John Bowlby and ethology : a study of cross-fertilization

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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.

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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)[15] 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.[16] 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

[15] 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".

[16] 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.[17] 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.

[17] 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[18] 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

[18] 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[19] 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 fashioned with his monkeys. For

[19] 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 like. 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 learn 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 you *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 going 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 carrying 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 matrilines. 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),[20] 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.”[21] 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

[20] 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).

[21] 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 is 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.