of these relationships and social manipulations on biological functioning became apparent. We now know that those

influences affect basically every biological system the body has. But had I not gained an appreciation of the importance of these relationships, I probably would have never looked at these other factors as a consequence of attachment related manipulations.

### **Conclusion**

The most interesting thing to me about Harlow and Bowlby is that even after all these years, the research areas pioneered by Harlow that clearly influenced Bowlby are still being actively pursued by developmental scientists across multiple disciplines, and the ideas about attachment that Bowlby developed into a formal theory are still in the mainstream of developmental psychology and child psychiatry, and are considered highly relevant in several other fields of clinical study. The contributions of both Harlow and Bowlby have stood the test of time very nicely, and that is the ultimate compliment one can pay to either a scientist or a theoretically oriented clinician, whether they are collecting their own empirical data or are using the findings of others to generate a creative and compelling theory. Attachment theory has basically stood the test of time over the past 50 years, and I believe it will continue to do so well into the future.

## **CHAPTER 6.**

## **DISCUSSION**



This thesis explored the roots of attachment theory and, more specifically, addressed the cross-fertilization of attachment theory and ethology. Our goal was to carefully investigate the influence of ethology on John Bowlby's thinking, as well as the reciprocal influence he had on ethological research. Until now, little attempt has been made to systematically research and extensively describe this episode in the history of attachment theory. In a description of the first half of John Bowlby's life, Van Dijken's (1998) emphasis was on the early stages of attachment theory and she did not analyze Bowlby's and Ainsworth's use of the new science of ethology in any detail. It is here that this research project started off. The project took a historical approach and used three different kinds of resources. First, Bowlby's publications and the ethological literature were thoroughly analyzed. A second way of retrieving important information was through oral histories. Finally, archival materials proved a very useful source of information. Of course, the information from these different sources was cross-validated in an iterative process.

Starting point for this thesis was the publication of Bowlby's (1951, 1952) monograph on maternal deprivation for the WHO and the different issues of separation that Bowlby reported in his study. In this respect, attention was drawn to observations made during wartime evacuations and in residential nurseries, to the discussion concerning visiting of children in hospital, and to results of clinical studies and hospitalization studies. Contrary to general belief, Bowlby was only one of many who were concerned about potentially harmful effects of temporary mother-child separations. Nevertheless, we concluded that the publication of his WHO monograph was an Archimedean point in the construction of attachment theory (Chapter 2).

At the time of publication of the WHO report, Bowlby was dissatisfied with psychoanalytic theory, because it could not account adequately for observed facts concerning the responses of young children to separation from their mothers and to deprivation of maternal care (Van Dijken, 1998; Van Dijken et al., 1998). So when Bowlby's attention was drawn to ethology in 1951, he quickly saw its potential as a new theoretical approach. Ainsworth later was to state that Bowlby's "discovery of ethology was the key that released the main structure of attachment theory all at once" and that "attachment theory began with a sudden flash of insight, sparked by ethology, that led to a scientific revolution, the understanding of personality development" (Southgate, Ainsworth & Southern, 1990, p. 13).

In this thesis, based on unique evidence from oral histories and little-known archival material, it was argued that Bowlby's interactions with key players in the field of ethology such as Huxley, Lorenz, Tinbergen, and especially British ethologist Robert Hinde were decisive in constructing a new framework to explain mother-child interactions in early life (Chapter 3). Almost as crucial was the work of American psychologist Harry Harlow, who provided Bowlby with evidence of studies on separation in rhesus monkeys, at a time when Bowlby was looking for empirical confirmation of his ideas. We used the hitherto undiscovered correspondence between Harlow and Bowlby in our analysis to illustrate the importance of the solid empirical foundation for Bowlby's theoretical construction (Chapter 4). Not only was Bowlby influenced by ethologists and animal psychologists, in his turn he also influenced the work of many in the field of animal behavior studies. Bowlby's influence

on Hinde and Harlow was discussed in Chapters 3 and 4 respectively, and the cross-fertilization of attachment theory and primate research in recent times was illustrated by an annotated and edited interview with Dr. Suomi (Chapter 5).

### **Limitations of the study**

The approach in this thesis has several potential limitations. First, an uncritical use of oral histories can lead to overreliance on a certain informant, while the human memory is generally very unreliable. We tackled this problem by cross-validating the information we gathered from eye-witnesses with archival materials, scientific publications, and other written sources. If any doubts arose as to the reliability of an informant because his or her account did not agree with contemporary documents, preference was given to the written sources.

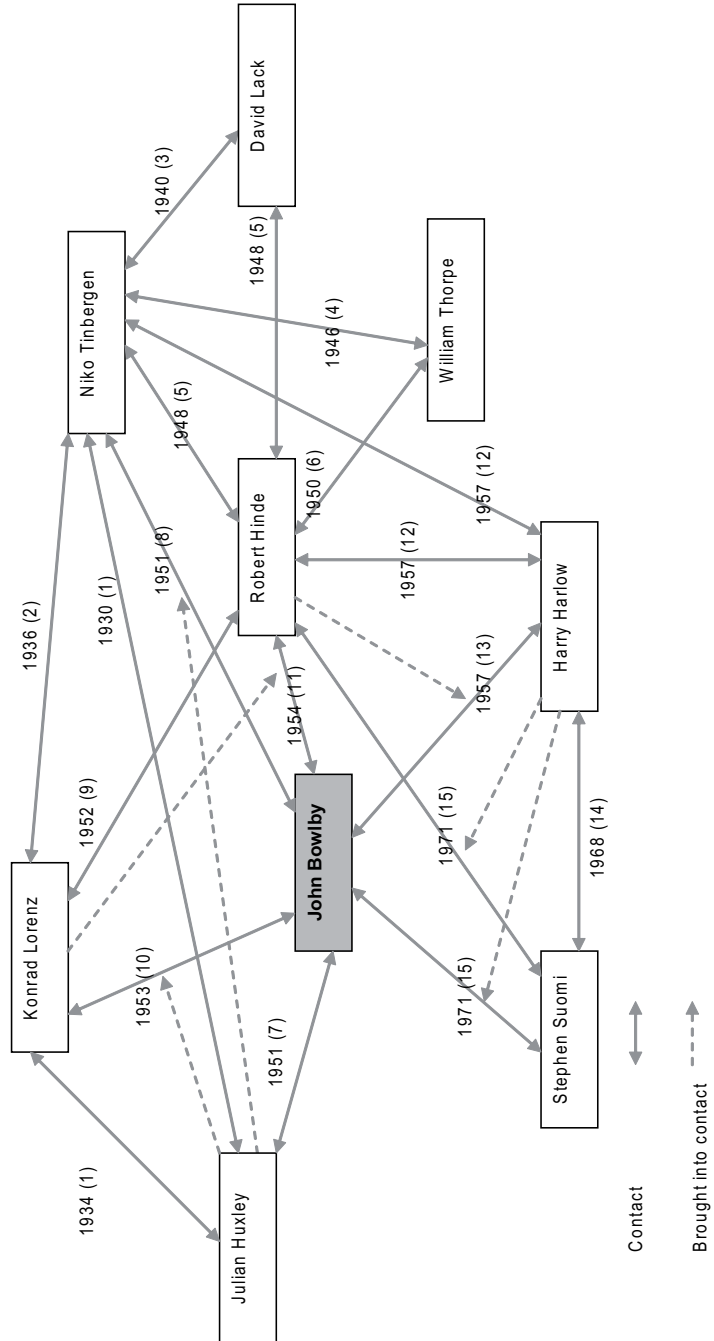
A second potential shortcoming of this study is that it combines intellectual history with biographical accounts. Critics might argue that such a combination is unsatisfactory and that one should either write a biography or a history of ideas. However, we consciously decided not to make a choice between either of these. Strictly speaking this thesis is not a biography, but it is part of a series of studies (Van Dijken & Van der Veer, 1997; Van Dijken, 1997, 1998; Van Dijken et al., 1998; Van der Horst et al., 2007; Suomi, Van der Horst & Van der Veer, 2008; Van der Horst, LeRoy & Van der Veer, 2008; Van der Horst & Van der Veer, 2008a, 2008b, in press), which together give the most complete biographical and scientific overview of Bowlby's life and work and the growth of attachment theory to date. In all, we believe it is a defensible mixture of an analysis of the history of the roots of attachment theory and the personal contacts and scientific debate between the persons closely involved.

These potential limitations aside, there is ample evidence of the cross-fertilization of ideas that was presented in this thesis. Here, this evidence will be further integrated and discussed. First, we will take a closer look at the interpersonal relations leading to the reciprocal influence of attachment and ethology by once more presenting Bowlby's interaction with ethologists and animal psychologists as discussed in previous chapters. These interactions are now summarized in a sociogram (see Figure 7) or "sociometric chart plotting the structure of interpersonal relations in a group situation" (Merriam-Webster Online Dictionary).

### **Sociogram**

It is no coincidence that the interactions in this sociogram, leading to Bowlby's introduction to ethology, start with Julian Huxley, Niko Tinbergen, and Konrad Lorenz. Bowlby acknowledged them in his *Attachment and loss* and claimed he was "grateful to all three for continuing [his] education and for encouragement" (Bowlby, 1969/1982, p. xviii). Tinbergen and Lorenz, as the proponents of continental ethology in the 1930s, met with their British counterpart Huxley at separate ornithological conferences in Amsterdam in 1930 and Oxford in 1934, respectively (Burckhardt, 2005, p. 160; Kruuk, 2003, p. 80). Later, both Lorenz and Huxley would attend the WHO conferences in Geneva and London from 1953-1956, meetings Bowlby also participated in. In 1936, Lorenz visited Leiden and during a symposium on *Instinct* was first introduced to Tinbergen (Roëll, 2000, p. 111). As we have

Figure 7. Sociogram of John Bowlby's interactions with ethologists and animal psychologists



seen, their interactions would have great impact on the field of biology and they were awarded the *Nobel Prize in Physiology or Medicine* (together with Karl von Frisch). In the 1940s, it was mostly Tinbergen who interacted with leading researchers in British ethology, much more so than did Lorenz. For example, Tinbergen corresponded with David Lack from 1940 onwards and met him for the first time at a conference in Leiden in 1946 (Burckhardt, 2005, pp. 285-286). Tinbergen and William Thorpe had their first encounter when Tinbergen was traveling through Great-Britain with Lack in 1946, right after their first encounter in Leiden (Kruuk, 2003, p. 143). In 1950, although he was officially supervised by Lack, Robert Hinde started his PhD with Tinbergen after the latter had made the move to Oxford (Kruuk, 2003, p. 339; Tinbergen, 1991, p. 463). Eventually, Thorpe set up an ornithological field station in Cambridge and asked Hinde to supervise the enterprise. Meanwhile Hinde and Lorenz first met at a symposium in Buldern in 1952, where Hinde impressed Lorenz with a paper on the mobbing reaction of chaffinches to owls (Van der Horst et al., 2007). Thus, in the 1930s and 1940s, a network of European ethologists was created, which quickly resumed its activity after the war. It was in the early 1950s that Bowlby became acquainted with many of them.

After his attention was drawn to ethology, Bowlby's first interaction with this network of ethologists was during a vacation with his family-in-law in 1951, where he met with Huxley, who encouraged him to go into ethology in more depth and referred him to the work of Tinbergen. After reading his way into ethology, Bowlby suggested that Lorenz was invited to the first WHO conference on "the psychobiological development of the child" (Tanner & Inhelder, 1971) in Geneva in 1953. At this meeting, Lorenz spoke highly of Hinde's work and Bowlby became interested in meeting Hinde. The first encounter between Hinde and Bowlby was rather by chance, though, during a scientific meeting on ethology and psychiatry organized by the RMPA in London in 1954. The organizers had intended to invite Tinbergen and Lorenz, but they were both unavailable, so Hinde and Bowlby were asked to participate instead (Van der Horst et al., 2007).

A couple of years later, in 1957, Tinbergen, Hinde and Harry Harlow attended the same conference in Stanford, where European ethologists and American animal psychologists, on the invitation of Daniel Lehrman, attempted to bridge their differences. After returning to England, Hinde drew Bowlby's attention to the work of Harlow and Harlow and Bowlby corresponded from 1957 onwards and visited each others laboratories in subsequent years (Van der Horst et al., 2008). Steve Suomi was introduced to Harlow by his father after the latter ran into Harlow on an airplane and through Harlow Suomi was later introduced to both Bowlby and Hinde at a scientific meeting in New York (Suomi et al., 2008).

The sociogram presented here (see Figure 7) summarizes the interactions Bowlby had with many influential ethologists and animal psychologists from the 1930s to the 1970s. Many of these interactions have been discussed in previous chapters and have been carefully documented. We present this sociogram as part of the evidence of a cross-fertilization of ethological and attachment ideas.

### **Applying ethology to attachment behavior: Tinbergen's four whys**

Another way of assessing ethology's influence on John Bowlby and attachment theory is to look at it from a theoretical perspective, i.e. to investigate how Bowlby used certain ethological ideas or notions in attachment theory. There is vast evidence in *Attachment and loss* (Bowlby, 1969/1982, 1973, 1980a) that he was indeed heavily influenced by the ethological framework. For example, in Volume 1 Bowlby addressed each of Tinbergen's four whys of behavior: evolution, causation, function and ontogeny. By describing how Bowlby answered these ethological questions for attachment behavior, we will here demonstrate this influence.

Earlier we addressed the issue of the concept of the Environment of Evolutionary Adaptedness (Van der Horst et al., 2007; see Chapter 3), which Bowlby used to answer Tinbergen's question of the evolution of behavior. Bowlby (1969/1982, p. 47) leaves no doubt about the central role of this concept in attachment theory: "the concept EEA is vital to the argument of this book". According to Bowlby, the behavior that ensures a tight bond between mother and child evolved into instinctive behavior as a result of natural selection: children attach themselves to their caregivers because of the survival value in man's EEA. According to Bowlby:

*the only relevant criterion by which to consider the natural adaptedness of any particular part of present day man's behavioural equipment is the degree to which and the way in which it might contribute to population survival in man's primeval environment, ... [i.e.] the one that man inhabited for two million years until changes of the past few thousand years led to the extraordinary variety of habitats he occupies today... It is against this picture of man's EEA that the environmentally stable behavioural equipment of man is considered. Much of this equipment... is so structured that it enables individuals of each sex and each age-group to take their place in the organised social group characteristic of the species. (Bowlby, 1969/1982, pp. 59/63-64; original italics)*

In their environment of adaptedness humans had to be equipped with instinctive behavioral systems to negate the dangers of predators or aggressive members of their own species. The bond between mother and child is the consequence of such an essential behavioral system. So attachment behavior is the behavior that promotes and maintains proximity to caregivers to ensure safety against such dangers. With this description of the EEA and the evolution of attachment as instinctive behavior, Bowlby answered one of Tinbergen's four whys.

Bowlby's (ibid., pp. 85-103) description of the causation of instinctive behavior, or the activation and termination of it, followed ideas proposed by Hinde and Tinbergen and is based on animal research. As causes for instinctive behavior, Bowlby named hormone levels, organization and autonomous action of the nervous system (CNS), and environmental stimuli. Bowlby (ibid., pp. 124-140) clearly distinguished causation, the immediate causes of a system's activation, from the function of a behavioral system: "The function of a [biological] system is that consequence of the system's activity which led to its

having been evolved, and which leads to its continuing to remain in the equipment of the species" (ibid., p. 127). Finally, Bowlby (ibid., pp. 145-174) addressed the issue of the ontogeny of instinctive behavior, which "usually take[s] at first a primitive form and proceed[s] thence to undergo an elaborate process of development" (ibid., p. 145). Bowlby (ibid., pp. 199-204) largely based his description of the ontogeny of attachment behavior in human infants on Ainsworth's (1963, 1967) Uganda reports. Her observations in Africa were a confirmation of Bowlby's theoretical ideas and of the results of Harlow's observations of and experiments with monkeys. We may thus conclude that Bowlby satisfied Tinbergen's four criteria for the satisfactory explanation of a biological phenomenon: he provided an account of the evolution, causation, function, and ontogeny of attachment behavior.

### **The role of observation and experiment**

Ethological studies were and still are characterized by the emphasis on observation of (animal) behavior in a natural environment (cf. Tinbergen, 1963). Bowlby valued this emphasis on observation of real life events, but early in his career encountered much resistance from psychoanalysts in the *British Psychoanalytical Society*, particularly the Kleinian school of thinking within psychoanalysis (Van Dijken, 1998). Bowlby not only criticized their neglect of observation, but their lack of what he called scientific training as well: "Unfortunately some of the leading people in psychoanalysis have had no scientific training. Neither Melanie Klein nor Anna Freud knew the first thing about scientific method. They were totally ignorant" (Bowlby et al., 1986, p. 45). Obviously, Bowlby valued observation and experiment in his research from early on in his career.

Bowlby did have the scientific and experimental background others within the psychoanalytic movement lacked: as a medical student at Cambridge he read experimental psychology and thus had a grounding in statistics and experimental design. Also, Bowlby was in the Research and Training Centre of the Officer's Selection Board during World War II, which he described as a practically oriented centre where he worked with three academic psychologists. "In the army I received what was really a post graduate training in psychology" (Bowlby in Senn, 1977, p. 9). So, although Bowlby was a clinician at heart, he did know about experimental procedures and when he went to the Tavistock Clinic after the war in 1946, his brief was to provide three strands – not only a clinical service and a training program, but a research program as well (Bowlby et al., 1986, p. 40). The fact that Bowlby preferred to do observations in natural situations, also becomes clear from his cooperation with James Robertson, resulting in *A two-year-old goes to hospital* (Robertson, 1952) and *Going to hospital with mother* (Robertson, 1958c).

Regarding the valuable role of observation and experiment, many experts of attachment theory underline the importance of Mary Ainsworth's contributions to attachment theory. Many stress the fact that attachment was not a one-man job by Bowlby (e.g., Van der Horst et al., 2007; Stevenson-Hinde, personal communication, September 10, 2007; Steele, personal communication, October 12, 2007; Waters, personal communication, October 15, 2007; Bretherton, personal communication, October 19, 2007). But these experts also value Ainsworth's theoretical contributions. For example, this is what her student Everett Waters said about her contributions:

I don't think that one should under-estimate Mary Ainsworth's theoretical contributions to [attachment theory]... She was thinking of herself as the empirical apologist for Bowlby's theory when in fact she was making important contributions to the way security was conceptualized... [Bowlby] certainly wouldn't have called it a one-man job... Bowlby needed Ainsworth. (Everett Waters, personal communication, October 15, 2007)

This thesis only briefly discussed Ainsworth's contribution to attachment theory (see Chapter 3). Her empirical as well as her theoretical contributions were of great importance to attachment theory as it evolved. Assessing Ainsworth's influence on Bowlby and attachment is outside the scope of this thesis, but is definitely the next step in further unraveling the roots of attachment theory.



*Figure 8. Mary Ainsworth and John Bowlby in Charlottesville in 1986. Picture courtesy of the Wellcome Library, London (AMWL: PP/BOW/L.19, nr. 23).*

### **Bowlby's scientific descent: Freudian or Darwinian?**

Finally, we will here address the issue of Bowlby's scientific descent. Experts of attachment theory differ in opinion whether attachment theory is a psychoanalytic (e.g., Fonagy, 1999; Slade, 1999) or an evolutionary theory (e.g., Belsky, 1999; Simpson, 1999). Bowlby himself stated that his "own position, regarding Freud's work, is that the phenomenons to which [Freud] called attention are immensely important; but the theories which he came up with are

very dated and inadequate” (Bowlby et al., 1986, p. 45). What Bowlby was trying to do was to get psychoanalysts on a more empirical track, to ask them not to neglect real-life experiences, and to gather evidence for their views: “If they come up with some interesting and hard data... I shall be interested, but not until then” (Warne, Bowlby, Crowcroft & Rae-Grant, 1980). Bowlby’s “main concern right back from the thirties ha[d] been to get psychoanalysis onto a decent scientific basis” (Dinnage, 1979, p. 325). Bowlby specifically stated that he “didn’t happen to go along with the particular theory that Freud had advanced – not because [Freud] was convinced of it, but because [Freud] couldn’t think of anything better” (Bowlby et al., 1986, p. 42). So, Bowlby concluded that “as long as you define psychoanalysis in terms of traditional theories, i.e. Freud’s theories and not the phenomena he was trying to explain, (a) my ideas are not psychoanalysis; and (b) no new ideas can ever be psychoanalysis – by definition” (ibid., p. 57). “If psychoanalysis is to attain full status as one of the behavioural sciences,” Bowlby (1969/1982, p. 9) added, “it must add to its traditional method the tried methods of the natural sciences.”

This methodological discussion on what ‘real science’ is, of course, is not unique. In the Netherlands a similar debate in psychology raged during the 1940s and 1950s. In her description of this episode in the history of Dutch psychology, Dehue (1995) “convincingly demonstrates the broad applicability of her Dutch perspective” (Nicholson, 2000, p. 212). In the UK, Bowlby was involved in the debate between adherents of an intuitive, hypothetical approach with emphasis on the unconscious and psychoanalytic interpretation versus advocates of an empirical-analytical approach with emphasis on real-life experiences and observable data. We have made clear that Bowlby chose the latter approach and wanted psychoanalysts to make the move to a more scientific, empirically based approach to psychology and the study of personality development.

However, it is not totally fair to say that Bowlby refuted all of psychoanalysis. On several occasions Bowlby testified that he never discarded psychoanalytic theory, but that he only wanted to “rewrite psychoanalysis in the light of ethological concepts” (Dinnage, 1979, p. 325). Maybe this was because he thought that “the phenomena of psychoanalysis are far too important to be left to the psychoanalytic *movement*” (Bowlby et al., 1986, p. 57; original italics). To this day, some experts of attachment theory emphasize “the psychoanalytic core of attachment theory: that part of attachment theory that sees emotion regulation as central to healthy development” (Steele, personal communication, October 12, 2007). Others, however, state that “it was John Bowlby’s great merit to include the evolutionary basis of attachment into his framework of thinking” (Keller, 2008).

The most striking observation in this respect is one of Bowlby’s own. As early as 1958, in one of the many letters Bowlby wrote to his wife Ursula over the years, Bowlby compared himself to both Freud and Darwin:

It pleases me to believe I have some of Darwin’s characteristics, tho[ugh] by no means all! He was a tremendously good observer [and] of course a full-time research worker all his life. I’m not that good an observer [and] very far from full-time. However I believe I have some of his capacity to live with a problem over many years, mulling over the data until the theory begins to take shape [and] also

keeping the theory close to the data. None of this vicious speculation! But broad bold theory when it comes. Two other characteristics I'm pleased to share with him – very systematic note making [and] drafting at top speed with plenty of revisions later. So however good a scientist I may or may not be, I think I'm the same sort of scientist as Darwin – [and] not the least like Freud. (Bowlby in a letter to Ursula, May 19, 1958; AMWL, PP/BOW/B.1/20) (original underlining)

With these perceptive comments Bowlby not only expressed his great admiration for Darwin – an admiration that becomes clear from his biography of Darwin as well (Bowlby, 1990) – but Bowlby also stated that scientifically speaking he placed himself in the tradition of Darwin. Careful observation, note making, and theory formation on the basis of hard data was to be preferred over facile armchair speculation. It was Bowlby's passion to understand and help children that suffered from a lack of love and understanding, a passion that originated in his own childhood (Van Dijken, 1998). He was a skilled and amiable clinician who saw countless children in the Tavistock Clinic and the London Childhood Guidance Centre and was committed to help them by all possible means. But it was his ultimate belief that these children would be helped best not by speculating about their internal conflicts and drives, but by observing and explaining their behavior in their real-life circumstances in the spirit of ethology.



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## **SAMENVATTING (SUMMARY IN DUTCH)**



### **Op zoek naar de wortels van de gehechtheidstheorie**

De Britse kinderpsychiater John Bowlby formuleerde in de jaren vijftig en zestig van de twintigste eeuw voor het eerst wat nu bekend staat als de gehechtheidstheorie. Deze theorie, waarin een verklaring wordt gegeven voor het bestaan en ontstaan van de band die kinderen met hun ouders of verzorgers aangaan, is tot op de dag van vandaag zeer invloedrijk. Bowlby stelde dat jonge kinderen, die om wat voor reden ook worden gescheiden van de moeder, hiervan in sommige gevallen blijvende gevolgen ondervinden. In een eerdere studie concludeerde Van Dijken dat de wortels van Bowlby's interesse in scheidingservaringen lagen in zijn eigen kindertijd en ervaringen die hij opdeed toen hij kort voor de Tweede Wereldoorlog zijn psychoanalytische training ontving van Melanie Klein. Waar Klein en andere psychoanalytici vooral vertrouwden op de inhoud van analytische sessie en het belang van de on(der)bewuste fantasie van een patiënt benadrukten, zag Bowlby vooral het belang van observatie en de invloed van de omgeving van het kind. In zijn beschrijving van de gevolgen van scheiding steunde Bowlby volgens Van Dijken sterk op ethologische inzichten en het pionierswerk van Mary Ainsworth. In deze dissertatie wordt voortgebouwd op de bevindingen uit de dissertatie van Van Dijken en worden de ethologische inzichten die Bowlby's kijk op de moeder-kindrelatie verrijkten nader beschreven. Van Dijken eindigde haar bijdrage over de "vroege" Bowlby's met de publicatie van de gezaghebbende monografie voor de Wereldgezondheidsorganisatie WHO in 1951; dit rapport is tevens het startpunt van de huidige studie. Hier volgt eerst een beschrijving van de door Bowlby gerapporteerde gegevens over de stand van zaken in het onderzoek naar de gevolgen van scheiding van moeder en kind.

### **Onverklaarde verschijnselen: verschillende vormen van scheidingservaringen**

De Tweede Wereldoorlog gaf psychologen en psychiaters menige mogelijkheid om de gevolgen van scheiding van moeder en kind te observeren. Al in de jaren twintig had de Britse regering plannen laten maken voor grootschalige evacuatie van kinderen in oorlogstijd. Vanaf september 1939 werden in korte tijd ongeveer 750.000 kinderen (zonder hun ouders) van Londen naar het platteland geëvacueerd. In eerste instantie berichtte men in toonaangevende medische tijdschriften over een zeer succesvol verlopen operatie, maar al snel volgde andere berichten. Bowlby schreef, samen met collega's Winnicott en Miller, een ingezonden brief aan de *British Medical Journal* waarin hij protest aantekende tegen de evacuatie, omdat deze voor het welzijn van de kinderen grote gevolgen zou hebben en bovendien op termijn zou zorgen voor wijdverbreide psychologische problemen.

Terwijl veel kinderen door de evacuaties werden ondergebracht bij gezinnen op het platteland, werden anderen in kindertehuizen geplaatst, bijvoorbeeld omdat hun ouders bij een bombardement om het leven waren gekomen. Een autoriteit op dit gebied was Sigmunds dochter Anna Freud, die samen met Dorothy Burlingham verschillende boeken publiceerde over de ervaringen van jonge kinderen in de kindertehuizen. De vaak ontroerende beschrijvingen maakten duidelijk dat de kinderen ondanks de goede lichamelijke verzorging wegwijnden. Burlingham en Freud benadrukten het belang van een stabiele relatie met een volwassene. Wanneer die relatie er niet was, zou dit gevolgen

hebben voor de ontwikkeling en persoonlijkheid van het kind en zou het kind onaangepast raken aan de maatschappij.

Een derde vorm van scheiding, die van kinderen opgenomen in ziekenhuizen, leidde in de jaren veertig en vijftig tot heftige debatten in de medische wereld. Het was tot die tijd algemeen gebruik dat kinderen niet of hooguit eenmaal per week werden bezocht door hun ouders. Bowlby mengde zich ook in dit debat: in een brief aan *The Lancet* beargumenteerde hij dat er redenen waren aan te nemen dat ziekenhuisbezoek van ouders essentieel was, zeker voor jongere kinderen. Hij vermoedde dat het verbieden van bezoek tot chronisch delinquent gedrag zou kunnen leiden en noemde voorbeelden van een antisociale jongen van zes en een stelend meisje van acht uit zijn praktijk, beiden met een geschiedenis van ziekenhuisopname zonder ouderbezoek. Bowlby kreeg aanvankelijk weinig steun voor zijn ideeën. De meerderheid van de ziekenhuizen was ernstig gekant tegen bezoek van ouders.

Natuurlijk zagen enkele doktoren het probleem wel. Het vroege werk van onder meer het echtpaar Henry en Cecile Pickerill in Nieuw-Zeeland en in Engeland van James Spence en van Beryce Maclennan was belangrijk en invloedrijk, omdat het liet zien dat er alternatieven waren voor de behandeling van kinderen in ziekenhuizen. Maar hun argumenten resulteerden niet in wijzigingen in ziekenhuisbeleid en slechts zelden in verruimde bezoektijden. Uiteindelijk zouden de meeste ogen pas worden geopend voor de gevoelens van de angstige patiëntjes door harde beschrijvingen en veelzeggende filmbeelden (bv. van René Spitz en James Robertson). Beroemd is in dit verband de lezing van Spence over de zorg voor kinderen in ziekenhuizen. De schrijnende situatie op de kinderafdelingen in ziekenhuizen was Bowlby een doorn in het oog. Het ontbrak hem op dat moment echter aan voldoende bewijzen om beleidsmakers te overtuigen van hun ongelijk.

De eerste systematische aanwijzingen dat scheiding van de ouders potentieel schadelijk kon zijn, kwamen van klinische studies en observaties van kinderen tijdens bezoeken aan zogenaamde *Child Guidance Clinics*. Psychoanalytici, psychiaters en psychologen stelden op basis van deze ervaringen met jonge patiënten dat elk kind behoefte had aan liefde en veiligheid en dat die behoefte vooral door de moeder kan worden vervuld. Bowlby zelf deed retrospectief onderzoek naar jeugdige delinquenten en kwam in deze studie tot de conclusie dat langdurige scheiding van de moeder in de eerste vijf levensjaren schadelijke gevolgen kon hebben, in het bijzonder antisociaal en delinquent gedrag.

Ten slotte werd er in deze periode voor het eerst gerapporteerd over het zogenaamde hospitalisatie-effect. Zeer invloedrijk op dit gebied zijn de studies van Harry Bakwin, William Goldfarb en Spitz. Bakwin beschreef in een studie de zorg voor jonge kinderen in een New Yorks ziekenhuis. De kinderen werden in afgesloten ruimten verzorgd zonder bezoek van de ouders te ontvangen. Ondanks deze maatregelen om infectie tegen te gaan, bleef de sterfte in het ziekenhuis hoog. Bakwin verklaarde dit door de psychologische verwaarlozing van de kinderen in de steriele omgeving. Goldfarb vergeleek in zijn onderzoek naar gedragsproblemen van adolescente pleegkinderen een groep die in de eerste drie levensjaren was opgegroeid bij een pleeggezin met een groep die in de eerste drie jaar in een kindertehuis was opgevangen. Zoals Goldfarb vermoedde had het

gebrek aan sociale interactie in de kindertehuizen gezorgd voor de problemen op latere leeftijd en hij concludeerde dat een gezonde interactie tussen kinderen en hun verzorgers van het grootste belang was. Spitz, ten slotte, kwam tot vergelijkbare conclusies in zijn studie naar de zorg voor kinderen tot een jaar oud. Spitz stelde dat een affectieve relatie belangrijk is voor de fysieke ontwikkeling en het gedrag van jonge kinderen en dat een tekort hieraan negatieve gevolgen heeft voor de ontwikkeling van de persoonlijkheid.

In deze omstandigheden verscheen in 1951 Bowlby's WHO-rapport. Bowlby schreef zijn rapport over deprivatie van de moeder na experts in verschillende Europese landen en de VS te hebben geconsulteerd (o.a. Goldfarb, Spitz). De verzamelde gegevens gaven een duidelijk beeld van de ernst van de situatie: wanneer kinderen van hun ouders worden gescheiden heeft dit grote gevolgen voor hun verdere ontwikkeling. Het probleem waar Bowlby mee worstelde was dat er wat hem betreft geen afdoende verklaring was voor de onderliggende mechanismen. Waarom ondervonden deze kinderen zulke ernstige gevolgen van de scheiding van hun moeder? Het was in deze periode dat Bowlby voor het eerst werd gewezen op een nieuw opkomende discipline: de ethologie.

### **Ethologie als nieuw theoretisch raamwerk: Lorenz, Tinbergen en Hinde**

In 1973 werd de *Nobelprijs voor de Fysiologie of Geneeskunde* uitgereikt aan drie gedragsonderzoekers, onder wie de Duitse zoöloog Konrad Lorenz en de Nederlandse bioloog Niko Tinbergen. Zij hadden sinds de jaren dertig van de twintigste eeuw gewerkt aan de formulering van de ethologie, een nieuwe discipline binnen de biologie die zich concentreerde op het observeerbare gedrag van dieren. Lorenz verwierf grote faam met zijn beschrijving van concept "inprenting": het fenomeen dat jonge ganzen en kauwen direct na de geboorte het eerste, bewegende object in hun omgeving gaan volgen. Tinbergen is vooral bekend geworden door het formuleren van vier vragen die altijd zouden moeten worden gesteld bij ethologisch onderzoek: de vragen naar de oorzaak, de oorsprong, de functie en de evolutionaire ontwikkeling van gedrag. Volgens ethologen moet het gedrag van dieren op dezelfde systematische manier worden bestudeerd als hun morfologie. Bovendien leggen zij de nadruk op het observeren van het gedrag van dieren in hun natuurlijke omgeving.

In 1951, het jaar dat Bowlby's WHO-rapport verscheen, werd hij voor het eerst gewezen op Lorenz' *Der Kumpan in der Umwelt des Vogels*, dat hij later schaarde onder de elf studies die zijn denken het meest hebben beïnvloed. Bowlby raakte gefascineerd en verdiepte zich vervolgens in de beschikbare ethologische literatuur, waaronder werk van Julian Huxley, Lorenz en Tinbergen. In de jaren vijftig had hij enkele ontmoetingen met Lorenz en op Bowlby's verzoek werd Lorenz uitgenodigd voor een studiegroep van de WHO over de "psychobiologische ontwikkeling van kinderen". Bovendien bezochten zij elkaars laboratorium enkele malen voor verdere discussie. Via Lorenz zou Bowlby in 1954 in contact komen met de Britse etholoog Robert Hinde, die Bowlby's mentor op het gebied van de ethologie zou worden. Vanaf het eerste moment woonde Hinde de wekelijkse interdisciplinaire bijeenkomsten van het Tavistock Instituut bij. Zowel Bowlby als Hinde heeft het grote belang van deze samenwerking onderkend en benadrukt. Onder invloed van Bowlby's ideeën en inzichten veranderde Hinde eind jaren vijftig zijn onderzoeksveld van

gedrag bij vogels naar observaties van apengedrag en nog later van menselijk gedrag. Met Tinbergen had Bowlby in eerste instantie vooral persoonlijk contact. Zo behandelde Bowlby – als psychiater – zowel Tinbergen als één van zijn kinderen. Tinbergen zou later benadrukken dat hij graag ook op theoretisch vlak meer voor Bowlby had willen betekenen, zoals Hinde dat had gedaan.

Bowlby achtte de methoden en theorieën van de ethologie zeer bruikbaar voor het verklaren van de gevolgen van de scheidingservaringen die hij eerder had beschreven in zijn rapport voor de WHO. Het theoretisch kader dat de ethologie Bowlby bood, leidde in 1958 tot de publicatie van een zeer invloedrijk artikel over *The nature of the child's tie to his mother*. In dit artikel betoogde Bowlby dat de band tussen moeder en kind niet het gevolg was van het feit dat de moeder het kind voedt, zoals psychoanalytici en leertheoretici beweerden. Volgens Bowlby bestond gehechtheidsgedrag uit vijf verschillende gedragingen: zuigen, zich vastklampen, volgen, huilen en lachen. Deze gedragingen zouden volgens Bowlby aangeboren zijn en konden worden verklaard vanuit evolutionair perspectief: het gehechtheidsgedrag zou de overleving van de soort bevorderen. Bowlby introduceerde ethologisch geïnspireerde concepten als basis voor een theoretische verklaring van de moeder-kindrelatie. Bowlby baseerde deze verklaring mede op zijn natuurlijke observatie van kinderen, maar hij ontbeerde vooralsnog een empirische ondersteuning van zijn theoretische ideeën.

### **Empirische ondersteuning: het belang van Harry Harlows werk met resusapen**

Empirische ondersteuning van zijn ideeën zou Bowlby vinden bij de Amerikaanse dierpsycholoog Harry Harlow. Tijdens een bijeenkomst van Europese ethologen en Amerikaanse dierpsychologen in Stanford in 1957, ontmoetten Hinde en Harlow elkaar voor het eerst en na terugkeer in Engeland lichtte Hinde Bowlby in over Harlows interesse in zijn werk. Kort daarna, in augustus 1957, werd het eerste contact door Bowlby via een brief gelegd. Op basis van de Harlows en Bowlby's wetenschappelijke publicaties en de correspondentie tussen beiden is een analyse uitgevoerd van de wederzijdse beïnvloeding van hun werk.

In Harlows baanbrekende en historisch belangrijke experimenten werden jonge resusapen direct na de geboorte van hun moeders gescheiden en geplaatst bij twee surrogaatmoeders: één van ijzerdraad en één van badstof. Harlow toonde aan dat de resusapen veel meer tijd doorbrachten op de zachte badstofmoeder ongeacht welke van de twee moeders zorgde voor de voeding. Bovendien vonden resusapen die alleen met een ijzerdraadmoeder opgroeiden in angstige situaties geen steun bij hun surrogaatmoeder, terwijl aapjes die opgroeiden bij een badstofmoeder dat wel vonden. Met zijn onderzoek toonde Harlow aan dat voeding geen doorslaggevende rol speelt bij het tot stand komen van de band tussen moeder en kind. Het onderzoek van Harlow was, naast het pionierswerk van Mary Ainsworth in Uganda, eind jaren vijftig een eerste empirische ondersteuning van Bowlby's ideeën over gehechtheid.

Niet alleen was het werk van Harlow belangrijk voor Bowlby, ook Bowlby's ideeën hebben Harlow sterk beïnvloed. In enkele experimenten waarbij resusapen direct na geboorte werden gescheiden van de moeder, vond Harlow dezelfde reacties die Bowlby

eerder samen met Robertson bij jonge kinderen die van hun moeder waren gescheiden had gevonden: de fasen van protest, wanhoop en onthechting. In het voorliggend onderzoek is geconcludeerd dat Harlow en Bowlby tegelijkertijd, maar onafhankelijk van elkaar, tot vergelijkbare inzichten kwamen. De interactie die daarop volgde is een zeer duidelijk voorbeeld van de kruisbestuiving tussen de ethologie en de gehechtheidstheorie.

Tot slot werd in deze dissertatie de kruisbestuiving in de laatste jaren verder geïllustreerd in een interview met een vooraanstaande wetenschapper uit het veld: dr. Stephen J. Suomi. In het interview beschrijft Suomi zijn persoonlijke en wetenschappelijke relatie met zowel Harlow als Bowlby. Verder reflecteert Suomi op de kruisbestuiving van gehechtheid en ethologie en de wederzijdse beïnvloeding van Bowlby en Harlow. Aandacht is er ook voor de psychoanalytische wortels van de gehechtheidstheorie en de ethologische concepten in Bowlby's werk.

### **Conclusies**

In deze dissertatie is op basis van een analyse van Bowlby's wetenschappelijke publicaties, enkele interviews met direct betrokkenen en uniek archiefmateriaal de kruisbestuiving van de gehechtheidstheorie en de ethologie onderzocht. Ten tijde van de publicatie van zijn WHO-rapport was Bowlby ontevreden over de vigerende opvattingen over en psychoanalytische verklaring voor de observaties van de gevolgen van scheiding van jonge kinderen. Toen Bowlby's aandacht werd gevestigd op de ethologie zag hij direct het potentieel van deze nieuwe discipline en begon hij de psychoanalyse te herschrijven in het licht van de ethologie. Dit nieuwe theoretisch raamwerk bepaalde in grote mate Bowlby's verklaring van gehechtheidsgedrag. Daarnaast werden Bowlby's theoretische ideeën ondersteund door het empirische onderzoek van Harlow. De invloed van Lorenz, Tinbergen, Hinde en Harlow was niet eenzijdig: zij werden op hun beurt in hoge mate beïnvloed door het theoretische werk van Bowlby. Hinde verschoof zijn aandacht naar observaties van en onderzoek naar gedrag bij apen en mensen; Tinbergen verdiepte zich onder invloed van Bowlby in een ethologische behandeling van autistische kinderen; en Harlow baseerde zijn experimenten naar totale separatie bij resusapen op Bowlby's beschrijving van de fasen van protest, wanhoop en onthechting bij kinderen.

Door zijn nieuwe ethologische inzichten raakte Bowlby nauw betrokken bij het debat tussen aanhangers van een intuïtieve, hypothetische benadering van gedrags- en persoonlijkheidsproblemen en psychoanalytische interpretatie in de klinische praktijk aan de ene kant en voorstanders van een empirisch-analytische benadering met nadruk op werkelijke, bewuste ervaringen en waarneembare feiten aan de andere kant. Hier is duidelijk gemaakt dat Bowlby de voorkeur gaf aan de laatste benadering en dat hij psychoanalytici wilde overtuigen van de noodzaak van een meer empirische aanpak voor het bestuderen van persoonlijkheidsproblemen. Toch is het niet terecht te stellen dat Bowlby de psychoanalyse in zijn geheel verwierp. Op verschillende plaatsen stelde Bowlby dat hij de psychoanalyse slechts wilde herschrijven in het licht van de ethologie. Tot op de dag van vandaag wordt door sommige gehechtheidsexperts de nadruk gelegd op de psychoanalytische wortels van de gehechtheidstheorie. Volgens anderen is het echter Bowlby's grootste verdienste dat hij koos voor een evolutionaire basis van gehechtheid.

Bowlby was een kundig en beminnelijk clinicus die ontelbare kinderen heeft behandeld in zijn kliniek en die zich inzette om hen op elke mogelijke manier te helpen. Maar hij was ervan overtuigd dat deze kinderen het meest gebaat waren bij een zorgvuldige observatie en verklaring van hun gedrag geplaatst in de context van hun omgeving, geheel in de geest van de ethologische traditie.

## **DANKWOORD (ACKNOWLEDGEMENTS)**



Uit wetenschappelijk onderzoek blijkt dat het dankwoord van een proefschrift voor de auteur – behalve de plaats om oprechte dankbaarheid te tonen – vooral de mogelijkheid biedt “de eigen wetenschappelijke identiteit te bevorderen en te tonen dat men tot een netwerk van deskundigen is toegetreten”.<sup>1</sup> Om te beginnen met dat laatste.

*I would like to thank all the interviewees who were willing to answer my ignorant questions in unnecessarily long interviews. The useful information they nevertheless provided, their insightful views, and their critical comments, hopefully made what I have reported here noteworthy. Many took the time and effort to host me, for which I am very grateful. I am indebted to the staff of the Wellcome Institute in London who was of great assistance during my visits and supplied me with the materials of the Bowlby archives.*

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Dat wat betreft mijn veronderstelde wetenschappelijke identiteit en vermeende deskundigheid. Zoals gezegd is dit deel van het proefschrift ook de plek om oprechte dankbaarheid te tonen. Familie en vrienden die in de afgelopen jaren op wat voor wijze dan ook interesse hebben getoond in wat ik deed, bedankt! Pap en mam, jullie zijn er altijd voor me, het lijkt zo vanzelfsprekend, maar dat is het niet. Als laatste bedankt ik jou, Fran, gewoon om wie je bent en wat je voor me betekent.

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Frank van der Horst, oktober 2008

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<sup>1</sup> Hyland, K. (2003). Dissertation acknowledgements. The anatomy of a Cinderella Genre. *Written communication*, 20 (3), 242-268; Hyland, K. (2004). Graduates' gratitude: the generic structure of dissertation acknowledgements. *English for Specific Purposes*, 23, 303-324; Hyland, K., & Tse, P. (2004). “I would like to thank my supervisor”. Acknowledgements in graduate dissertations. *International Journal of Applied Linguistics*, 14 (2), 259-274.



# **CURRICULUM VITAE**



Frank van der Horst werd geboren op 5 augustus 1977 te Delft. Van 1989 tot 1995 bezocht hij het gymnasium aan het Christelijk Lyceum aldaar. Vervolgens studeerde hij tussen 1995 en 2000 geschiedenis aan de Universiteit Leiden en specialiseerde zich in de Vaderlandse geschiedenis. Zijn scriptie behelsde een biografie van de achttiende eeuwse marinekapitein en -hervormer Hendrik August baron van Kinckel. Aansluitend studeerde Frank van 2000 tot 2005 Pedagogische Wetenschappen aan dezelfde Leidse instelling en koos voor de richting gezinspedagogiek. In de afrondende scriptie werden de ethologische noties in Bowlby's trilogie *Attachment and loss* beschreven. In de periode 2004-2009 werkte Frank als promovendus op de afdeling Algemene en Gezinspedagogiek-Datatheorie (AGP-D) en deed onderzoek naar de wederzijdse beïnvloeding van ethologie en gehechtheidstheorie. De resultaten van het onderzoek zijn beschreven in dit proefschrift. Naast het promotietraject was hij in deze periode voor één dag per week aangesteld als docent en gaf onderwijs in alle fasen van de opleiding Pedagogiek.

Frank van der Horst was born on August 5, 1977 in Delft, the Netherlands. In 1995 he completed his secondary education at the Christelijk Lyceum. Subsequently, he studied Dutch maritime history at Leiden University (1995-2000). His graduate thesis was a biography of eighteenth century naval officer and reformer Hendrik August baron van Kinckel. Next Frank studied Education and Child studies at the same university and specialised in Child and Family Studies (2000-2005). In his graduate thesis he described the ethological notions in Bowlby's trilogy *Attachment and loss*. From 2004 to 2009 Frank was a PhD-student at the Centre for Child and Family Studies and conducted research on the roots of attachment theory. The results of this research are described in the present thesis. While working as a PhD-student, Frank was also appointed lecturer for one day a week and took part in different courses for both undergraduates and graduate students.

