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

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## **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 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/BOW/L.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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