

The Strange Situation of the Ethological Theory of Attachment

A Historical Perspective

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Abstract

This chapter examines the history of some challenges to John Bowlby's and Mary Ainsworth's ethological attachment theory (EAT). Bowlby and Ainsworth argued that the mother-infant relationship is a natural dyad designed by evolution in which the instinctual responses of one party activate instinctual responses in the other, and that secure attachment is an adaptation. This chapter focuses on EAT's two fundamental tenets: the universality of attachment patterns and the biological foundations of the attachment system. It shows that several scholars have challenged those tenets over the years and argues that attachment researchers have not addressed those challenges successfully.

Introduction

Commissioned by the World Health Organization (WHO) to write a report about the effects of maternal separation, British psychiatrist and psychoanalyst John Bowlby published *Maternal Care and Mental Health* (Bowlby 1951), followed two years later by its popular version, *Child Care and the Growth of Love* (Bowlby 1953). Bowlby argued that maternal care and love are essential for a child's psychological development. Although highly influential, Bowlby's conclusions also incited great controversy. In 1962, the WHO published a new

report with the revealing title *Deprivation of Maternal Care: A Reassessment of Its Effects* (World Health Organization 1962). In this report a number of scholars criticized Bowlby mainly for unduly extrapolating from observations of sick children in hospitals and children in severely deprived conditions to infants growing up in standard circumstances. By that time, however, Bowlby had turned to biology to ground his views about the determinant role of maternal care.

In his 1958 paper, “The Nature of the Child’s Tie to His Mother,” Bowlby introduced his ethological attachment theory (EAT), which he later expanded in his 1969 book *Attachment*. According to this theory, natural selection designed a system to attach infants to their mothers, and a correct integration of this mother-infant dyad is necessary for a child’s adequate emotional development. North American psychologist Mary Ainsworth and some of her students presented observational and experimental work in support of this theory (Ainsworth 1967; Ainsworth et al. 1978).

Since then, EAT has become one of the most prominent and influential theories of child development. It has informed a wide range of policies and practices in education, law, and child care (see Chapters 13 and 14, this volume). It has also expanded into a theory of personality, with scholars analyzing attachment issues in practically all of an individual’s relationships.

Since its inception, EAT has also received serious criticisms. However, this important point about its contested status is little known because most historical presentations to date are celebratory accounts written by attachment theorists or supporters (Ainsworth and Bowlby 1991; Bretherton 1991, 1992; van der Horst 2011), or by others who rely on their accounts (Karen 1998).

My own historical examination of post-World War II American views about children’s emotional needs has revealed that scholars in a variety of fields raised a series of powerful criticisms of attachment theory that have not been adequately addressed to this day (Vicedo 2013). According to Bowlby, the Austrian ethologist Konrad Lorenz’s studies of imprinting, the experiments with rhesus monkeys conducted by the American psychologist Harry Harlow and British primatologist Robert Hinde, as well as the observations of children by British social worker James Robertson and North American psychologist Mary Ainsworth all supported his ethological attachment theory. But I have shown that this was not so (Vicedo 2013). Comparative psychologists and animal researchers convincingly disproved many of Lorenz’s assertions about imprinting and successfully challenged his conception of instincts. Harlow emphasized the existence of different affectional systems and showed that the lack of maternal care alone did not cause irreversible effects. Hinde highlighted the need to understand mother-infant relations in the context of family and social relations in each primate group and showed that the effects of early separations per se do not determine the infant’s future behavior. Robertson denied that his observations in hospitals supported Bowlby’s views, and he opposed Bowlby’s extrapolation from cases of long

separations in hospitals to the context of everyday child-rearing practices. Several psychologists and psychoanalysts, including Anna Freud, also criticized Bowlby's use of data about children suffering from profound and extended deprivations to explain development in standard circumstances (for references, see Vicedo 2013). Each of these criticisms is significant on its own. Taken together, they amount to a serious challenge to EAT's standing as good science.

In my view, a combination of social factors and methodological/rhetorical strategies explains the great appeal of Bowlby's theory in the American context. In the United States, scientific and social interest in the development of emotions grew considerably after World War II. Specifically, the question of the effects of maternal separation on a child's emotional development acquired poignant relevance in discussions about the consequences of women's increasing presence in the workforce. In the context of Cold War debates about gender roles and working mothers, Bowlby's views helped justify the patriarchal family with its separate, clearly defined parental roles. In addition, Bowlby and Ainsworth used several strategies which helped boost their theory's popularity: they presented a united front despite their differences; they cited mostly work that supported their views; and they repeatedly claimed that biological science validated their theory.

My book, *The Nature and Nurture of Love* (Vicedo 2013), focuses on the use of ethological ideas to support attachment theory until the late 1970s. In this chapter, I examine another fertile source of criticisms not addressed in my book: the challenges to the theory from cross-cultural studies of childhood and child-rearing conducted by cultural anthropologists. This line of criticism goes back to the 1950s and has acquired impressive momentum since the 1990s. I will argue that cultural psychologists and anthropologists have presented a powerful challenge to the uniformity of attachment behaviors and the normativity of secure attachments. In addition, since attachment researchers appeal to the biological basis of attachment to defend the universality of attachment behaviors, I analyze EAT's evolutionary framework in Bowlby's work and in the work of his followers. I argue that, in fact, there is no adequate scientific foundation for that appeal.

Recently, social anthropologist Sara Harkness (2015:196) wrote:

...attachment research at present finds itself in a strange situation, still committed to a vision of infant-caregiver relationships based on a (probably idealized) model of middle-class Anglo-American family life in the 1950s that does not correspond to the circumstances of care for most of the world's babies.

By exploring the challenges to EAT from cultural anthropology and psychology, and the shortcomings in EAT's evolutionary framework, I aim to illuminate in this chapter how EAT came to find itself in that strange situation.

Mead: Challenging the Focus on the Mother-Infant Dyad

The famous American anthropologist Margaret Mead launched the first challenge to Bowlby's universalistic claims. As I will show below, neither Bowlby nor Ainsworth took this challenge seriously.

As one of the first anthropologists to make childhood central to ethnographic studies, Mead became a leader of the Culture and Personality Studies movement. This movement, which reached its most fertile years in the 1930s and 1940s, called for an interdisciplinary effort to understand child-rearing and its role in different societies. Mead shared the conviction that childhood studies required input from diverse fields and areas of expertise (Mead 1955). Among the many efforts to move in this direction, the Six Cultures Study of Socialization (SCSS) deserves special mention. Initiated in 1954 by Harvard anthropologist John W. M. Whiting, with the collaboration of Yale psychologists Irvin L. Child and William W. Lambert, SCSS included studies carried out in Mexico, New England, the Philippines, India, and southwestern Kenya (Whiting and Whiting 1975; LeVine 2001, 2007, 2010).

Mead welcomed the post-World War II surge of interest in child development, but worried because social prescriptions and policies were being justified by ideas that lacked adequate support (Mead 1954:474). In a review of the work done from the mid-1940s to the mid-1950s, including Bowlby's influential 1951 report for the WHO, Mead pointed out two problematic trends: the inflated statements about the role of any single factor in child-rearing, and the "exaggerated and poorly supported claims of the importance of the mother as a single figure in the infant's life" (Mead 1954:476).

Like Bowlby, Mead was interested in the biological aspects of caretaking, but she was wary of possible misconceptions. She noted that Lorenz's and Tinbergen's ethological work opened the "possibility of tackling from a new point of view...the whole question of the instinctual elements in parent-child relationships" (Mead 1954:476). However, she also called attention to an important confusion:

At present, the specific biological situation of the continuing relationship of the child to its biological mother and its need for care by human beings are being hopelessly confused in the growing insistence that child and biological mother, or mother surrogate, must never be separated, that all separation even for a few days is inevitably damaging, and that if long enough it does irreversible damage.

In her opinion, there was no anthropological evidence to support "the value of such an accentuation of *the tie between mother and child*." In fact, cross-cultural studies suggested that "adjustment is most facilitated if the child is cared for by many warm, friendly people" (Mead 1954:477, italics added for emphasis).

Bowlby did not view caretaking that way, as he made clear in the title of his 1958 paper "The Nature of the Child's Tie to His Mother." In this first

presentation of EAT, he appealed to biology to defend the instinctual nature of the ties between mother and child. Further, he argued that without a proper functioning of those ties, children would develop emotional pathologies. Bowlby also defended the child's need for monotropy, the existence of a hierarchy in attachments, with the infant's main attachment centered on a single figure, usually the biological mother (Bowlby 1958). He did not include, however, information about mother-infant relations in different cultures.

In 1962, when the WHO published an evaluation of the conclusions of Bowlby's 1951 report, Mead noted again in her contribution the ethnocentric character of Bowlby's ideas. She reiterated her view that studies of other societies showed that a large number of nurturing figures could provide the security children need for healthy emotional development. She concluded that by giving the mores of his own culture the status of universal behavior and by positing a biological underpinning, Bowlby had committed the sin of "reification." He had taken "a set of ethnocentric observations on our own society, combined with assumptions of biological requirements which are incompatible with *Homo sapiens*," and turned them into "a set of universals" (Mead 1962:58). No doubt, these were serious charges.

Bowlby left the task of responding to his critics to Mary Ainsworth. An American-Canadian psychologist, Ainsworth had worked in Bowlby's group in London before moving to Uganda in 1954 and again, a year later, to the United States. At the time, she was planning to write a book with Bowlby and James Robertson. In writing her response for the WHO 1962 volume, Ainsworth consulted with Bowlby and followed his suggestions (Vicedo 2013).

In her response to Mead, Ainsworth claimed that Mead had misunderstood Bowlby. The view that Bowlby sponsored "an exclusive mother-child pair as the ideal" was a misunderstanding for several reasons:

1. Bowlby "argued for the desirability of a major mother-figure, not necessarily the biological mother, whose care is supplemented by other figures, including a father-figure."
2. Dispersion of maternal care was "not likely to be the norm in any primitive society."
3. "It seems entirely likely that the infant himself is innately monotropic."

So, "a situation (whether brought about by an 'experimental society' or through some individual variation in a traditional society) which impedes monotropic attachment will distort the normal course of development" (Ainsworth 1962:146–147). In this strange text, Ainsworth first denied that Bowlby defended monotropy, but then went on to argue that monotropy was probably the norm in primitive societies, that it was probably innate, and that departures from it would lead to pathology. Thus, after claiming that Mead's critique of Bowlby rested on the false assumption that he supported monotropy, Ainsworth literally presented a defense of monotropy rather than a rejection.

Ainsworth never addressed Mead's challenge seriously. The limited nature of Ainsworth's own research, which became a major reference point in the attachment literature, is telling. During her 1954–1955 stay in Uganda, Ainsworth conducted a study of the relationships between mothers and their infant children in that culture. She followed the first fifteen months of life of 28 babies during home visits to about 20 Ganda households, observing mother-infant interactions while interviewing the mothers with the help of an interpreter. This resulted in her 1967 book *Infancy in Uganda*. She focused on three basic aspects of an infant's behavior to assess attachment: the use of mother as a secure base for exploration; the infant's distress in brief separations from mother; and the infant's fear when encountering strangers. She classified the infants she had observed into three groups: secure-attached (16 children); insecure-attached (7 children); and non-attached (5 children). Ainsworth identified numerous factors that played a role in the development of infant-mother attachment, but she concluded that the determinant ones were the conduct and feelings of the mother, mainly her sensitivity in responding to her baby's signals (Ainsworth 1967:400). Although most of these children were taken care of by several caretakers, Ainsworth did not investigate this. Ainsworth's book was important in expanding the limited database upon which Bowlby was relying, but it did not provide an in-depth ethnographic analysis of Ganda's childcare practices. Though Ainsworth discussed Mead's views on multiple caregivers, she did not examine the cooperative caretaking of the Ganda nor did she explore the significance of the Ganda family practices in their socio-cultural context.

Bowlby did not address Mead's challenge either. In 1969, two years after the publication of Ainsworth's book, Bowlby published *Attachment* in which he mentioned the word "anthropological" only once and referred to Mead only once, in a footnote, and just to reiterate that she had misunderstood him (Bowlby 1969:303). Although he presented a theory of child development that aimed to have a universal character, Bowlby did not engage the existing anthropological literature on children from diverse cultures.¹ This did not escape attention. At least one reviewer found Bowlby's small data sample and selective referencing regarding other cultures problematic: "Of observations in non-European cultures, repeated use is made only of Ainsworth's research in Uganda" (Maas 1970:414–415).

The lack of serious engagement with Mead's points is surprising since her critique was directed at three fundamental levels: empirical, conceptual, and methodological. First, she did not think that Bowlby had sufficient evidence to claim that (a) maternal care and love were *sine qua non* for a child's emotional development and (b) separation from the mother would result in catastrophic

¹ He also made no mention of Mead in any of his other books. Failure to address Mead's critique has persisted for decades, in particular by sympathizers of Bowlby's views in their attempts to define the history of the field (Karen 1998; van der Horst 2011).

consequences for both the individual and the species. Second, she pointed out that Bowlby was mixing up two issues: child-rearing practices needed to support a specific sociocultural arrangement versus practices essential for human survival. Third, she challenged Bowlby's approach to studying children's basic emotional needs by focusing on those aspects of child-rearing practices that were important in his own society.

How was it possible to have ignored Mead's critique, given her intellectual stature in American social science? At least four factors played a role: First, in general, Bowlby did not address criticisms of his work, and neither did Ainsworth. Second, despite its influence in the postwar years, by the mid-1960s the Culture and Personality movement had practically disappeared. In addition, the lack of a unified theoretical framework and the problems of operationalizing research and translating between cultures made it difficult for the different ethnographic studies to cohere into a general account of child development (LeVine 2001). Third, the disciplinary goals of anthropology and psychology propelled studies of children in different directions. Much of psychological research has aimed to establish universal generalizations about the human mind, focusing on experimentation to reach knowledge of assumed universal capacities that explain human behavior. In contrast, anthropological studies of childhood have focused on understanding specific practices in a variety of cultures (Vicedo 2017). Fourth, ethology was successful in arguing that social behavior is a matter of instincts. In the mid- to late-1960s, biological explanations of human behavior held great appeal in the social sciences and the wider society (Degler 1991). Books such as Lorenz's *On Aggression* (Lorenz 1966) and Desmond Morris's *The Naked Ape* (Morris 1967) became instant bestsellers. In addition, other writers who were sympathetic to the aims of ethology published highly successful books on the animal roots of human conduct (Ardrey 1961, 1966; Tiger 1969). The search for the biological roots of human behavior also fueled interest in animal research. Lorenz's studies of imprinting in ducks and Harry Harlow's experiments with rhesus monkeys made it into *Life Magazine* and *The New York Times*.

Though Mead along with other anthropologists, such as Ashley Montagu (1968), remained critical of biological reductionism, biological explanations of human behavior enjoyed tremendous popularity. These conditions worked in favor of Bowlby's approach in presenting EAT as an account of universal affects and behaviors based on biological knowledge. This stood in stark contrast to anthropology's emphasis on the diversity of emotional experiences and on the complexity of studying emotions in different societies. The romance of child development with biology dates back to the origin of the field and is fraught with bitter disappointments (Morss 1990). In the wake of ethology's success and with advances in genetics and evolution grabbing the headlines, Bowlby's proposal gained momentum.

Bowlby: The Stone Age Child and the Paleo Mother

Given the centrality of Bowlby's claims about the evolutionary support for EAT and the cursory manner in which they are often presented, it is worth examining Bowlby's main ideas in detail. My analysis helps to clarify Bowlby's position on some of the points that have become controversial in the attachment literature. It also shows that Bowlby's presentation of the attachment system as an adaptation was a hypothesis for which he did not provide sufficient evidence.

In his writings from the 1950s, Bowlby imported several concepts from the discipline of ethology. Starting in the 1930s, the European animal researchers Konrad Lorenz and Niko Tinbergen developed ethology as the biological study of animal behavior and argued that much social behavior is instinctual. After World War II, ethology as a field of scientific inquiry became highly successful. For their foundational contributions, Tinbergen and Lorenz would share the Nobel Prize in Physiology or Medicine with Karl von Frisch in 1973. Bowlby interacted with Lorenz and Tinbergen at several workshops and corresponded with them regularly.

In 1958, Bowlby aimed to provide a synthesis of psychoanalytical and ethological ideas through his paper, "The Nature of the Child's Tie to His Mother." He argued that infants are born with instinctual responses which "tie" them to their mothers; the behavior of mothers, in turn, is preprogrammed to connect them to their infants. Five instinctual behaviors help the child create this bond with the mother: sucking, clinging, following, crying, and smiling. Though Bowlby did not specify which maternal behaviors are instinctual, he claimed that the proper functioning of the mother-infant tie is necessary for the adequate emotional and psychological development of the infant.

In support of his views, Bowlby appealed to the authority of biology: "The theory of Component Instinctual Responses, it is claimed, is rooted firmly in biological theory and requires no dynamic which is not plainly explicable in terms of the survival of the species" (Bowlby 1958:369). Bowlby emphasized that he was rejecting the psychoanalytic concept of instinct as energy that needs to be released and was adopting in its place the ethological concept of instinct. Lorenz had defined instincts as species-specific patterns of behavior that are innate, are the result of natural selection, and are impervious to experience. For Bowlby, the behaviors of infant toward mother and mother toward child are instances of instinctual behavior. Studying the literature on animal behavior, especially primatology, he developed these ideas further over the next ten years and presented them in his 1969 book *Attachment* (Bowlby 1969).

Attachment was the first volume of his influential trilogy on the effects of maternal deprivation (the three titles were *Attachment*, *Separation*, and *Loss*), and the major work that delineated his ethological theory of attachment. After a brief introductory discussion about his "point of view" (i.e., psychoanalysis informed by ethology), Bowlby presented the "observations to be explained"

(i.e., the data that his theory needed to explain). He listed the work of Dorothy Burlingham and Anna Freud at the Hampstead Nurseries in London, who showed that children separated from their families during World War II often regressed in their development. He also listed the work of René Spitz and Katherine Wolf, which looked at the detrimental effects of multiple caretakers for children who lived in institutions. He relied especially on the observations provided by James Robertson (Bowlby 1982:26). Robertson had documented the profound distress experienced by children separated from their families during lengthy hospital stays. It is, however, important to note that by 1969, Spitz's work had been thoroughly discredited (for a devastating critique of the work on maternal deprivation that Bowlby relied upon in his 1951 WHO report, see Pinneau 1955; Casler 1961), and Anna Freud had criticized Bowlby's extrapolations of the experiences of children during the war to the home setting in normal circumstances (Freud 1960).

The book's second part is devoted to the analysis of "instinctive behavior." Bowlby considered that some patterns of human behavior, including the care of babies and the attachment of young to parents, "seem best considered as expressions of some common plan and, since they are of obvious survival value, as instances of instinctive behaviour" (Bowlby 1969:39).

In *Attachment*, Bowlby revised his earlier views about instincts, reflecting the influence of Tinbergen and Hinde. Over the years, ethologists had modified their goals and main ideas as a result of criticisms from other researchers (Burkhardt 2005; Vicedo 2013). In 1953, American comparative psychologist Daniel Lehrman criticized Lorenz for not recognizing the significant role of development in behavior and for wrongly assuming that the inheritance of a trait implies its developmental fixity (Lehrman 1953). Hinde, a close friend of Lehrman, also criticized the ethological concept of drive (Hinde 1956). Bowlby now considered instinctive behavior to be "the result of integrated control systems operating within a certain kind of environment" (Bowlby 1969:44). He emphasized that instinctive behavior is not inherited; rather "what is inherited is a potential to develop certain sorts of system, termed here behavioural systems," that are then shaped by the particular environment of development (Bowlby 1969:45). But there are limits. And those limits are essential to an understanding of the biological and psychological consequences of changes in the environment (Bowlby 1969:46):

The recognition that behavioural equipment, like anatomical and physiological equipment, can contribute to survival and propagation only when it develops and operates within an environment that falls within prescribed limits is crucial to an understanding of both instinctive behaviour and psychopathology.

For Bowlby, there is always an environment to which a system, man-made or biological, is adapted. He called this the system's "environment of adaptedness" (Bowlby 1969:47). In a man-made system, one would refer to the system's "environment of designed adaptedness." In living organisms, one should

talk about the “environment of evolutionary adaptedness” (EEA). According to Bowlby, the EEA is different for each system in each species. To avoid confusion from the polysemy of the term “adaptation,” he proposed using this word when referring to the process and “adaptedness” when referring to the state or condition of being adapted (Bowlby 1969:51).

Bowlby also asserted that “only within its environment of adaptedness can it be expected that a system will work efficiently” (Bowlby 1969:47). To explain this, he put forth an analogy with man-made systems. These work best in the “environment” for which they were designed. For example, a small car built to run on English roads will work best there. “Whether or not it will also suit other environments, however, is unknown,” Bowlby wrote. Given the difficulty of knowing whether the car would work in the Arctic Circle or the Sahara, for example, he noted that “until it is shown to be more extended, it is wise to assume that the car’s environment of adaptedness is limited to London streets” (Bowlby 1969:52).

Focusing then on humans, Bowlby claimed that most human behaviors are adaptations resulting from selection in “man’s environment of evolutionary adaptedness” (Bowlby 1969:58). He argued that the rate of man-made environmental change had “far outstripped the pace at which natural selection is able to work.” So, he noted, one could be “fairly sure that none of the environments in which civilised, or even half-civilised, man lives today conforms to the environment in which man’s environmentally stable behavioural systems were evolved and to which they are intrinsically adapted” (Bowlby 1969:59). Bowlby concluded that “the environment in terms of which the adaptedness of man’s instinctive equipment must be considered is the one that man inhabited for two million years until changes of the past few thousand years led to the extraordinary variety of habitats he occupies today.” According to Bowlby, this original environment “presented the difficulties and hazards that acted as selective agents during the evolution of the behavioral equipment that still is man’s today. This means that man’s primeval environment is, almost certainly, also his environment of evolutionary adaptedness.” Further, he claimed that if his conclusion was correct, “*the only relevant criterion by which to consider the natural adaptedness of any particular part of present-day man’s behavioral equipment is the degree to which and the way in which it might contribute to population survival in man’s primeval environment*” (Bowlby 1969:59). In sum, human behavioral systems were “designed” in the Pleistocene by natural selection and could only be understood by knowing their function in that ancestral environment.

The question naturally follows: Does the human behavioral equipment, “designed” by natural selection in the EEA, still work in today’s environment? Bowlby said that this could not be known without empirical research. Furthermore, he claimed (Bowlby 1969:60–61) that he was not concerned with such questions:

...enormously important though they are, all questions as to whether man's present behavioural equipment is adapted to his many present-day environments, especially urban environments, are not strictly relevant to this book, which is concerned only with elemental responses originating in bygone times. What matters here is that, if man's behavioural equipment is indeed adapted to the primeval environment in which man once lived, it is only by reference to that environment that its structure can be understood...It is impossible to understand man's instinctive behaviour until we know something of the environment in which it evolved.

This is a surprising statement, given that Bowlby presented his book as an exploration of instinctive attachment behavior and its consequences for mental health.

Having established this general framework for understanding the human mind, Bowlby also translated his earlier 1958 paper into the new language of instinctual control systems. He proposed that babies and mothers are born with a repertoire of attachment behaviors that evolved through natural selection in the EEA. The infant's attachment system comprises those behaviors that lead the infant to seek proximity to the mother—behaviors that were selected by evolution because they enhanced the infant's protection from predators. In turn, the behavior of parents "that is reciprocal" to infants' attachment behavior conforms the caregiving system (Bowlby 1969:182). As noted above, in his 1958 paper Bowlby had not specified which maternal behaviors are instinctual. In 1969, his account of maternal caregiving was also underdeveloped. Even chapter thirteen, where Bowlby covered caregiving behavior, was devoted primarily to the child's behavior. The mother is portrayed as the one reacting or responding to the child's proximity-seeking actions: signaling behavior (crying, smiling, babbling) or approach behavior (approaching and following).

Strongly influenced by Lorenz's studies of imprinting, Bowlby viewed the tie between the mother and infant as an instance of that phenomenon. Imprinting is the process whereby the newborns in some species (e.g., ducks and geese) follow the first object they see upon hatching. Normally, that object would be their mother. According to Lorenz, if newborns do not follow their mother, they will not develop the social and sexual responses typical of their species in adulthood. For Lorenz, imprinting occurs during a critical period and has determinant effects on the social behavior of the animal. Other animal researchers, however, showed that imprinting is a more flexible phenomenon and that its effects can be reversed (Bateson 1966). In *Attachment*, Bowlby accepted some of the criticisms made of Lorenz's work and defended a more generic concept of imprinting. For him, this meant "the development of a clearly defined preference," which develops "fairly quickly...during a limited phase" and then remains "comparatively fixed" (Bowlby 1969:168). He asserted that the attachment between infant and mother is sufficiently similar to the process Lorenz described to be conceptualized as imprinting (Bowlby 1969:223).

In short, for Bowlby, the infant's attachment system and the caregiving system were configured by natural selection in the EEA. The child becomes

imprinted on the mother. The mother's appropriate responses to the child during a crucial period of development are necessary for the survival and proper social development of the child.

But what would happen if there was a deviation from the environment to which those systems were adapted? As noted earlier, Bowlby claimed that it is not possible to know in advance how a system would perform outside its environment of adaptedness. Now, however, he asserted that a change in environment would likely imply a malfunctioning of the system. Bowlby (1969:166) was clear about this:

...it is wise to be cautious and to assume that the more the social environment in which a human child is reared deviates from the environment of evolutionary adaptedness (which is probably father, mother, and siblings in a social environment comprising grandparents and a limited number of other known families), the greater will be the risk of his developing maladaptive patterns of social behaviour.

What constituted these "maladaptive" patterns of social behavior? For Bowlby, these were behaviors that do not promote survival and reproduction. Problems of attachment put at risk the survival of the species, although as long as the system fulfills its functions in enough individuals within a population, the species would likely survive. In addition, he suggested that at the level of individual mental health, problems of attachment would lead to individual psychopathology.

The question naturally arose: Is having an attachment to a person other than the biological mother a deviation that would lead to "maladaptive" patterns of behavior? That is, does a child need his/her biological mother (see Keller and Chaudhary, this volume)? As presented above, the question of whether Bowlby believed that a child attaches only to one person, and the question of whether that person is or should be the biological mother, became highly controversial. They remain so today. Let's address them.

First, regarding monotropy, Bowlby believed that although an infant can attach to more than one person, there is always one principal attachment figure. As he put it (Bowlby 1969:309):

Because the bias of a child to attach himself especially to one figure seems to be well established and also to have far-reaching implications for psychopathology, I believe it merits a special term. In the earlier paper I referred to it as "monotropy."

In fact, in his original definition, monotropy was "the tendency for instinctual responses to be directed toward a particular individual or group of individuals and not promiscuously toward many" (Bowlby 1958:370). Clearly that definition was self-contradictory and needed clarification. So, in 1969, Bowlby made clear that he believed the child was biased to attach mainly to one person.

Second, as for the main figure, in many, though not all, of his writings, Bowlby included a footnote stating that the word "mother" was to be understood

as “mother-figure”; that is, the person who provided care for the infant. In *Attachment*, Bowlby (1969:304) asked: “Can a woman other than a child’s natural mother fill adequately the role of principal attachment-figure?” He answered in the affirmative, but qualified this as follows (Bowlby 1969:306):

Though there can be no doubt that a substitute mother can behave in a completely mothering way to a child, and that many do so, it may well be less easy for a substitute mother than for a natural mother to do so. For example, knowledge of what elicits mothering behaviour in other species suggests that hormonal levels following parturition and stimuli emanating from the newborn baby himself may both be of great importance. If this is so for human mothers also, a substitute mother must be at a disadvantage compared with a natural mother. On the one hand, a substitute cannot be exposed to the same hormonal levels as the natural mother; on the other, a substitute may have little or nothing to do with the baby to be mothered until he is weeks or months old. In consequence of these limitations, a substitute’s mothering responses may well be less strong and less consistently elicited than those of a natural mother.

In brief, Bowlby believed that biology had prepared the biological mother for the caregiver role. Thus, she would be the most adequate main attachment figure for her child.

Bowlby indeed saw the mother-child pair as the ideal. He considered monotypy a proven fact of nature, he considered the mother the natural attachment figure, and his work focused on the effects of deprivation or separation from the biological mother. Further, he believed that attachment problems resulted from lack of mothering, not from lack of caregiving (Bowlby 1969:357):

Disturbances of attachment behavior are of many kinds. In the Western world much the commonest, in my view, are the results of too little mothering, or of mothering coming from a succession of different people.

He also condemned the existence of childcare centers on several occasions.

Last, but not least, Bowlby’s specific use of words made his defense of the biological mother’s central role clear. There is no reason to say “mother” if you mean “caretaker” or to refer to “mothering” if you mean “caregiving.” There is no reason to say mother, but add a footnote to state that what you mean is “mother-figure.” In the period during which he wrote, Bowlby was aware that most people were interpreting his writings to mean mother. He often cited authors who believed that lack of maternal love pushed their children into deep pathologies. For example, to support his views he cited the work of Bruno Bettelheim and Margaret Mahler, both of whom were well known for defending the idea that rejecting mothers cause their children’s autism (Bowlby 1969:345–346; 1982:346–347).

It is also telling that Bowlby did not revise his views about the importance of the mother when he published a second edition of *Attachment* in 1982. He still presented the same “observations to be explained” that he had included in

1969, though by then most of those studies had been thoroughly criticized, as I pointed out above. By 1982, even Robertson had rejected in print the view that his observations of children in hospitals could support the generalizations made by Bowlby (Robertson and Robertson 1971).

In the 1982 edition, Bowlby mainly updated some assertions about primatology and biology, adopting ideas that further restricted who could be an attachment figure. He said one should not talk about the welfare of the “species,” as he had done following Lorenz. Instead, he now referred to the genetic theory of natural selection as described by George C. Williams and popularized by Richard Dawkins and E. O. Wilson. Neo-Darwinian theory took “the individual gene” as the central unit of selection in evolution (Bowlby 1982:55). Adopting this view, Bowlby noted that “the ultimate outcome to be attained is always the survival of the genes an individual is carrying” (Bowlby 1982:56). In the gene-centric model proposed by Bowlby, only the biological mother or another kin member could have been selected by natural selection to assume a caretaker role in the EEA.

It must be made clear that the presentation of the human attachment system as an adaptation was a hypothesis advanced by Bowlby, but he did not present the necessary evidence to sustain that claim. First, most of the empirical work discussed in *Attachment* is about nonhuman animals. Second, in the neo-Darwinian, gene-centric evolutionary model, natural selection always selects among alternatives and can only lead to evolutionary change if a behavior has a genetic basis. Thus, to show that the secure attachment system (proximity-seeking infant and sensitive-responding mother) is in fact an adaptation, Bowlby would have needed to demonstrate:

- that there are genes for proximity-seeking behavior,
- that natural selection favored babies with genes for proximity seeking over babies without them,
- that sensitive mothering is genetically based, and
- that sensitive mothering provided higher fitness for mothers than its alternatives.

Bowlby, however, lacked evidence for any of these basic points. In fact, to date there are no studies which show the existence of genes for proximity-seeking and sensitive-mothering behaviors. In addition, we do not know how to assign fitness values to those behaviors. It is thus not possible to say that they are adaptations. I will return to these points later.

Given that Bowlby was presenting the evolutionary framework as a hypothesis, his ethological theory of attachment might have remained merely a grand theoretical scheme. Thanks to Ainsworth’s research and the data generated by many scholars using the laboratory procedure she introduced to test attachment, that empirical foundation became much larger. Whether it is strong enough to support the theory in a convincing manner is, however, another issue.

Ainsworth: The Experimental Support from the Strange Situation

After leaving Uganda, Ainsworth obtained a job in the United States at Johns Hopkins University. In the mid-1960s, she attempted to replicate her Ganda study in Baltimore, Maryland. Ainsworth and three assistants observed 26 infants from white middle-class families in their homes. They took notes on mother-infant interactions at three-week intervals from 3–54 weeks of age. In Baltimore, however, Ainsworth did not observe the behaviors that she had used to assess attachment in Uganda. Thus, in 1964 she devised a procedure to elicit those behaviors: the Strange Situation.

The goal of the Strange Situation, a twenty-minute laboratory procedure, was to assess (a) whether the infants used the mother as a secure base to explore, (b) how they reacted when their mothers left them in an unfamiliar environment with a stranger, and (c) how they behaved when their mothers returned. It was a laboratory procedure that involved seven episodes of about three minutes each in the following sequence:

- The child and mother are alone.
- A female stranger enters the room and engages the infant.
- The mother leaves; the mother returns and the stranger leaves.
- The mother leaves again; the stranger returns.
- The mother returns and the stranger leaves.

Twenty-three of the infants from the Baltimore study (ranging in age from 9–24 months) were brought to the lab in their fifty-first week of study. Depending on the child's behavior, the children were divided in three main groups and eight subgroups.

In *Patterns of Attachment*, Ainsworth et al. (1978) presented the Strange Situation as a reliable test to categorize not only behaviors, but also children and their mothers. Depending mainly on the behavior of the infants upon their mothers' return, infants were classified into different categories: 65% as securely attached (B), those children who greeted the mother and sought contact with her; 20% as anxious-avoidant (A), those children who appeared to reject the mother upon her return; and 13% as anxious-resistant or ambivalent (C), those children who displayed both attachment behavior but also signs of being upset with the mother. In addition, the children's reactions were taken to be an index of the quality of their attachment to their mothers (Ainsworth et al. 1978:xi). According to Ainsworth et al. (1978:144–146), the securely attached children experienced sensitive maternal care; insecurely attached children had mothers who were inconsistent, rejecting, or unresponsive.²

Clearly, the maternal style is not observed in the Strange Situation since the mother only enters and leaves the room following a set script. In the original

² Later, Main and Solomon (1990) introduced another category: disorganized (D) for children who did not fit any of the previous three categories.

study, the correlation between the child's behavior and the mother's behavior was established from the analysis of the observations of the mothers at home. Designed as a complement to the Baltimore study, the interpretation of the Strange Situation is tied to the results of that study. As Ainsworth et al. (1978:321) clearly stated:

We must emphasize that individual differences in strange-situation behavior would have been well-nigh uninterpretable without extensive data about correlated individual differences in other situations, and especially without the naturalistic data that we collected in regard to Sample 1 at home throughout the first year of life.

In the Baltimore study, maternal behavior had been categorized along four dimensions: acceptance-rejection, cooperation-interference, accessibility-ignoring, and sensitivity-insensitivity. The analysis showed that infants of group B had the most sensitive mothers, while mothers in group A were more rejecting than those of infants in group C.

Despite the small sample and the fact that this study could at most establish a correlation between maternal care and child's behavior, Ainsworth presented it as showing a causal relation. Thus, the quality of the care provided by the mother was simply inferred from the child's behavior observed in the lab. Since the authors claimed that maternal sensitivity was the main factor in shaping the attachment, the categorization of children automatically entailed a categorization of the mothers. As a way of identifying sensitive mothers, this categorization also implied a moral judgment about mothers (LeVine and Norman 2001).

Furthermore, *Patterns of Attachment* presented the results noted above as proof of Bowlby's ethological attachment theory and its prescriptive implications. Ainsworth et al. (1978:95) argued that these were "normative findings" depicting "certain features of the species-characteristic organization of attachment behavior in the human 1-year-old and its interplay with other behavioral systems." They felt that these results confirmed Bowlby's theory: "We consider that the normative findings substantially support Bowlby's (1969, 1973) descriptions of the organization and function of infant attachment behavior" (Ainsworth et al. 1978:95). However, they did not explain how the behavior of children in a laboratory setting could provide empirical backing for Bowlby's claim about attachment being an adaptation, which would have required evidence about the course of historical developments many thousands of years ago.

Ainsworth had earlier followed Bowlby in adopting an evolutionary explanation for attachment and in viewing the biological mother as the primary attachment figure. In *Infancy in Uganda*, Ainsworth acknowledged the "new ethological view of instinct" as a main influence on her perspective (Ainsworth 1967:432). The same year that Bowlby published *Attachment*, Ainsworth endorsed Bowlby's position (Ainsworth 1969). Here, she addressed the questions

regarding the deviations from the behavioral system constructed in the EEA and the role of the biological mother. Given that these two issues have been very controversial in the attachment literature to this day, it is worth citing Ainsworth at length.

Ainsworth (1969:1003) noted that Bowlby's conception of attachment took into account "both (a) the intraspecies uniformities in attachment and its development and (b) the deviations which are nonadaptive and which form the basis for a variety of pathologies." As she had stated earlier (Ainsworth 1969:1000):

The function of a system is the one that gave it species-survival advantage in the "environment of evolutionary adaptedness"—the original environment in which the species first emerged. The biological function of the behavior may or may not give special advantage in one or another of the various environments in which present-day man lives, but this is a quite distinct consideration. Genetic programming continues to bias the infant to behave in ways adapted to the original environment of evolutionary adaptedness, and, similarly, under all the layers of individual learning and cultural acquisition, there is still a bias for mothers to behave reciprocally—a bias which may have been more or less sharpened or blunted by learning in any individual mother.

In this passage Ainsworth claimed that because attachment was an adaptation in the EEA, the infant is still genetically inclined to attach to its mother, and the mother is genetically inclined to attach to her infant. Therefore, for Ainsworth, the mother is the ideal attachment figure for the infant. Indeed, regarding the role of mothers, she claimed (Ainsworth 1969:995–996) that:

...ethologists hold that those aspects of the genetic code which regulate the development of attachment of infant to mother are adapted to an environment in which it is a well-nigh universal experience that it is the mother (rather than some biologically inappropriate object) who will be present under conditions which facilitate the infant's becoming attached to her.

It is, however, unclear to whom she was referring, since she did not provide a citation to any work by ethologists.

In later years, Ainsworth continued to appeal to biology in order to support her views, but she only provided a handful of references to the biological foundations of attachment. Thus, in 1970, Ainsworth and Bell claimed that because of the risks involved in the helpless infancy of the human species and its need for protection, "it is inferred, therefore, that the genetic code makes provision for infant behaviors which have the usual (although not necessarily invariable) outcome of bringing infant and mother together" (Ainsworth and Bell 1970:51). However, from the fact that a certain feature would increase the fitness of an organism, we cannot infer that the genetic code made a "provision" for such a feature.

Finally, as mentioned above, Ainsworth presented her work on the Strange Situation as a contribution to Bowlby's evolutionary model of attachment. Thus, her 1978 book with Blehar, Waters, and Wall about patterns of attachment

presented their work as throwing “important light on the concept of infant-mother attachment as viewed from an evolutionary-ethological standpoint” (Ainsworth et al. 1978:322), although what little they said about evolution is only a summary of Bowlby’s views. They referred to the survival uses of protection and how attachment patterns are consistent with them. In a contemporary paper, Ainsworth (1979:37) claimed that “the behavior of the securely attached infant and his responsive mother...may be recognized as the expected evolutionary outcome of infant attachment and attachment behavior and of a reciprocal maternal behavior system which are preadapted to each other.” In addition, “to the extent that the present environment of rearing departs from the environment to which a baby’s behavior is preadapted, behavioral anomalies may be expected to occur” (Ainsworth 1979:5; for the same point, see Ainsworth et al. 1978:9).

Although these are rather confusing statements, they make one thing clear: if Ainsworth believed that the infant’s attachment system and the maternal behavior system were “preadapted” to each other, then the ideal attachment figure for a child must be its mother. For all the talk in the works by Ainsworth and by Bowlby about attachment “figures,” their emphasis on the genetic basis for these systems, the view that they are adaptations, and the notion that deviations from the EEA produced pathologies, all lead inevitably to seeing the mother-infant dyad as the ideal. Thus, Ainsworth’s work did not help overcome the monotropic, mother-based positions of Bowlby.

Nevertheless, Ainsworth’s twenty-minute procedure to categorize infants and mothers undoubtedly became central to the successful spread of Bowlby’s views. In the context of post-World War II views of science, the operationalization of attachment research via the Strange Situation played a key role in the rise and expansion of attachment theory. Having an easy, short, and cheap laboratory tool to study attachment was crucial to EAT’s enormous appeal in American psychology. As historians of science have documented, after World War II, psychology and the social sciences turned increasingly toward a model of science that aimed to emulate the natural sciences in their reliance on experiments, reproducibility of results, and search for universal generalizations (Herman 1995; Solovey 2013). The Strange Situation fit well within that model: it was a laboratory procedure that promised causal explanations of behavior and also afforded predictions. In addition, it was presented as confirming a theory that made universal claims about emotions backed by biological science. Thus, it was in line with the methodological and epistemological goals of mainstream postwar psychology.

However, serious problems remained. Not only had Mead’s powerful challenge gone unanswered, but some developmental psychologists in the 1970s were calling attention to the reductionistic stance implicit in this vision of science, children, and mothers. In addition, Ainsworth’s adoption of Bowlby’s evolutionary framework to interpret her results encouraged criticisms of the uses of evolutionary ideas to support specific views on attachment.

Critical Assessments of the Strange Situation and the Ethological Attachment Theory: From the Child in the Lab to General Views about Child Development and Human Nature

Despite the key role of the Strange Situation in leading to an explosion of work on attachment, the procedure encountered serious criticisms. Several major developmental psychologists challenged the simplistic assumptions in this type of laboratory work. Others criticized specific implementations of it.

By the late 1970s, some child psychologists criticized the increasing reliance on simple experiments to understand child development. At Cornell, Urie Bronfenbrenner (1977:513) put it with graphic clarity: “Much of contemporary developmental psychology is *the science of the strange behavior of children in strange situations with strange adults for the briefest possible periods of time.*”

So did Yale researcher William Kessen. In a 1978 address to the developmental psychology division of the American Psychological Association entitled “The American child and other cultural inventions,” Kessen found it problematic to believe in a “free-standing” and “self-contained” child with its instincts, traits, and attachments as essential components of its nature. According to him, this vision of children also made it possible for psychologists to take the child as the unit of study rather than to consider the child as part of a more complex system of relations and network of influences. The conception of the “isolable” child had important and troubling consequences: “basically, we have observed those parts of development that the child could readily transport to our laboratories or to our testing sites” (Kessen 1979:819). This atomistic view of children led psychologists to ignore the *role of context* in child development.

Some scholars proposed an integration of experimental work in psychology with ethnographic research in order to get a fuller and more realistic view of child development. For example, developmental psychologists Charles M. Super and Sara Harkness put forward the concept of the “developmental niche” to refer to an individual’s experience of culture at any developmental stage (Super and Harkness 1986). Michael Cole and his colleagues at the UCSD Laboratory of Comparative Human Cognition argued for the significance of the local interpersonal context to understand cognitive development (Cole 1985).

In addition, an exhaustive and careful review of Ainsworth’s implementation of the Strange Situation Procedure led to substantial criticisms. In 1984, Michael E. Lamb, Ross A. Thompson, William P. Gardner, Eric L. Charnov, and David Estes presented a detailed analysis of the Strange Situation (Lamb et al. 1984a, b) which Lamb, Thompson, Gardner, Charnov, and Connell extended into book form a year later (Lamb et al. 1985). They found serious methodological problems with the procedure: the data were not sufficiently reliable and the samples were too small to support the generalizations put forth by Ainsworth’s group. Furthermore, “observer reliability was never assessed

in the homes and was inadequately assessed in the Strange Situation” (Lamb et al. 1985:65). Their review was devastating, leading to the conclusion that the results from the Baltimore study should “be viewed with great caution” (Lamb et al. 1984b:131).

Furthermore, the biological basis of attachment theory came under trenchant criticism in the early 1980s, soon after the publication of *Patterns of Attachment*. Hinde penned the first critical examination of EAT’s biological claims. In 1982, he expressed concern about the appeal to natural selection in order to argue that “any one mothering style is necessarily best” (Hinde 1982:72). Hinde pointed out that it was unlikely that natural selection would have produced stereotypy, given that the optimal mothering behavior varies with a number of factors. He suggested that it was more likely that selection would have favored conditional maternal strategies (Hinde 1982:71). Finally, he also questioned “the assumption that infant behavior and maternal behavior are adapted to mesh with each other” (Hinde 1982:73).

Lamb and colleagues also provided a critical analysis of the use of biology in attachment theory (Lamb et al. 1984a, b, 1985). First, they rejected the adequacy of the imprinting model for conceptualizing an infant’s attachment to an adult, since imprinting only operates in some species. In addition, they noted that there was no evidence for the existence of critical or sensitive periods in human social development (Lamb et al. 1985:24).

They also criticized how Ainsworth et al. (1978) interpreted the different patterns found in the Strange Situation by reference to Bowlby’s evolutionary framework. The most common pattern of behavior, B, which involved seeking proximity and contact with the mother was labeled “secure attachment” because it was the pattern that Bowlby argued would have led to higher fitness in the EEA. The behavioral patterns of children in the A and C categories were considered “maladaptive.” Lamb and coauthors agreed with Hinde’s point that it was unlikely that natural selection had favored a single parental strategy, as behavioral ecologists supported the view that behavioral strategies are often conditional and frequency dependent. They thus challenged the “ideal single pattern” model underlying EAT and, specifically, the view that secure attachment, as evaluated by the Strange Situation, is adaptive, while other types of attachment behaviors are maladaptive (Lamb et al. 1985:52).

Lamb and his coauthors raised additional problems with the biological claims invoked to support EAT as well. They questioned the assumption that human parents and infants had evolved to form intimate, harmonious relationships, especially given the work of Robert Trivers on parent-offspring conflict (Trivers 1974). They called for caution in using the EEA as a heuristic tool since any hypothesis based on assumptions about it are “inevitably speculative” (Lamb et al. 1985:49). They also noted that unless researchers were able to establish the fitness consequences of the behavioral patterns observed in the Strange Situation, speculations about the evolutionary meaning of different patterns of infantile behavior were “of limited value” (Lamb et al. 1984a:164).

In addition, they called for clarification of the different meanings of the term “adaptation” as used in the attachment literature. Sometimes it was used to refer to developmental mental health and sometimes to biological fitness, and there was no reason to assume that these two were related. Lamb et al. noted as well that it was unclear “whether ‘adaptive’ attachment behavior” was thought to “bring fitness advantages to infants in contemporary times” (Lamb et al. 1984b:143). Again, what was adaptive in the EEA might not be biologically adaptive in the present, and might be of no consequence for contemporary psychological well-being either. Moreover, they pointed out that the conflation between the biological and psychological senses of adaptive was biasing the interpretation of empirical research. In their words: “Researchers... have tended to look for associations between B group behavior and a broad range of optimal outcomes, and links between A- or C-group behavior with more negative or maladaptive outcomes” (Lamb et al. 1985:55).

These assessments had far-reaching implications for the status of EAT and the Strange Situation. The charges were numerous and raised severe doubts about the core of the theoretical and empirical foundations of attachment studies. However, attachment researchers failed to respond to them. Lamb had been a student with Ainsworth, and she took his criticisms as a betrayal of the cause of attachment research (Ainsworth 1998; Karen 1998:265). As the lead author of the critical publications, he took the brunt of the unfavorable replies from insiders. Although Lamb went on to a brilliant career at the University of Cambridge, many attachment researchers effectively ostracized him.

In subsequent years the challenges to the foundations of attachment studies continued to mount. In the early 1990s, Robert Hinde and Joan Stevenson-Hinde questioned other claims about the biological foundations of attachment. They criticized the extrapolations from suppositions about the EEA to the view that secure attachment behavior is normal and, therefore, desirable. The title of their paper, “Attachment: Biological, Cultural and Individual Desiderata” highlighted the need to separate different claims made in the attachment literature, which often confused these levels. At the individual level, what matters is psychological well-being; at the biological level it is maximizing an individual’s inclusive fitness; and at the cultural level it is the goals and norms of a society. They called for clarification of the claim that secure attachment is best (Hinde and Stevenson-Hinde 1990).

Despite the fact that Bowlby always appealed to Hinde’s work to support his position, how far Hinde’s views were from Bowlby’s can be clearly appreciated in a paper that Hinde prepared as a tribute to Bowlby. Here, Hinde emphasized first that his research with rhesus monkey troops had shown that the effects of maternal separation depend on “diverse factors that interact in a complex way” (Hinde 1991:157). He saw those results as agreeing with what other researchers had shown in macaques (Stephen Suomi) and in human infants (Jim and Joyce Robertson, Michael Rutter). Thus, rather than focus on dyads, he stressed “the importance of seeing the individual as set within a

network of relationships” (Hinde 1991:158). Further, he noted, in humans one needed to add the effect of “the sociocultural structure” (Hinde 1991:158).

Summarizing points he had made in earlier writings, Hinde strongly objected to the interpretation that the secure attachments identified in the Strange Situation implied that “what is natural is best, and that it is natural for children to have sensitive, responsive mothers” (Hinde 1991:160). After enumerating many of the points noted earlier, he concluded again that “the desideratum of ‘psychological health’ may depart from those of either biology or culture or both” (Hinde 1991:161). Noting that this was perhaps an issue “on which John Bowlby and I would not have seen exactly eye-to-eye,” he concluded that he was “hesitant about describing a particular type of child-mother relationship as optimal.” Condensed among nice words of praise for Bowlby’s leadership and gratefulness for how much he owed him, Hinde’s paper presented a group of key points against EAT.

Still, neither criticisms about the implementation of the Strange Situation nor criticisms of EAT’s evolutionary claims had a serious impact on attachment research. As had already happened with Mead’s earlier challenge to Bowlby, attachment scholars forged ahead by ignoring the mounting criticisms. In subsequent years, this field grew mostly by focusing on the Strange Situation Procedure. In fact, both supporters (Bretherton 1991:25) and critics (Quinn and Mageo 2013:4) have noted how the explosion of work using the Strange Situation has led to a confounding of attachment theory with the instrument. By facilitating the training of graduate students and the establishment of research networks, the Strange Situation led to an explosion of literature on attachment.

However, the interpretation of the role of maternal sensitivity in attachment that is at the center of many studies using the Strange Situation Procedure rests on very shaky foundations. The Strange Situation can be used for many purposes, but its use to infer that children’s behavior is mainly due to the relationship with their mothers derives from a flawed study. Despite Lamb et al.’s criticisms, attachment theorists continue to present the Baltimore study as a model of careful research, asserting that the observers took running notes scored by time markers every five minutes, and translated their notes into audiorecorded narratives immediately after each visit (Bretherton 2013:462). However, my review of the archived original data in the summer of 2015 revealed that this was not the case. Although confidentiality prevents quoting directly from these data, the narrative reports from these observations that I have seen cannot be considered trustworthy scientific reports. Several of them are permeated with subjective evaluations of the mothers’ personality from day one, including moral judgments. Other reports reveal tensions between the observer and the observed mothers. In addition, the reports from the different observers vary substantially in nature and quality, and most of them do not include notes taken every five minutes. In fact, one observer did not write up the observations until months later. Since these observations were key to Ainsworth’s interpretation

of children's behavior in the Strange Situation, the quality of their observations calls into question the validity of the Baltimore study and throws doubt on the links it supposedly helped to establish between the children's behavior and the conduct of mothers.

Attachment researchers also continued to assert that biological science supported EAT. In a 1991 paper, Ainsworth (1991:33) wrote: "The great strength of attachment theory in guiding research is that it focuses on a basic system of behavior—the attachment system—that is biologically rooted and thus species-characteristic." But to say that a behavior is "biologically rooted" is a vacuous statement. Most human behaviors are "biologically rooted," since we are living beings and the products of evolution.

As we saw, Ainsworth et al. (1978:322) presented their book on the patterns of attachment that are identified in the Strange Situation as contributing to the "evolutionary-ethological standpoint" of attachment theory. Other researchers make stronger claims about the significance of Ainsworth's work for EAT: "Ainsworth's work has paved the way to (1) a deeper understanding of the phylogenetic foundations of the socioemotional development of infants in the context of their relationships with their significant caregivers" (Sagi-Schwartz 1990:11). However, this widespread idea that the results of the Strange Situation provided confirmation for EAT is mistaken for the simple reason that the behavior of children in a lab is incapable of illuminating the phylogenetic foundations of children's emotions. Ainsworth's Strange Situation laboratory procedure may well measure how a child reacts after being left alone with a stranger, but research of this type cannot provide empirical support for the view that a particular form of mother-infant attachment is an adaptation. Claims about behavioral adaptations are claims about the history of the behaviors. However, the observation of current behaviors in a laboratory cannot provide evidence about their phylogenetic history. Phylogeny is about reconstructing evolutionary history. Ainsworth observed how children acted at home as well as under specific laboratory conditions. These observations can detect patterns of behavior. However, patterns of behavior per se do not provide any evidence about phylogeny. Phylogenetic reconstruction requires other kinds of data: about the history of the character (behavioral, morphological, or physiological trait) under study, about its origin, comparative data from different groups to enable decisions as to whether the character under consideration is a homology or analogy, etc. To reconstruct evolutionary history, we need to put together demographic data with paleontological evidence, primatology, archeological results, as well as knowledge about present-day hunter-gatherer societies.

Therefore, it is incorrect to present Ainsworth's work as providing evidence for the evolutionary foundations of attachment theory. Even assuming the validity of her results and her interpretation of them, her work is unable to confirm or refute claims about the phylogenetic history of human behaviors and emotions. As Bowlby himself once noted, to show that attachment is an

adaptation, one must turn to the archaeological record, anthropological studies of hunter-gatherer societies, and studies of other primates (Bowlby 1969:61).

Recent Proposals: Saving Attachment in the Environment of Evolutionary Adaptedness

Much of the attachment literature has failed to address the challenges discussed above. However, some attachment researchers have understood the need for a different approach and tried to present better evidence for the evolutionary ideas that Bowlby and Ainsworth defended. In what follows I review the major proposals put forward since the 1990s to develop the “biological basis” of EAT. I conclude that those proposals have not presented sufficient evidence to accept EAT.³

The Universality of Human Social Attachment as an Adaptive Process

In several writings, German psychologists Klaus and Karin Grossmann have presented attachment as a “biological program” (Grossmann and Grossmann 1990:41, 45). In their 2005 paper, “Universality of Human Social Attachment as an Adaptive Process,” they assert: “Attachment is the phylogenetically programmed propensity of a human child to form a special relationship with responsive caregivers as part of the infant-parent bond” (Grossmann and Grossmann 2005:199). It is not clear whether they are claiming that infants have a propensity to attach to parents, or to any caregiver. They emphasize the genetic basis of the attachment system: “We view attachments as the developmental process during which infants’ genetic programs become phenotypically manifest, observable, and testable as a function of caretakers’ responsiveness” (Grossmann and Grossmann 2005:202). They also present attachment as “a universal genetic program valid in all cultures, despite clearly observable variations in parental caregiving behaviors between existing cultures and within cultures in different epochs” (Grossmann and Grossmann 2005:202–203).

These strong pronouncements are surprising because the Grossmanns also recognize that attachment theory has yet to fulfill its aspiration to provide an ethological explanation of attachment behavior. They note the need to address four questions that Tinbergen had argued were central to a biological account of behavior: causation, development, function, and evolution (Tinbergen 1963). But the few paragraphs they provide under each category contain general assertions about behavior, with few specifics about attachment (Grossmann and Grossmann 2005:218–222). For example, under causation they state (Grossmann and Grossmann 2005:218): “The general assumption

³ I do not examine James S. Chisholm’s work because he presents his most recent views in this volume (see Chapter 11).

is that physiological structures of the central nervous system and hormonal processes ‘cause’ human behavioral adaptation at the proximate level.” After some comments about the role of hormones in close bonds, they note that some “unanswered questions” remain. On the specifics about attachment, they admit that they can only speculate (Grossmann and Grossmann 2005:219, italics added for emphasis):

Comparison of previously experienced maternal sensitivity of the insecurely with the securely attached toddlers suggests that the differences in physiological stress *may have been* a consequence of suboptimal attachment experiences, which, in turn, *may have influenced* the toddler’s physiological reactions to stress. Hypo- or hyperactivation of physiological systems *might be a response* to past inappropriate management of the infant’s homeostasis by the mothering figure...”

What the Grossmanns write about development is of limited value as well (Grossmann and Grossmann 2005:219): “In attachment theory, the developmental processes are assumed to be basic and thus universal for human infants.” By this they mean that infants “are born prepared to become social” and ready to learn from those around them. After noting these facts, which are facts that no developmental theory would deny, they say (Grossmann and Grossmann 2005:219):

As an open genetic program, infants’ attachment development accommodates a limited variety of caregiving practices and styles if they comply at least minimally with an infants’ (sic) basic attachment needs to be protected and cared for by at least one reliably available individual.

To support this, they cite studies in communist countries and Israeli *kibbutzim* where children were raised with interchangeable caregivers. These studies showed that children developed less competently, according to attachment measures of competency, but they tell us nothing about how the “open genetic program” operates. Nor do they present any research showing how this program develops in different individuals.

The sections on the function and evolution of attachment are equally disappointing. The section on function repeats Bowlby’s view that attachment conferred protection from wild animals and dangers in the EEA, and simply adds that sociobiology would include other nonkin humans among the dangers. The rest of the section does not address function. The last section, evolution, also does not say anything about the evolution of attachment.

Not surprisingly, the Grossmanns conclude that further research is needed about the evolutionary aspects of attachment. Indeed, all the points they raise in their conclusion for further study show that one could not yet talk about the “universality of human social attachment as an adaptive process,” despite the fact that this is the title of their paper. Last, and very importantly, the Grossmanns never clarify what they mean by adaptive in their paper.

The Nature of the Child's Ties

In an explicit recognition of the significance of the evolutionary framework for attachment theory, the leading chapter in all the editions of the *Handbook of Attachment* (the major compendium of advances in the field) is entitled "The Nature of the Child's Ties." The title pays homage to Bowlby's 1958 foundational paper, while dropping the controversial "mother" from the title. Still, it is not clear here to whom the child is supposed to attach, except to the generic "attachment figure." In this widely cited paper, June Cassidy (1999:4) claims: "The most fundamental aspect of attachment theory is its focus on the biological bases of attachment behavior." After summarizing Bowlby's views, Cassidy (1999:5) concludes:

In a basic Darwinian sense, then, the proclivity to seek proximity is a behavioral adaptation in the same way that a fox's white coat on the tundra is an adaptation. Within this framework, attachment is considered a normal and healthy characteristic of humans throughout the life span.

Nowhere in the article, however, does she present the evidence needed to show that. Whereas we have a pretty good understanding of the genetics of coat colors in animals and the environments in which foxes live, we still do not know anything about the genetics for attachment behaviors and very little about the environments in which they were supposed to develop in the EEA. We also do not know which alternatives selection would have to choose from or their fitness values.

The lack of evidence to support evolutionary explanations of attachment behaviors is made evident in Cassidy's proposals to explain the evolution of monotropy. She offers the following scenario (Cassidy 1999:15, italics added for emphasis):

First, the infant's tendency to prefer a principal attachment figure *may* contribute to the establishment of a relationship in which that one attachment figure assumes principal responsibility for the child... Second, monotropy *may* be the most efficient for the child as the child does not have to assess who would be the best person to help. Third, monotropy *may* be the child's contribution to a process I term "reciprocal hierarchical bonding," in which the child matches an attachment hierarchy to the hierarchy of the caregiving in his or her environment."

Yes, monotropy may have evolved in any (or all) of those ways. Then again, it may not have.

In addition to the lack of evidence for this just-so story about the evolution of monotropy, two other aspects of Cassidy's paper are noteworthy. First, it is confusing to see her explanation of monotropy given that numerous attachment theorists have claimed that monotropy is not part of attachment theory. Second, note the conflation between the biological, the normal, and the healthy. Is she suggesting that attachment is normal and healthy because it is an adaptation?

As Hinde and others have pointed out in several publications, one does not follow from the other.

Despite these issues, in the new 2016 edition of the *Handbook*, Cassidy (2016:17) concludes: “Bowlby and Ainsworth’s original ideas have held up well...nothing that has emerged from the thousands of studies produced over the past 40 years has led to a serious challenge to the core theory” (Cassidy 2016:17). But, as we have seen here, the challenges are many and deep. At this point, it is difficult to understand what, if anything, those who are committed to this paradigm would consider a challenge to it.

The Innate Attachment System

Jeffrey A. Simpson and Jay Belsky have published several papers aimed at developing an evolutionary framework for attachment, both independently (Belsky 1997a; Simpson 1999) and later in a joint chapter in the second and third editions of the *Handbook of Attachment* (Simpson and Belsky 2010, 2016).

In their joint paper, Simpson and Belsky (2010:131) claim that “attachment theory is one of a handful of major middle-level evolutionary theories.” On the “innate attachment system” they note: “Guided by Darwin, Bowlby believed that the attachment system was genetically ‘wired’ into many species through intense directional selection during evolutionary history” (Simpson and Belsky 2010:132). However, since views about evolution have changed since Bowlby’s early writings, they aim to “place attachment theory in a modern (neo-Darwinian) evolutionary perspective” (Simpson and Belsky 2010:132). According to Simpson and Belsky (2010:132–133), that perspective would include a series of views that “share a central premise: that much of the human mind and human social behavior reflect adaptations to the major obstacles to inclusive fitness that humans repeatedly faced throughout evolutionary history.”

After one section about those central evolutionary views, they examine the “stable features of the social EEA” (Simpson and Belsky 2010:135): “For thousands of generations, our ancestors lived in small, cooperative groups. Most people within a tribe were biologically related to one another, and strangers were encountered rather infrequently.” Later, in the section devoted to individual differences in attachment, Simpson and Belsky (2010:138) claim: “Ainsworth’s Strange Situation is well suited to detect different patterns of attachment because it presents infants with two common cues to danger in the EEA: being left alone, and being left with a stranger.” But how could situations that infants hardly ever, if at all, encountered in the EEA become “common” cues to danger? And if they were not common cues to danger, then what is the value of the Strange Situation Procedure for understanding better the EEA for attachment?

It is also unclear how one would evaluate what the best strategy might have been in situations in which the infant did not see the mother nearby in the EEA. Following Bowlby, Simpson and Belsky (2010:131) say that crying was the

first best strategy to attract the mother. If that failed, the second-best strategy was to remain aloof so as not to attract predators. But if crying was likely to attract a predator, why was it the best strategy? In a social environment where babies were hardly ever left alone, would it not be the best strategy for a baby to wait calmly and quietly for the mother's return? Then, if the mother did not return, as a second strategy the baby could cry to alert another adult of the group. This ordering of strategies makes more sense and it has as much evidence—or as little—as the one assumed in attachment theory.

Following Hinde's suggestion that natural selection probably would not have led to stereotyping in parental care, Simpson and Belsky note, as others before them, that each attachment pattern (secure, anxious-ambivalent, and anxious-avoidant) *could be* a different "strategy" that "*could have solved* adaptive problems presented by different kinds of rearing environments" (Simpson and Belsky 2010:138, italics for emphasis). But note the highly speculative nature of the proposed evolutionary explanations, as in the case of avoidance behavior (Simpson and Belsky 2010:139, italics for emphasis):

During evolutionary history, this behavioral strategy *would have increased* survival among infants who, *if they placed* too many demands on their parents, *might have been* abandoned....*If* maternal rejection served as a proximal cue of the severity of future environments, avoidant tendencies *might have allowed* children not only to move away from their parents earlier, but to become more opportunistic and advantage-taking, thereby facilitating survival and early reproduction in such arduous environments.

Again, I do not deny that all of this may have happened that way. But what evidence do we have to substantiate that it did, or that this explanation is superior to others?

In the last part of their paper, Belsky and Simpson present different evolutionary models of social development across the life span. It is not possible to review them here. However, the existence of several different proposals attests to the fact that these are theoretical models, based on various imaginative scenarios, none of which have sufficient empirical support.

Mothers plus Others

Sociobiologist Sarah Hrdy has also presented attachment theory as being part of evolutionary theory, claiming that it is "arguably evolutionary theory's most important contribution to human well-being." Although in several writings she has challenged attachment theory's focus on the mother-infant dyad, Hrdy presents her views to "correct an underlying assumption about the universality of exclusive maternal care in primates, not to challenge Bowlby's fundamental insights" (Hrdy 2009:82). Thus, her own proposal for the evolution of attachment, the cooperative breeding hypothesis, is put forward to support attachment theory rather than to challenge it.

In Hrdy's view, Bowlby was right about attachment being an adaptation in the early part of primate evolution. Indeed, Hrdy (2009:114) claims that: "As attachment theorists have long assumed, all primate infants evolved to seek contact with a warm and nurturing mother. There is no questioning Bowlby's insight on this point." During this early period, according to Hrdy, ancestral primate babies had little to worry about since the mother's urges to stay attached to her babies were equally strong, and thus these mothers were in constant contact with their offspring. Yet Hrdy does not tell us why we cannot question something that she recognizes is an assumption. Neither does she tell us what evidence supports her reconstruction of primate evolution.

In Hrdy's evolutionary scenario, "at some point in the emergence of the genus *Homo*, however, mothers became more trusting, handing even quite young infants over to others to temporarily hold and carry" (Hrdy 2009:114). She argues that allomaternal assistance was essential for children's survival during the EEA. In turn, alloparents enhanced "their inclusive fitness by helping kin" (Hrdy 2005c:11). Furthermore, Hrdy sees cooperative breeding as the motor of the capacities for mind reading and cooperation that eventually led humans beyond other primates. She proposes that maternal separations caused the development of the little apes' increasing ability to read the facial expressions of others, leading eventually to the human's greater ability to read minds and to empathize, compared to other apes. Thus, in this account, cooperative breeding not only explains why human babies can attach to several caretakers but it also explains the extraordinary evolution of the cognitive and emotional capacities of our human ancestors.

In several places, however, Hrdy recognizes the speculative nature of her proposal. She asks, "as critical as a grandmother (or a great-aunt) might be, how likely were they to be present? The answer is not knowable" (Hrdy 2005c:16). At another point she acknowledges writing in a "speculative vein" and notes that her model "relies on a number of assumptions" (Hrdy 2005c:25, 28). Despite these qualifications, however, in other places she makes assertions for which we do not have—and probably cannot have—any good evidence. For example: "Back in the Pleistocene, any child who was fortunate enough to grow up acquired a sense of emotional security by default" (Hrdy 2009:290). I am not even sure we can understand what a "sense of emotional security" would have meant in the Pleistocene, let alone how to evaluate who had it and why.

To support her cooperative breeding hypothesis, Hrdy places strong weight on studies showing that in some contemporary societies with high child mortality, alloparents affect child survival. Although clearly this is important information, it is not sufficient to extrapolate to evolutionary scenarios in the EEA for two main reasons: (a) we do not have any information about child mortality rates in the Pleistocene or their causes; (b) from the fact that something would have been useful, we cannot conclude that it was selected. In addition, there are

competing evolutionary explanations of human sociality (Sober and Wilson 1998; Richerson and Boyd 2005; Wilson 2012; Hawkes 2014).

Where Are We Today?

Some attachment theorists today provide sophisticated accounts of the possible evolutionary scenarios for attachment patterns that often take into account different ecological contexts and adopt a life history perspective. Some of them postulate the existence of different strategies of parental care and recognize that diverse attachment patterns can be adaptive. But the models put forth to defend the evolutionary basis of attachment rest on substantive assumptions about the human mind and about evolution that are still the subject of much debate (Gangestad and Simpson 2007). In addition, these models often rely on very different views. Thus, it is not clear which particular evolutionary framework is accepted in the attachment community today. The result is a number of competing frameworks, each of which is highly speculative.

In reviewing critically the proposals for an evolutionary account of attachment, I do not reject the general attempt to understand better the evolutionary history of the human mind and behavior. My point here is epistemological: Given the speculative nature of their proposals, scientists today cannot justifiably present EAT as part of evolutionary science or as grounded on accepted evolutionary views. Claims, such as “attachment theory is a special branch of Darwinian evolution theory” (van IJzendoorn et al. 2006:108), are unjustified.⁴ While there is nothing wrong with using the EEA as a heuristic tool to help us think about the possible scenarios in the evolution of behavior, it is not legitimate to conclude that any one of these scenarios confers evidence for EAT. To advance in this area, attachment researchers need to clarify the diverse claims made in the literature, identify which of them are still largely speculative, determine what sorts of evidence each of them would require to move beyond the realm of speculation, and then pursue research that could produce evidence for or against the various claims.

To do this, I doubt that the best way is to reconstruct the shape of attachment in the EEA because, to begin, the concept of the EEA has been controversial (Laland and Brown 2002; Plotkin 2004:150–152; Buller 2005). For some authors, including Bowlby, the EEA refers to a period, the Pleistocene, from about 2 million years ago to 10,000 years ago. In contrast, other evolutionary psychologists claim that the EEA is not a concrete historical period, but the statistical composite of the combined set of selection pressures that led to a given adaptation. Regardless, we do not know how to reconstruct either of those scenarios. We know little about the social and psychological makeup of our ancestors or the selective pressures they encountered. Although evolutionary

⁴ I do not address their evolutionary model because it is basically a diagram that presents attachment as an innate mechanism, but they do not elaborate or present evidence to support this.

psychologists believe that we have sufficient data to derive some conclusions, in this case we do not know enough to favor one hypothesis over the others (monotropy, allomothering, conditional strategies, no attachment system).

Over the years, leading figures in evolutionary biology and evolutionary anthropology have emphasized the difficulties in reconstructing the evolutionary past of the human species (Lewontin 1995/1982:163–171; Tattersall 2012:200). We must recognize that the reconstruction of behavioral characteristics still is—and will remain—incredibly difficult. To do this in the area of attachment, we would need to know about migration patterns, family size and structure, diseases, nutrition, work and social habits, and social conditions that affect child-rearing: fertility rates, marriage patterns, variation in family size, etc. Using our fertile imaginations, we can construct many such scenarios, some more plausible than others. Yet, we are still far from being able to make well-supported scientific claims about child development in our deep past.

Equally important, even if we could assert that secure attachment was an adaptation in the EEA, what would be its relevance for us today? Bowlby, Ainsworth, and other attachment theorists argued that it would be relevant for us because our minds have not changed much since the EEA. As many evolutionary psychologists put it, we have Stone Age minds (Cosmides and Tooby 1997). Yet, this is highly controversial. How likely is it that we are still behaving and feeling like our Paleolithic ancestors? In fact, how likely is it that the way they behaved and felt remained static during two million years, a long time period that saw tremendous changes at many levels?

In my view, the assumption of stasis in the evolution of our emotional and psychological makeup is at odds with scientific work in different areas. Given new discoveries about epigenetics, neural plasticity, and developmental biology, why should one assume that our emotional makeup has remained fixed since the Stone Age? We know that evolutionary change can occur in relatively short periods of time. Peter and Rosemary Grant have documented evolution by natural selection in the Galapagos finches in a period of merely a few decades. They demonstrated that alterations in body and beak size resulted from changes in the food supply (Grant 1986; Grant and Grant 2008). Evolutionary changes in humans since the rise of domesticated animals have also been documented (Richerson and Boyd 2005:191–192). Other scholars argue that rates of adaptive evolution in the Neolithic and later periods were much higher than in earlier periods in human evolution (Hawks et al. 2007). What we know about epigenetics shows that evolutionary changes could be much faster than we had assumed as well (Jablonka and Lamb 2014).

In short, we need to have much more data about the genetics, the epigenetics, and the fitness of specific attachment behaviors as well as about the ecological and cultural circumstances of our ancestors. Until then, we can construct many “just-so stories” about evolutionary scenarios, but it would be premature to assert that babies have stone-age minds, as EAT claims. Ainsworth (2013/1983:459) said:

I have sometimes been accused of being out of touch with current changes in lifestyles, but I believe that the problem is that infants are perhaps a million or so years out of touch with them. Their inbuilt evolutionary adaptations tend not to match new lifestyles, much as we would like to believe infants to be infinitely adaptable.

But nobody is defending, as she suggests, the idea that infants are “infinitely adaptable.” Instead, the point is that in order to show that the mind of an infant was set in stone “a million or so years” ago, as she believed, one needs to provide some evidence. So far, we do not have such evidence. (Additionally, note that Ainsworth’s use of “adaptable” conflated the biological and psychological senses of adaptation).

Furthermore, even if the secure attachment pattern were proven to be an adaptation also to be adaptive now, it would still not follow that deviations from it are pathological. When the environment changes, a previous behavior may or may not be adaptive. It could be less or more adaptive than it was in the previous environment, or the change in environment could have no effect on the fitness of the trait.

We need to separate biological facts from social goals. When we say that secure attachment is optimal, we are not only making a claim about biological evolution, we are also making a value claim. What forms of child-rearing are valuable to us cannot be settled only by knowledge of biological and psychological processes. In child-rearing, what is best depends on one’s views about what a good society is and on what counts as being a good child and a good caretaker in that good society. That is why to understand attachments we need to know how different societies integrate those values into child-rearing, which leads us back to where we started: culture.

New Insights from Ethnographic Research on Child-Rearing

While attachment workers focused mainly on (a) administering the Strange Situation Procedure in a variety of contexts and (b) testing children’s competencies, many cross-cultural psychologists and anthropologists kept raising the point Mead had put forward: there is more diversity in child-rearing than attachment theorists have acknowledged. After interest in cultural psychology and psychological anthropology increased in the 1990s, studies of child-rearing revealed important new insights. It would be impossible to review them all here; I will summarize some with special relevance to attachment theory.

Some ethnographic studies provided much needed research on mothers. In her 1992 book, *Death Without Weeping: The Violence of Everyday Life in Brazil*, Nancy Scheper-Hughes demonstrated the complexity of maternal sentiments. She observed three generations of mothers living in dismal economic and social conditions in the slums of the sugar plantation town of Timbauba, Brazil, during 1964–1984 and 1985–1992. The average woman had

9.5 pregnancies, 8 births, and 3.5 infant deaths. Scheper-Hughes argued that the high infant death rate shaped the attitudes and emotions of mothers toward their infants. Aware that their babies might not survive, the mothers did not immediately “personalize” their infants, did not name them right after birth, and did not mourn their deaths as is done in Western societies. She concluded that maternal love is not a uniform, universal, and natural monolithic affect. Challenging attachment theory, she called for a “pragmatics of motherhood”: a recognition that mother love “represents a matrix of images, meanings, sentiments, and practices that are everywhere socially and culturally produced” (Scheper-Hughes 1992:341; 2014).

That parental caregiving is not simply a system of innate responses is also made clear by David Lancy, who has shown that parents in many cultures focus on “not-attaching” to the infant until the infant can be considered a person in that community (Lancy 2014, 2015). Lancy compiled information on over 200 cases from the ethnographic and archaeological records representing all areas of the world and historical periods, from the Mesolithic to the present. He reports that in many societies, infants are not given status as a person at birth. For example, the Wari’ native Amazonian communities compare an infant to unripe fruit because they consider that the infant is still in the process of being made, and the Nankani in northern Ghana delay judging whether a baby is human until they are sure it is not a spirit or bush-child. Since babies are not considered real persons, the parents resist attaching to them (Lancy 2015:41, 49).

Ethnographic studies also found that mothers have different goals and follow different norms in child-rearing (LeVine et al. 1994; Harwood et al. 1995; Weisner 2005; Mageo 2013; Otto 2014). For example, in Kenya, Gusii mothers, who bear an average of ten children, focus first on their survival and then on teaching them compliance, contrary to mothers in the United States who do not aim for compliance in child-rearing (LeVine et al. 1994). In her studies of Samoan family relations, Mageo (2013) found that socialization is not oriented to develop feelings of security, but to encourage separation.

Other important studies have demonstrated the need for research to go beyond the mother-infant dyad since mothers are not the only caregivers in many societies; indeed, cooperative child care is a widespread practice. Weisner and Gallimore (1977) presented a thorough review of anthropological studies showing the importance of children as caretakers. Since then, much more ethnographic work has confirmed that children and other family and community members are an intrinsic part of caretaking networks in many societies (Gottlieb 2004; Meehan and Hawks 2013; Gottlieb 2014; Röttger-Rössler 2014; Weisner 2014). Psychologists have also called for studying children within social networks (Lewis 2005).

Cultural anthropologists and psychologists have also argued that attachment researchers’ views about children’s competence have been shaped by a particular set of values prominent in Western societies. For example, creativity, autonomy, self-reliance, and independent exploration are all important personality

traits for middle-class Westerners who educate their children to embrace those values. Other cultures, however, do not share those values. In some societies, parents educate their children to comply and subordinate their desires to the group's needs. If children in those cultures are measured by Western standards, their development will seem inadequate.

In sum, cross-cultural studies have shown that child-rearing goals, caretaking practices, conceptions of the child and childhood, and children's social relations and attachments are varied and multifaceted. They all acquire specific significance within the cultural context of a given society (Rogoff 2003; Keller 2007, 2013a; Everett 2014; Röttger-Rössler 2014; Keller and Chaudhary as well as Morelli et al., this volume).

Historians have also demonstrated that views about children, mothers, and child-rearing have changed over time, often in profound ways. Ecological, socioeconomic, and cultural factors have shaped different visions of childhood and child development throughout history (deMause 1975; Zelizer 1985; Elder et al. 1994; Koops and Zuckerman 2003; Stearns 2003; Kessel 2009; Mintz 2009; Everett 2014; Mayes and Lasonde 2014). Historical analysis also reveals deep changes in visions of motherhood and what counts as a good mother (Dally 1982; Apple and Golden 1997; Ladd-Taylor and Umansky 1998; Plant 2010). Additionally, different scientific perspectives have shaped varied, and sometimes contradictory, notions of caretaking and child-rearing (Hulbert 2003; Apple 2006).

On the basis of these results, many scholars have called for a rejection of the central core tenets of EAT. Some view EAT as a cultural ideological product of a specific historical context (Lancy 2014; Vicedo 2013; LeVine 2014). Other cultural anthropologists and cross-cultural psychologists call for further work on child-rearing in different cultural contexts and exploration of the significance of cultural variations (Rothbaum et al. 2000b; Keller 2008; Morelli and Henry 2013; Quinn and Mageo 2013; Keller 2014b; Otto and Keller 2014). None of these critics deny that children form attachments and that some of those influence their psychological growth. However, they reject the view that child development follows a universal and uniform pattern.

For a long time, attachment scholars remained reluctant to incorporate insights, provided by ethnographic work and psychological research, which fell outside their paradigm. Recently, some attachment supporters have written about the cultural dimensions of attachment, feeling frustrated by the criticisms of their cultural blindness (e.g., Mesman et al. 2016b). In addressing the cultural critiques, however, these authors often explain discrepancies away or find that studies which do not support their position are methodologically flawed (Mesman et al. 2016b:799). Most importantly, although they incorporate some ideas from cultural studies (e.g., the importance of multiple caretaking), they always end up concluding that all is well with the EAT paradigm as developed by Bowlby and Ainsworth. Thus, it is perhaps fair to say that the frustration is mutual, as students of other cultures believe that attachment researchers just

pay lip service to the significance of culture, while they continue without sound justification to postulate a uniform attachment “behavioral system.”

Supporters of the traditional EAT paradigm still believe there is a “pre-programmed” attachment system that is species-characteristic. Many of them talk about a biological program that is expressed in a universal and uniform manner with small cultural variations. They adopt, therefore, a vision of human nature in which biology is a fundamental stratum upon which culture superimposes new deposits through history. Ainsworth, for example, talked about the genetic programming “under all the layers of individual learning and cultural acquisition” (Ainsworth 1969:1000).

In 1966, however, that conception of the relation of biology to culture was brilliantly critiqued by the anthropologist Clifford Geertz. In “The Impact of the Concept of Culture on the Concept of Man,” Geertz argued that anthropological studies and new studies in biology were already putting to rest a stratigraphic vision of human nature inherited from the eighteenth century, which saw man as “a composite of ‘levels,’ each superimposed upon those beneath it and underpinning those above it.” This led to an intellectual strategy that aimed to “strip off the motley forms of culture” in order to reveal the “regularities of social organization,” by peeling off the different layers superimposed on the biological foundations (Geertz 1973/1966:37). Geertz thought the human sciences were finally giving up the hopeless search for a universal and uniform human nature.

EAT is one of the areas in which, *pace* Geertz, the search for the universal and uniform human nature rooted (or determined) in our ancestral past has remained alive. Cultural critics argue, however, that we still need to carry out empirical studies about social relations in a large number of cultures before we are ready to establish generalizations about child-rearing and child development. Moreover, anthropological work has shown that no single factor can explain a child’s emotional development and that we need to understand the cultural context in order to be able to interpret actions and emotions. Acts, behaviors, and feelings are part of cultural and psychological processes that acquire meaning within a large network of affects, other people’s behavior and expectations, social norms, and individual goals.

That is why the relationship between anthropology and psychology cannot be a mere affair or marriage of convenience. Anthropologists cannot work without some assumptions about the human mind, and developmental psychologists, including attachment researchers, cannot make sound claims about the universal nature of the human mind or the human child without investigating real children in real situations in diverse societies. Child development studies need to incorporate ethnographic research in different cultures. By engaging psychological insights about the human mind, anthropologists could overcome what historian of science Peter Galison has called “the limits of localism” (Galison 2016). Whether scholars aim for a cultural psychology that might be able to offer “universalism without the uniformity” (Shweder and

Sullivan 1993:517) or for a program of “developmental contextualism” (Lamb 2005:110), they need to carry out interdisciplinary research that integrates the results from the field with work from the laboratory.⁵ Toward this end, Morelli et al. (this volume) present a new paradigm to understand children’s attachments in Chapter 6.

Conclusion

Bowlby and Ainsworth claimed that the evolutionary framework they adopted was the defining characteristic of the theory of attachment they jointly developed, and that is what other supporters of attachment theory have emphasized: “One of the great strengths of attachment theory is that it is based firmly within an evolutionary, ethological and ultimately a biological framework” (Byng-Hall 1991:6). Appealing to the biological basis of attachment, Bowlby, Ainsworth, and their many followers today have presented the ethological attachment theory as having universal application. They make two central claims: (a) the mother-infant dyad is a biological system that became an adaptation in the EEA, and (b) secure attachment is the path for an infant’s optimal emotional development.

In this chapter, I have shown that those claims have had a contested history. A review of that history reveals that both claims remain unsubstantiated. From Mead to the present, ethnographic studies have provided extensive evidence for the diversity of child-rearing in different cultures. Those studies challenge the view that deviations from the secure-attachment pattern lead to pathological outcomes and that maternal sensitivity, as defined by attachment theorists, is necessary for a child’s adequate emotional development. From Hinde’s 1982 critique to the present, a number of researchers have also challenged the claim that evolutionary biology supports the notion that there is an ideal way of child-rearing.

Attachment theorists also continue to defend the uniformity and universality of attachment because in their view it is an adaptation. In turn, they believe that by being an adaptation, attachment must be universal and uniform. For example, Ainsworth (1991:33) wrote: “The great strength of attachment theory in guiding research is that it focuses on a basic system of behavior—the attachment system—that is biologically rooted and thus species-characteristic.” Recently, Mesman et al. (2016b:804) claimed: “The cross-cultural studies included here support Bowlby’s (1969/1982) idea that attachment is indeed a universal phenomenon, and an evolutionary explanation seems to be warranted.”

But the logic tying biology and universality rests on a mistaken assumption. A feature of an organism can be an adaptation without being universal, and universality is not sufficient to show that a feature is an adaptation or adaptive.

⁵ For an example of solid experimental work on how children learn to trust, see Harris (2012).

An evolutionary explanation is warranted when we have evidence for the evolution of the characteristic in question. In the case of the attachment system, it is still lacking.

In a very basic sense, EAT assumes a view of human nature in which diversity, variability, and historical change do not matter. By seeing variation as superficial, at best, and pathological, at worst, EAT denies the transformative power of historical change and cultural context.

What anthropology, cultural psychology, evolutionary biology, and history teach us is that diversity is the norm across time and space. Infants and mothers are part of sociocultural networks that vary and change. It is not simply that culture “has a role,” provides the “context,” or “adds” something to the developing mind of the child. Culture is much more than that: it is transformative at the individual and the species level. Psychological development is a process affected by many factors that interact in numerous ways. Evolution is also a historical process in which ecological, cultural, and biological factors interact in complex ways that affect ontogenetic and phylogenetic development. Perhaps what is needed are new metaphors and new conceptual frameworks to analyze the interrelation between all the factors that contribute to the dynamic processes of life histories at different levels.

As we become more aware of the intricate ways in which biology and culture influence each other, genetic-centric models do not seem the most appropriate vehicles for understanding the evolution of processes like child-rearing, for these are processes in which the role of cultural evolution is key. Better models of the coevolution of culture and biology are necessary before we can present a general theory of social attachments.

Bowlby adopted a conception of the evolution of mind that is still under considerable debate (Gangestad and Simpson 2007). He also relied on a neo-Darwinian view of evolution that has received substantial criticisms. As Denis Walsh has argued, we need to move beyond the conception of adaptation as design and of organisms as designed machines. He urges us to see organisms “as agents, making a place for themselves in the world” (Walsh 2015:185). As he reminds us, evolution is an ecological phenomenon, and organisms’ plasticity to engage with the ecological conditions which they encounter and create themselves is central to adaptive change. If this is true for all organisms, how much more so it must be for human beings who have been actively creating their own environments at an unparalleled pace. As Tattersall (2012:148) reminds us: “We differ today far more from our earliest Pleistocene ancestors than do any other of the creatures with which we share the planet.” That fact has much to do with our ability to modify the diverse environments we inhabit, an ability in which our biological and cultural features have interacted in complex ways for thousands of years.

Understanding that intertwined evolution in a way that does not simplify the biological and the cultural aspects of our nature still requires our concerted efforts. As Evelyn Fox Keller (2014a:2428) argues:

If much of what the genome “does” is to respond to signals from its environment, then the bifurcations of developmental influences into the categories of genetic and environmental, or nature and nurture, makes no sense. Similarly, if we understand the term environment as including cultural dynamics, perhaps neither does the division of biological from cultural factors.

Reflecting elsewhere upon the conflicted history of biology and anthropology during the last century, Keller finds that going beyond the biological/cultural divide “is not going to be so easy” (Keller 2016a:38). Yet, what other good alternative do we have? Understanding the complexity of humans, who are integrated biological–socio–cultural systems, will require a better integration of the natural and human sciences.

Until we make further progress along these lines, we should be wary of scientific hype in the area of child development. As some scholars have recently pointed out, science hype (i.e., exaggerating the state of scientific knowledge in one area) hurts science, as the public becomes distrustful of the scientists who make claims beyond the evidence (Caulfield and Condit 2012). In areas in which scientific pronouncements influence matters of social policy, to go beyond the evidence is not only unscientific, it is socially irresponsible. In the area of child development, to avoid science hype, unsound policies based on unsupported claims, and ethnocentrism, it would behoove us to present our scientific findings in a way that acknowledges their often tentative and limited nature.

From the truism that neglect and abuse have serious detrimental effects for children, attachment theorists moved to erect an enormous theoretical edifice (LeVine and Miller 1990:79). But the occasional lip service paid to the existence of other types of caregiving is no longer sufficient. Critics are demanding a change in research practices through:

1. the decentering of WEIRD (western, educated, and from industrialized, rich, and democratic countries; Henrich et al. 2010) samples as the basis for theories about universal patterns of behavior, and
2. a healthy modesty about the use of speculations regarding adaptations and the role of these speculations in constructing a theory about human nature that has enormous implications for the lives of people all over the world.

Both scientific standards and social responsibility require attachment researchers and all of us to be more humble.

Reviewing Bowlby’s 1969 book shortly after it was published, Maas (1970:414) already noted:

What we need is not more dogma but verifiable patternings (sic) of influences upon man’s early—and later—development. Ill-validated explanations may be seized upon all too quickly for prescriptive application. Theory, especially in this field of inquiry, should be developed, tested, presented, and reviewed critically.

Almost 50 years later, those words still hold true.

Acknowledgments

I am grateful to Juan Ilerbaig, Mark Solovey, Gilda Morelli, Robert LeVine, and Naomi Quinn for suggestions to improve this paper. I also benefited from discussions at the Forum and from comments sent by various participants.