

The Organization of Attachment Relationships

Maturation, Culture, and Context

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PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE
The Pitt Building, Trumpington Street, Cambridge, United Kingdom

CAMBRIDGE UNIVERSITY PRESS

The Edinburgh Building, Cambridge CB2 2RU, UK <http://www.cup.cam.ac.uk>
40 West 20th Street, New York, NY 10011-4211, USA <http://www.cup.org>
10 Stamford Road, Oakleigh, Melbourne 3166, Australia

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First published 2000

Printed in the United States of America

Typeface New Baskerville 10/12.5 *System* Quark XPress™ [HT]

A catalog record for this book is available from the British Library

Library of Congress Cataloging-in-Publication Data

The organization of attachment relationships : maturation, culture,
and context/edited by Patricia McKinsey Crittenden, Angelika Hartl Claussen.
p. cm.

Includes bibliographical references.

ISBN 0 521 58002 1 (hardbound)

1. Attachment behavior. 2. Attachment behavior in children.

I. Crittenden, Patricia McKinsey. II. Claussen, Angelika Hartl.

BF575.A86.074 2000

155—dc21

99-33726

ISBN 0 521 58002 1 hardback

CHAPTER ONE

Introduction

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Every theory faces occasional watershed periods. Attachment theory successfully negotiated one about 20 years ago after the publication of Ainsworth's *Patterns of Attachment in Infancy* (Ainsworth, Blehar, Waters, & Wall, 1978) and Bowlby's third and final volume of *Attachment and Loss* (1980). The central issue at that time was to establish attachment as an empirically and theoretically sound domain of study. Bretherton and Waters' monograph of the Society for Research on Child Development, *Growing Points in Attachment Theory and Research*, could be considered evidence of the successful resolution of that issue (Bretherton & Waters, 1985).

The outcome of the research that followed these volumes has been a consolidation of central principles and empirical findings. Specifically, the universality of Ainsworth's three patterns across a variety of cultures is well established, as is the relation of anxious attachment to risk conditions and developmental problems. Moreover, as Bowlby proposed, the effects of attachment are observed across the entire life span, and there is evidence of continuity of pattern across both individual lives and generations. There have, of course, been disputes about numerous topics, for example, the role of temperament, cultural variation, and the best way to describe the patterning of at-risk children. Nevertheless, there is now a large body of attachment research and theory on which a different sort of research program for the future can be built.

Today we may be in another watershed period. The very success of the work from the mid-1970s to the present and the broad acceptance of quality of attachment as a critical developmental variable mean that researchers are applying attachment theory and assessments to samples not seen in infancy, to clinically extreme samples, and to samples in an increasing variety of cultures. Successful negotiation of this expanded interest may require concurrent expansion of theory and methodology in ways that focus on differentiation rather than unity (van IJzendoorn, 1990). Such a change in perspectives is, in fact, quite common in science; see, for example, Lesch

(1996) and Mazumdar (1995) for discussions of the role of alternating patterns of unity/continuity and differentiation/discontinuity in scientific thought. Attachment now seems to hold great promise for a great variety of researchers and mental health practitioners. That promise may best be fulfilled if we venture away from our secure base of shared knowledge and explore different perspectives vigorously.

This volume attempts to effect that expansion by focusing on three issues: culture, maturation, and developmental context. These issues are quite similar to those proposed by Belsky (1995) with regard to the ecology of human development. That is, Bronfenbrenner's ecological model (Bronfenbrenner, 1979) addressed the issues of *how* developmental processes and context interact to shape both individual lives and, reciprocally, the contexts in which lives are lived. Belsky focused on the unstated central question of *why* these processes functioned as they did and proposed that evolutionary theory could illuminate the issue. Although both questions are important, the question of why we organize, mentally and behaviorally, as we do is central to the chapters in this volume that are devoted to theory. The answer that is offered is drawn from an extension of Bowlby's work. It is proposed that danger, including the need to prepare for and respond to danger, is the central organizing principle around which strategies for self-protection are organized.

The empirical chapters focus more directly on how this organization occurs. The selection of contributors expands the range of perspectives on attachment. The contributing authors have been selected from a wider range of countries and with fewer English-speaking investigators than usual, including, in particular, investigators from former Soviet bloc and Mediterranean countries. The intent is to expose attachment researchers to competing perspectives and to highlight a group of emerging attachment investigators. Doing so, however, creates tension between scientific methodologies and psychological/political theories. The theoretical chapters address both the common ground among these perspectives and the ways attachment theory itself might adapt to its broadening international environment. Another point of tension, tied to this broadening perspective, is the evaluative perspective implicit in attachment theory. Indeed, even the term *quality of attachment* carries implicitly the notion of good and bad qualities. Although this term will be used frequently in this volume, its preferred synonyms are pattern of attachment, attachment strategy, and (most preferably) self-protective strategy. It is proposed that attachment terminology should carry no implication of evaluation in either the moral or adaptive sense. Finally, the theoretical chapters integrate current thinking in other areas of science into the discussion of the meaning of individual differences in attachment among individuals and across the life span.

CULTURE

In recognition of the universality of the contribution of attachment to developmental processes, standardized assessments of pattern of attachment are being applied in an increasing array of contexts that are defined by culture (Marris, 1991). If, however, there is validity to Vygotsky's theory of the contribution of culture and history to development (Vygotsky, 1987) or to Bronfenbrenner's theory of the social ecology of development (Bronfenbrenner, 1979), we must account more fully for the effect of culture on the organization, function, and development of patterns of attachment. Indeed, Anglo-centrism could be an impediment to the growth of attachment theory (Takahashi, 1990). Culture, however, is hardly a neutral issue that can be resolved simply with the careful accretion of empirical data. Because of the thorniness of the problem, a brief discussion of cultural issues is relevant.

When this volume was first conceived, the clear plan was to look at attachment through the perspectives of culture, maturation, and context. Over the time it has taken to complete the volume, culture has come to stand out as the most challenging topic. In particular, the challenge seemed to be to describe each culture such that those for whom a given culture is home would nod at the description of themselves, as though to say, "Yes, that is us," while, at the same time, those outside the culture would think, "Yes, indeed, that's them!" Since then, I have traveled to all but one of the countries represented in this volume, spent time with all of the authors, and, with only two exceptions, viewed or actually coded some of the data reported here. I have packed and unpacked my bags, both concretely and metaphorically, until "home" has become paradoxically everywhere and nowhere. In the process, I have come to understand that what one sees is only partly a function of what one looks at. More intriguing is the impact of where one stands when one looks and what one has seen in the past (cf. Kelly, 1955). Time and place color observation such that it cannot be objective and be the same to everyone. Moreover, I have slowly come to wonder whether the empirical methodology that is intended to protect us from bias cannot also obscure our ability to see the bias. I fear this could be especially true in attachment research where the object of observation is a pattern of behavior rather than discrete behaviors and where it is so easy to impose both behavioral expectations and value judgments on the patterns.

For example, recently with a group of 18 Finnish, Russian, and Swedish colleagues, I played a Strange Situation videotape of an American girl whom I had offered as an exemplar of secure attachment, a B3. April was alone and facing the camera with her back to the door when her mother entered. Because of her position, we were able to see her face before she saw her mother; in other words, we saw her expectations before they were realized.

April's face changed expression as she heard the door open and I asked the 18 observers what they saw. No answers. We replayed the two to three seconds of videotape several times, and slowly the answers came: a smile, excitement, satisfaction. I played the tape yet again and narrated what I saw: stilling as she listened, closing of her mouth, pursing of her lips – in anger? – at her mother who had twice left without saying a word. No one else had seen that. After I narrated it, everyone saw it. Of course, April was also glad to see her mother, and, when she actually greeted her later, it was with joy and intimacy. So was there also a fleeting moment of anger? Can B's be angry? Was I right and did expectation bias what everyone else saw? Can we accept anger as a part of security when we have focused so intensively on the positive aspects of security?

Furthermore, when this group constructed a Swedish/Finnish reliability test, coders from each culture overidentified Type B in their own nationality and underidentified it in the foreign children. This effect has been repeated many times with different arrays of cultures. It appears that there is a confusion of normative with Type B.

These issues of what one sees and who can see it now pervade this volume. Sometimes, the issue appears to be methodologically resolvable. For example, chapter 19 presents a discussion of the widely discrepant classifications given to a single set of eight East German and Russian Strange Situation videotapes from the Ahnert et al. study by several "expert" coders. Which is the correct classification for each infant? What matters most in arriving at a classification: who trained the coder and how reliably they code, what version of the classificatory system the coder used, what culture the coder lives in or language they speak, whether the coder is "Eastern" or "Western," what the coder's personal attachment history is, and so on? We have not asked these questions, but they may matter. Possibly, as Klaus and Karin Grossmann suggested, we should hold an international seminar on classificatory procedures and resolve this issue (Personal Communication, December 1995). Or maybe new findings, new samples, and new researchers would necessarily undermine that agreement (presuming it could be reached at all). If that happened, would it be evidence that science was working or evidence of the failure of our methods?

This circumstance of disagreement among reputable coders and across boundaries of nation, culture, and training poses perplexing problems that must be faced directly if attachment theory is to represent human experience adequately. Although, in the natural sciences, standardizing units of measures has greatly improved international cooperation and empirical advance, it is also true that discrepancies have been highlighted. These then became the focus of future research and the basis for defining new measures. The difficulties of doing this in the social sciences and with observational measures are substantial, but the task needs to be undertaken with both an open mind to

what will be observed and acceptance of the notion that change in understanding is the basic process underlying research.

Other issues are less amenable to objective resolution. The longer I travel and the more often I return to each country, the more I wrestle with describing cultural variation, understanding the effects of culture, and explaining to myself why cultures vary, that is, the function of culture. I'm a psychologist, not an anthropologist, and I feel ill prepared to do this. A quick look at the anthropological literature, however, confirms that the problems of observer and observed are universal (Devereux, 1968; Geertz, 1989, 1995; Lofland & Lofland, 1984; Marsella, 1998). These problems are also relentless; ignoring them creates greater bias and misunderstanding than confronting them (Gergen, Gulerce, Lock, & Misra, 1996; Marsella, 1998). I'm also an American and, of course, a specific American with my own developmental history. When am I viewing through human eyes? a woman's eyes? American eyes? uniquely my own eyes? Needless to say, I am the one person who cannot answer these questions, but because they apply equally to all of us, they are important.

One way in which the chapters in this volume addressed the issue of culture was by having each set of authors include, in their introduction, a section on aspects of their culture and national history related to attachment and, in their discussion, an interpretation of their results as they might reflect cultural themes or influences. These sections proved to be very difficult to write, and there were many queries about how to meet this request. I had naively assumed that each of us knew our own culture best and that, before I (the perpetual foreigner) was audacious enough to address the topic, members of each culture should have the opportunity to define themselves. It was not as I expected. Indeed, I have learned at least as much about being American by viewing America from other countries and through others' eyes as I have learned about the other countries. So, although I now think that few of us are prepared to describe our own culture, each of us attempts it in this volume.

Culture is the ground we walk on, but because it is always there and invariant, we rarely see it. Only when the ground is unfamiliar, and especially when one stumbles over it, does it suddenly become foreground. This has been my experience when viewing data gathered from different cultures. Often what seems odd to the outsider is so normal as to be invisible to investigators within the culture. I have been welcomed everywhere that I have traveled, and each of my hosts has offered me an opportunity to understand their culture's way of living. Sometimes I think I understood or at least made some progress toward understanding. Other times, I saw things so differently, particularly with regard to secure and anxious attachment, that the discrepancy became painful to me and a problem for us. A particular issue became the distribution of the patterns of attachment in

different cultural/national groups. This issue appears throughout this volume and is the focus of the final chapter.

MATURATION

It is ironic that, although the great majority of attachment research has been carried out by developmental psychologists, the theory as practiced is substantially less developmental than most other developmental theories. That is, attachment researchers have paid more attention to the validation and temporal extension of the infant patterns of attachment than to the interactive effects of maturation and experience on the organization of attachment beyond infancy. It is not that the focus on infancy has not been fruitful; to the contrary, it has been very productive. Consider, for example, the important longitudinal research generated from Sroufe and Egeland's Minnesota sample and Grossmann's German samples. Furthermore, numerous other longitudinal studies of shorter duration also have increased our understanding of the long-term consequences of individual differences in pattern of attachment. The majority of such studies have in common, however, a focus on prediction from infant patterns of attachment to later outcomes. Where expansions have been made, for example, the controlling patterns (Cassidy & Marvin, 1991; Main & Cassidy, 1988), they tend to reflect observed behavior that is described rather than articulated in terms of developmental processes that are tied to neurological maturation and that have implications for continued elaboration at later ages.

It seems entirely possible, however, that the neurological limitations of the infant brain would constrain considerably the range of possible organizations of attachment in infancy. If substantially different or more complex organizations develop after infancy, longitudinal studies based on infant pattern of attachment may fail to capture these. Moreover, children live in more complex and varied environments than infants; this may lead to greater complexity, variation, and specificity of organization of attachment after infancy. Therefore, an important challenge facing attachment theory is to access other bodies of developmental theory, including, in particular, the cognitive neurosciences, to explore the possible elaboration of attachment strategies beyond infancy. Numerous other bodies of scientific thought ranging from biology to physics, astronomy, and geology are now fairly humming with ideas based on interactive processes and indeterminate outcomes. Especially as these notions affect the underlying genetics and neurology of human development, attachment can benefit from these nonlinear ways of conceptualizing temporally ordered processes.

Several of the chapters in this volume provide starting points for developmental theorizing. Rauh, Ziegenhain, Müller, and Wijnroks in chapter 14 consider the transition from infancy to the preschool years in a Berlin sample

and demonstrate how the method of assessing pattern of attachment changes, sometimes dramatically, the nature of the findings concerning developmental change. In their study, depending on the method of classifying Strange Situations of 21-month-old children, the developmental transition is one of dramatically increased anxiety or substantially increased security. Grossmann and Grossmann in chapter 2, also observing German toddlers, point to the importance of different parental roles, even in relation to a single child. In chapter 6, Lippe and myself in a study of 5- to 8-year-old Egyptian children highlight the role of gender in interaction with development in some cultures. Hans, Bernstein, and Sims in chapter 15 follow the developmental pathways of two normally developing children whose context provides too little predictability and who, in middle childhood, begin to display relatively extreme patterns that, in adolescence, are contained and reintegrated within the normative range. Their chapter highlights both the variations within the Type C pattern and the interaction of maturation with development to create a changing array of adaptive possibilities. In chapter 16, Black, Jaeger, McCartney, and myself pick this theme up in a sample of 16-year-old adolescent girls and offer data that new organizations and processes are under way in midadolescence. Again, failure to account for maturational change in our methods would result in very different findings. Soares, Fremmer-Bombik, Grossmann, and Silva in chapter 17 carry the theme to the transition from adolescence to adulthood.

CONTEXT

Two decades of research demonstrate that children in high-risk circumstances are less often securely attached than children developing in more favorable circumstances. These contextual factors are of many types: dangerous families, poverty, parental psychopathology, institutionalization, and so on. In this volume, the focus is less on the lack of securely attached children and the identification of risk factors and more on the essential qualities of contexts that produce differential effects in children's mental and behavioral organization and on the self-protective strategies that children use in threatening situations. In particular, danger in its various forms is seen as the underlying experience associated with risk, and the need to predict and prepare for danger is viewed as central to human strategic organization. Thus, rather than comparing threatened (i.e., at risk) children with nonthreatened children and finding the former lacking in security, it may be more meaningful to ask how the threatened child's strategy reduces the danger. From this perspective, there is the possibility that without new conceptualizations of patterning and strategy, the study of attachment in the context of risk will prove to be a barren promise.

To date, the primary means of thinking about attachment and psychopathology have included type A/C organization (Crittenden, 1985a;

Radke-Yarrow, Cummings, Kuczynski, & Chapman, 1985), disorganization (Main & Solomon, 1986, 1990), and, in the Adult Attachment Interview, the “Unresolved” and “Cannot Classify” categories (Hesse, 1996; Main & Goldwyn, under contract). Given the variety of human disorders and breadth and detail of clinical classificatory systems (e.g., DSM-IV, ICD10), the current interest of clinicians in attachment theory will fade if we do not adapt our constructs to match, and exceed, the breadth and specificity of those already in use. A particular contribution of attachment theory could be to emphasize developmental pathways, mental processes, and psychological functions associated with psychopathology or risk for pathology (as opposed to the symptom clusters that typify more traditional diagnostic systems). These topics, however, need considerable elaboration from their present state to be diagnostically and clinically useful above and beyond current clinical procedures.

Several chapters explore risk. In chapter 8, Moilanen, Kunelius, Tirkonnen, and I explore in toddlers the dual contexts of twin status and living in Finland, a country with extreme seasonal changes in light and living patterns. The startling finding that risk (being an unhealthy twin in the dark months) yields higher proportions of Type B attachment forces reconsideration of the effects of context on maternal and child behavior. Lis’s study of institutionalized children in Poland (see chapter 9) is a reminder of both the importance of detailed observation and the variation of effects, depending on critical differences within the context. In this case, particular adaptations within the institution moderated the expected negative effects of institutionalization on the children. Chisholm’s two case studies of Romanian children adopted by Canadian families expand this theme in chapter 10. Although the Romanian institutions had few of the advantages of the Polish setting, differences in the receiving families were tied to differences in developmental pathway for the children. In chapter 11, Teti considers the context of maternal depression and, like the other investigators, finds important differences in the range of outcomes as well as evidence of patterning not seen in infancy. Lange, Claussen, Partridge, and I (see chapter 12) consider directly the issue of danger in a sample of abused and neglected children. We find both an increasing array of patterning, as compared to the Ainsworth set of infant patterns, and evidence of dyssynchrony within a single individual’s array of internal representations when these are tied to different memory systems. The implications of this finding for behavior are considered. In the more normative context, three studies consider the effects of day care on children. The Rauh et al. study (chapter 14) is particularly interesting because it suggests that the process through which entry to day care is accomplished is significant to the outcome; in this study, age at entry was not related to pattern of attachment. In chapter 3, Fava Vizziello, Ferrero, and Musicco explore day care with somewhat older Italian preschool-aged children. They find that the parental situation is predictive of children’s adaptation, although, when the receiving

staff was attuned to children's needs, they moderated this influence. In chapter 5, Bohlin and Hagekull explore the issues of day care, attachment, and behavior problems in a Swedish sample of 4-year-olds. Their finding that entry to day care between 12 and 15 months of age was related to higher rates of Type C attachment is intriguing. Although these studies by no means close the issue of the effects of day care on young children, they do highlight the importance of maturation and cultural context on variation in effects. As Bohlin and Hagekull (this volume) and many others have noted, the effects of day care are tied to many differing factors. Sorting these out for children who differ in age, culture, risk status, and parental influence is necessary before this resource can be used consistently to benefit children.

CONCLUSIONS

Over the course of two decades, I have come to conceptualize attachment as a theory about protection from danger and the patterns of attachment as strategies for predicting and protecting oneself from danger. In this sense, I find all patterns adaptive – in the context in which they are learned. Do cultures differ in their dangerousness and in the types of danger to which individuals have been historically and are now exposed? I think so, and I address this issue in the final chapter. Here I want to point out that when we observe children with non-B patterns, we are observing children responding to the perception of danger. For the normative A1-2 and C1-2 patterns, the danger is very slight and requires only a little cramping of one's possible self. For others, however, the danger is greater, and the self is more distorted in an effort to accommodate the threat and protect oneself. Sometimes, safety cannot be achieved with any strategy. Observing children in such situations is extremely uncomfortable and a challenge to our "scientific" distance.

Recently, I viewed Strange Situation videotapes with a group of researchers studying the effects of maternal drug addiction on children. In one tape, there was a 4-year-old boy who seemed extremely independent and distant from an intimidatingly withdrawn mother who exuded hostility. However, when he was left alone for three minutes, this seemingly competent, independent little boy fell apart. After a brief and silent search around the room, he sank to the floor in a corner, his body curled almost foetally and his wide-open mouth emitting intermittent and eerie wails into the vacuous space. Suddenly, his teeth started clacking together as he repeatedly whispered the words, "I want my mommy"; his lips did not move. When the stranger entered, he cried more loudly and fixed his gaze directly on her. Following the directions given to her, she tried to "jolly" him out of his crying: "Come on, you're a big boy," "You're not really crying – you don't even have tears!" When I asked my colleagues how the boy felt, they said he began with genuine distress but ended up pretending to cry; he was probably a Type C child

feigning helplessness to coerce adults into taking care of him. Many questions later, I admitted to being astonished that no one felt his unspeakable agony as being beyond tears or thought that he needed, and deserved, to be held comfortingly, even if only by a stranger. I was even more surprised when everyone in the room, including especially the stranger, agreed and was openly relieved to discover that they were permitted to feel (for him and for all observed children). Where do scientists put their feelings when they work? Where should we put them?

Suffering deserves a voice, and attachment theory, by its very subject matter, is supremely suited to the task, whether it is for a child whose hope is dying, a mother whose hope died years ago, or a whole culture struggling with persistent threat. At the same time, we must acknowledge the uncertainty of knowing what we see. Each of us speaks with many voices and sees through many eyes. Failing to acknowledge this is fooling ourselves and deceiving our colleagues. Thus, there is a tension in this volume, one that I hope can be left as productive uncertainty that may lead to thoughtful reorganization of our theory and methods.

To understand anxious attachment, I think we must both understand a child's situation and feel for him or her. When assigned with informed compassion, an anxious pattern of attachment does not describe inadequacies, but rather acknowledges a child's attempts to cope with the challenges of his or her world. Knowing what children can do to protect themselves is at least as important as knowing how "securely" they would act if they experienced no threat. Indeed, so little effort is needed to survive safety that even the foolish, infirm, and stupid can manage the task. On the other hand, it may be that only the intelligent, capable, prudent, and wary can survive danger. The point is that I fear that we have taken a deficit approach to thinking about "anxious" attachment. No wonder no one wants their child, family, or culture to be associated with one of the anxious patterns. Recognizing the accomplishment and adaptation implied by the non-B patterns and placing them in the ecological context of family, culture, and history can help us understand human relationships better and change the negative value placed on the Type A and C patterns. From this perspective, I recommend John Bowlby's original advice to Mary Ainsworth that she name the infant patterns A, B, and C until she knew what they meant. Possibly, in spite of all our existing attachment research, we do not yet fully understand the meaning of the patterns, the full range of meaningfully different subpatterns, or the full implications of the interactions of history, maturation, context, and person that produce the patterns. By increasing the application of the attachment paradigm to more countries and cultural groups, we may reach a better understanding of the range and meaning of human variation and be given the opportunity to refine our theory and methods to reflect that variation more nearly.