W. Koops, J.B. Hoeksma & D.C. van den Boom

1. Stagnation and Progress in Research of Early Mother-Child Interaction and Attachment

Abstract

This chapter offers an introduction to the central theme of this book and elucidates the structure of the book. It is stressed that the traditional attachment research approach follows traditional psychoanalytic assumptions on the decisive role of the mother as well as of early experiences. It is argued that empirical support for the sensitivity-attachment hypothesis is still not definite, neither logically, nor methodologically. Three non-traditional alternatives are offered: experimental research on basic mechanisms and basic developmental processes; bio-ecological research; and analyses of age related changes. Examples from these approaches are discussed: investigating postural control and visual attention at the base of sensitivity, analyzing the ecological context, studying mother-child interaction from a dynamic systems perspective, and studying attachment and mother-child interaction from a Wohlwillian approach. It is described how this book has been structured within this general context of traditional and non-traditional approaches of early mother-child interaction and attachment.

1. Introduction

Early caregiver-child interaction is a classical object of pedagogical and developmental psychological theorizing. For more than two centuries an abundance of speculative theories and firm convictions based on it have been developed. Pedagogical advice and sincere warnings to young mothers (and fathers) have been written down by concerned pedagogues. A cultural historically important example can be found in the First Book of Rousseau's Emile (Rousseau, 1762), devoted to education and development from birth to the second year. Rousseau explained that it is the mother who should nurse her own child, and that 'there is no substitute for maternal solicitude' (Rousseau, p.45). He wrote - for the first time in Western history - that children's crying has a communicative function, and that reading the child's tears is of the utmost interest, because from these tears '.... is born man's first relation to all that surrounds him; here is formed the first link in that long chain of which the social order is formed' (Rousseau, p.65). It is not too difficult to spot the continuity in Rousseau's ideas and those of the most influential twentieth century child psychiatrist, John Bowlby. (This historical continuity was described earlier by Koops & Kalverboer (1987)). In modern developmental psychology the mother-child relationship still has the same actuality as in Rousseau's 18th century France. But nowadays it is not only a topic of speculation, but also a matter of empirical analysis.
In 1986 the Dutch Ministry of Education acknowledged the importance of fundamental research of caregiver-child interaction and invested Dfl 3,000,000,- for a nation-wide Dutch research project, called ‘Experimental longitudinal research on caregiver-child relationships’ (the Dutch title led to the acronym ELO). The basic goal was to experimentally test the core hypothesis of classic attachment theory: the sensitivity-attachment hypothesis. This hypothesis states that mother’s sensitive responsiveness determines attachment security (see section 3 and 4). Part of the inspiration for the study was drawn from the successful study by Riksen-Walraven (1977; 1978), summarized in Riksen-Walraven & Van Aken. Riksen-Walraven devised two intervention programs: the Stimulation-program and the Responsiveness-program. In both cases mothers of 9 month old babies received a booklet with information on the development of 9- to 12-month-old children. In the S-program it was emphasized that ‘the babies learn a lot from what they see, hear and feel’, in the R-program that ‘babies learn most from the effects of their own behavior’. The effect of the programs was that parents from the experimental groups showed increased stimulation (S-program) of their babies and turned out to be more responsive (R-program). The S- and R-programs had positive effects on the children’s competence motivation. According to most participants in the ELO project these results could be considered as (indirect) empirical support for the sensitivity-attachment hypothesis. ELO was intended as an experiment to manipulate mother’s responsivity and thereby the attachment security (to be measured with the Strange Situation procedure). Therefore the experimental manipulation was predominantly based on Riksen-Walraven’s R-program: mothers from the experimental condition were offered the booklet from the R-program to increase their responsivity and thereby attachment security. Theoretically, secure attachment should have positive consequences for the long term social and cognitive development. Therefore in ELO some theoretically relevant dependent variables were longitudinally measured at the children’s ages of 12, 18 and 30 months.

Departments of Developmental Psychology of four Universities were involved. From each of these locations a professor of developmental psychology was appointed as a member of the ELO-directorate, and at least one Ph.D. student executed a dissertation research project. At each location a specific problem group of babies was investigated. At all locations the same basic research design was used: a pretest-posttest control group design (Campbell & Stanley, 1966) with random or matched random assignment to the experimental and control conditions. Table 1 summarizes the detailed information about the Universities, the directors, the (former) Ph.D. students and the specific (problem-) groups of babies.

Intensive theoretical and methodological discussions within the ELO group led to certain differentiations between the four locations.

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1 Names in bold type refer to the authors of chapters in this volume. Quotations from these chapters are - for practical reasons - without references to the exact page(s).
Table 1. An overview of participants, samples and dissertations of ELO (Experimental Longitudinal Research on caregiver-child relationships; a nationwide project in the Netherlands)

<table>
<thead>
<tr>
<th>Universities</th>
<th>‘Directors’</th>
<th>Former Ph.D. students</th>
<th>Subjects</th>
<th>Dissertations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Free University Amsterdam</td>
<td>W. Koops</td>
<td>J.B. Hoeksma</td>
<td>Children with cleft lip and palate</td>
<td>Hoeksma &amp; Koomen (1991)</td>
</tr>
<tr>
<td>Catholic University of Nijmegen</td>
<td>C.F.M. van Lieshout</td>
<td>J.Th. H. Mey</td>
<td>Low SES-children</td>
<td>Mey (1992)</td>
</tr>
<tr>
<td>Utrecht University</td>
<td>R. Hoksbergen</td>
<td>L. Wijnroks</td>
<td>Preterm children</td>
<td>Wijnroks (1994)</td>
</tr>
<tr>
<td>Groningen University</td>
<td>A.F. Kalverboer</td>
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For instance the Free University of Amsterdam group had to decide on the basis of theoretical as well as empirical analyses that experimental induction of attachment security was not justified for their particular group of (CLP-) babies (see Hoeksma, Koomen & Koops, 1987). It is not our intention, however, to offer a detailed account of the ELO-project in this chapter.

The ELO project was successfully completed: the most detailed overview of the results can be found in the five dissertations: Hoeksma & Koomen, (1991)², Mey (1992), Juffer (1993), Wijnroks (1994). Lastly it should be mentioned that two senior researchers played an important role in the training and supervising of the young researchers preparing their dissertations: Dr Riksen-Walraven (her inspiring work has already been mentioned) and Dr van den Boom, who was the first researcher in The Netherlands who conducted an experimental investigation of the sensitivity-attachment hypothesis (van den Boom, 1988). Dr van den Boom, a licensed trainer also fulfilled the essential task of training the investigators and their assistants in scoring the observed behavior in the Ainsworth Strange Situation procedure.

In this book some of the results of the ELO studies are summarized (Juffer, Rosenboom, Hoksbergen, Riksen-Walraven & Kohnstamm; Wijnroks & Kalverboer; Koomen & Hoeksma) as well as ELO-related theoretical and methodological analyses, empirical demonstrations, and discussions of the new and alternative approaches of the research questions of the ELO-project (Riksen-Walraven & Van Aken; Van den Boom & Hoeksma; Kalverboer; Hoeksma, Van den Boom, Koomen & Koops).

In May 1995 an international colloquium was organized³ to present the results of the ELO-project to the international community of developmental psy-

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² This book contains two dissertations, both authors earned a degree with their contribution to this common book.

³ With financial and organizational support of the Royal Netherlands Academy of Arts and Sciences.
chologists, and to discuss theoretical and methodological implications with internationally recognized experts in research of early mother-child interaction and attachment. Several of these experts contributed papers to the colloquium and to this book. In this chapter we will now try to outline the structure of this book, and the relations between contributions. In addition, we offer some comments and conclusions.

Section 2 is devoted to what may not be forgotten to be the original background of the attachment theory: psychoanalysis. Section 3 will analyze the limitations of the traditional approaches resulting from this background. In the next two sections examples of these biases, as well as alternatives for existing research strategies, will be discussed. In section 4 the focus is on the sensitivity-attachment hypothesis and on the mother-centeredness of much of the attachment research. The lack of interest in developmental change is the subject of section 5. Finally, in section 6 some general conclusions on the future of mother-child interaction research will be drawn.

2. The psychoanalytic background of attachment theory

It is rather exceptional in developmental psychology that a research procedure survives more than two decades. In that sense Mary Ainsworth may be considered as the founder of perhaps the most successful research procedure in the field of early social development: The Strange Situation. The procedure has been so successful that alternative research approaches of caregiver-child interaction have been largely marginalized.

Our first question pertains to the success of the Ainsworth Strange Situation as such. Why has this paradigm been so successful in all parts of the world for such an exceptionally long period? Our answer is that attachment theory is filling in the most important gap in the - regretfully - most influential psychological theory: psycho-analysis; and that the Strange Situation is considered as the research tool to obtain the highly needed empirical scientific support. We will now first summarize our arguments, indicating that for understanding the origin of attachment research, one cannot suffice with referring to biology/ethology, but indeed should go back to Sigmund Freud.

It is well-known that Freud introduced in 1896 the idea that neurosis is an effect of, as he put it, sexual seduction by an adult in the first ten years of life. However, it is less well-known that only four years later Freud discovered that the shocking stories told by patients about their childhood were completely unfounded. The stories were not true, they had been made up. The Dutch psychiatrist Van den Berg expressed this as follows: ‘The psychotraumata which had resulted in neurotic disturbances appeared to have the unexpected and certainly undesirable quality that they had never happened’ (Van den Berg, 1964, p.51t).

What could Freud do to save his theory? - if not: to safe his face? He withdrew slowly, putting the causative factor of the

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* Van den Berg’s book on ‘Dubious Maternal Affection’ was first published in Dutch in 1958; the historical analysis in this paragraph has been based on this book, literal quotations are translations of the fourth reprint of 1964; the American translation (Van den Berg, 1972) was not available.
neurosis even further in the past. An individual does not become neurotic during childhood, but much earlier. To quote Van den Berg again: 'The child is born as a potential neurotic. He begins with an hopeless program; he is a sadist, a masochist, an exhibitionist, a fetishist and a homosexual' (Van den Berg, 1964, p.52). But this move was not easily accepted. For many psychiatrists and for laymen as well, it was hard to believe that neurosis originates from happenings in primeval times, and/or to consider the child polymorphously disturbed. And Freud himself too found it difficult to consistently stick to this idea: since 1900 he alternatingly wrote that the child was in principle a born neurotic, and still came up occasionally with his original psychotrauma theory. A particularly difficult phenomenon for Freud’s sexual psychotrauma theory was the so-called ‘shell shock’. This concept refers to a wide variety of serious neurotic disorders during the First World War. It was impossible for Freud and his followers to link these disorders to infantile sexual traumata. It was too evident that the war neuroses had to do with mourning and loss of loved persons in the present (see Newcombe & Lemer, 1982). By and large Freud’s life-long contradictory statements made the origin of neurotic symptoms an inaccessible chapter of Freudian theory until his death in 1939 and even thereafter. The ‘desertion of the psychotrauma’ - as Van den Berg coined it - left a serious gap in the very foundations of psychoanalytic theory. And it was precisely this gap that was filled by the discovery of hospitalism, and the subtleties of mother-child attachment.5

Hildegard Hetzer - founder of developmental psychology in Austria- coined the concept of Hospitalismus (German for hospitalism) already in 1927. The concept was accepted by the scientific community after René Spitz had published his influential, but also very disputable paper on hospitalism in the 1945 edition of 'The Psychoanalytic Study of The Child'. After publication of this paper the idea was generally accepted that the young child only flourishes if cared for by a devoted and loving mother. Spitz’s data did not add much to what had already been published in the twenties and thirties (in German; see for references Van den Berg, 1964). New and revolutionary were his extremely dramatic tone and his emphasis on the seriousness of the consequences of hospitalism in the first year of life. According to Spitz, hospitalism, that is, the lack of love in the first year of life, leads ‘....practically without exception to unsociability, criminality, debility, insanity, and neurosis’ (Spitz, 1945). ‘All the great evils that can ravage human life are the effects of a coldness during the first year’, summarizes Van den Berg (1964, p.15). Spitz’s conception was heavily supported by a now classical WHO research report by

5 Newcombe and Lemer (1982) show that it was Melanie Klein, Bowlby’s supervisor at Maudsley Hospital in the early thirties, who created a solution for the embarrassment of Freudian theorists by suggesting that mourning might be linked to separation anxiety in early childhood. She proposed infantile interactions with mother as another possible source of traumata, together with or instead of Freud’s infantile sexual psychotraumata (Klein, 1935; 1940).
John Bowlby on 'Maternal Care and Mental Health' (Bowlby, 1951). From a research-methodological point of view Spitz's and Bowlby's papers are disputable. A detailed critical review of Spitz' work was written by Pinneau (1955). More recent and still highly critical reviews were written by Van den Berg (1958) and Rutter (1972; 1979) (see also Koomen & Hoeksma). Despite all the very convincing counter-argumentation, however, Spitz's and Bowlby's conclusions were (and still are?) eagerly accepted by well-trained academics. Why was (is) that? We are convinced that this was because these scholars saw in Spitz's and Bowlby's papers a new conception - that of hospitalism - which filled in the gap in the foundations of Psychoanalysis, which was caused by the desertion of the psychotrauma. Of course, in the long run theoretical needs are not sufficient to let methodologically disputable data survive. So there grew a worldwide interest in the creation of a research tool to empirically substantiate the hospitalization/attachment theory. Ainsworth and Wittig's creation of the Strange Situation procedure in 1969 precisely offered such a tool to measure individual differences in the patterning of attachment behaviors.

It is important that historical research like that of Van Dijken en Van der Veer offers detailed and well-documented insights in the continuity in Bowlby's theoretical ideas. The first information they offer in their contribution to this book is strongly supportive of the view that there is much continuity between the early Bowlby (on hospitalization) and the later Bowlby (on attachment) (see also Newcombe & Lerner, 1982). We will wait for more interesting information from this research project. In the meantime we will demonstrate in the next section that it is likely that some of the persistent biases in the research questions, and in the methodology of the investigations aimed at answering these questions, are to be considered as resulting from some of the original psychoanalytic biases.

3. Traditional approaches

The Strange Situation allowed for an analytical empirical approach of Freudian theorizing, thereby strongly furthering international communication and exchange of empirical data. However, this virtue turned out to be a drawback. Indeed, the Strange Situation led to what Connell and Goldsmith (1982) labelled an 'hour-glass methodology', referring to the narrow bottleneck of the Strange Situation through which too many research questions are squeezed. In this paragraph we will discuss three restraints imposed on attachment research by the original psychoanalytical background.

Let us first look at one of the core concepts of the attachment theory: 'sensitive responsiveness'. Since the well-known Baltimore study it has been repeated many times that '...the most widely accepted aspect of ethological attachment theory is the notion that infant-adult attachments arise from the early interaction' (Waters, Vaughn & Egeland, 1980). The Baltimore Study showed a significant relation between early maternal sensitivity and attachment classification of the child measured later in the Strange Situation. To quote Belsky: 'a cottage industry' developed seeking to replicate - or refute - Ainsworth's findings. Belsky's chapter offers
a review of the relevant studies. However, there are some logical and methodological problems with the products of this industry. Available evidence for the tenability of what we call the sensitivity-attachment hypothesis is still disputable. 

The hypothesis states that sensitive responsiveness is the main determinant of attachment security. Therefore there should be covariation between both variables; empirical data indicate that this covariation exists. However, these data do not logically permit the conclusion that therefore the hypothesis is true. (If p implies q and q is true, than p can still either be false or true). For inferring cause and effect three conditions should be fulfilled. The first one being: there should be covariation between the supposed cause and effect. This evidence is available. The second one is: cause and effect should be ordered in time correctly. As Van den Boom and Hoeksma demonstrate the evidence on ordering in time is fixed by design. The third condition is: a proper inference about cause and effect should rule out alternative explanations. Lamb, Thompson, Gardner, Charnov and Estes (1985) and Hoeksma et al. offer ample examples of possible alternatives.

Why are so many researchers so eager to conclude that the existing evidence is in favor of the sensitivity-attachment hypothesis? Why has the common research design with respect to ordering in time been limited to investigating the effect of mother’s early sensitivity on later attachment behavior of the child? Lamb et al. (1985) asked, more than ten years ago, to supplement this common design with observations of the child’s behavior at an early age and the caregiver’s behavior at a later age. But this kind of supplemen-
tary data is still lacking. And why do so few researchers feel the need to rule out alternative explanations? It is our conviction that the original psycho-analytic perspective, deeply rooted in Western culture, still conditions even competent and creative researchers of our days, if not all of us, to find and accept evidence for the causal relation between early maternal deprivation (lack of sensitivity ) and optimal child development.

Our second point pertains to the observation that most attachment researchers assume that the quality of the mother-child interaction is mainly dependent on the mother’s contribution to the interaction. As far as the infant affects the quality of the interaction, it is - since the Baltimore study - thought to be reflected in the sensitivity of the mother. In the attachment literature there is hardly any research in which mother and child effects are disentangled. This in spite of excellent statistical tools, designed for this purpose. Van den Boom and Hoeksma express this conclusion by stating that the research on sensitivity is unidirectional or at best ‘implicitly bidirectional’. Why is there so little interest in the direct investigation of the relative input by mother and child in the interaction? Again we assume this being a consequence of the original psychoanalytic hypothesis: it should be the mother who determines the child’s development and not the other way around.

Our third point pertains to the lack of theory and instrumentation for understanding and investigating developmental, i.e., age related change. The Strange Situation procedure offered the possibility to measure a central construct referring to mother-child interaction, i.e., the quality of attachment, and to relate it to
all kinds of antecedents and consequences. When mention is made in the literature of 'the development of attachment', it is mainly this type of research which is referred to. In this way, however, development 'is reduced to a static structure' - to express it in the language of Van Geert. Very little attention has been paid to what may be regarded as the core question for research of developmental processes, i.e., the one concerning developmental change of attachment over time. As is well-known: the Strange Situation procedure cannot be used to study attachment behaviors before the end of the first year of life, because the stress-inducers, stranger anxiety and separation distress, become effective during the second half of the first year of life. But with some imagination one can of course come up with alternative operationalizations, to be used for measuring earlier manifestations of attachment-like behavior. Why have these alternatives hardly been developed until now? Why is there so little interest in the developmental course of attachment behaviors? We suppose that by now the reader will consider this question a rhetorical one.

In the next two paragraphs we will analyze these three main problem areas of traditional attachment theory. Since adherents of the sensitivity-attachment hypothesis attribute sensitivity to the mother (not to the child), we will analyze these two biases jointly in section 4. Developmental issues will be discussed in section 5. In each paragraph we will try to demonstrate firstly that even in the excellent papers for this colloquium there are still signs of the traditional biases and secondly that alternative theoretical and methodological approaches are available.

4. The sensitivity-attachment hypothesis

In this section we concentrate on experimental evidence for the sensitivity-attachment hypothesis. In section 4.1 we will evaluate the findings from the ELO-dissertations and from a recent meta-analysis on experimental studies. In section 4.2 we will review non-traditional alternative approaches of the core questions of attachment theory. We will conclude in section 4.3 that future progress is dependent on the exploitation of bi- and multidirectional (systemic) approaches.

4.1 Experimental evidence

Belsky specifies the 'classic' perspective of attachment theory as follows: 'the long-term course of the relationship is judged to be disproportionately influenced by the mother, as the more powerful agent in the relationship'. This quotation nicely describes the basic unidirectional position of attachment theory. And of course this position is in accord with the original unidirectional, mother-blaming, psychoanalytic roots. The plausible relationships looming up from the abundance of data (Belsky & Cassidy, 1994; Belsky) offer a coherent picture of the state of the art. However, the data still should be tested in such a way that logical alternatives for the unidirectional interpretations can be excluded. This is the final step on the way to justify causal inferences. Often, experiments can help out. Let us carefully look therefore at the existing experimental evidence.

First we will consider the experimental studies, carried out within and related to the ELO-project. Most of these experiments have not yet been published inter-
nationally, therefore they could not be included in Belsky’s scrutiny. We will only summarize the relevant findings, as far as these are not described in the chapters by ELO participants in this volume.

In spite of the original aims of the ELO project, Hoeksma and Koomen decided, based on questionnaire data, not to implement an intervention program (like Riksen-Walraven’s R-program, discussed in section 1) with their group of infants with cleft lip and palate (Hoeksma, Koomen & Koops, 1987). They did not find any differences between a group of infants with cleft lips and/or palate and a group of controls with respect to parents' reports on ‘recognition and interpretation of signals’, ‘response to signals’ and ‘perceived insecurity’, and found it unjustified to formulate specific hypotheses about the attachment development of children born with cleft lip and palate.

Riksen-Walraven’s studies (Riksen-Walraven, 1977; 1978; Riksen-Walraven & Van Aken) did not include a measure on attachment; her studies demonstrate that it is possible to influence mothers’ responsivity to and stimulation of her child, and possibly certain positive effects on the child’s behavior, but no conclusions about the attachment relationship can be drawn. Three ELO studies (Mey, 1992; Juffer, 1993; Wijnroks, 1994) applied Riksen-Walraven’s intervention and used the attachment classification as an outcome measure. A dissertation of Rosenboom (1994) reports on a later, ELO-linked, replication of Juffer’s study. Were these studies successful in demonstrating a causal relationship between sensitivity and attachment security?

First we summarize the effects on the sensitivity of the mother: Mey and Rosenboom did not find the expected positive effects of their experimental manipulations of mother’s sensitivity. Wijnroks and Juffer only found a partial increase in sensitivity. In Wijnroks’ experiment two criterion situations were used to observe sensitive responsiveness: a situation in which mother and child are involved in ‘object play’ and a situation in which mothers have to ‘instruct’ their children (‘instruction situation’). Wijnroks found a positive effect of the experimental manipulation in the ‘object play’ situation, but not in the ‘instruction situation’. Juffer used two separate experimental manipulations (‘interventions’), like Riksen-Walraven: one existing of written information in the form of a booklet for mothers, the second existing of verbal feedback by assistants of the investigators, on videofilms with fragments of interactions of the involved mother and child. It turned out that the video version led to increased sensitivity, but the booklet version did not. Overall the results of the four experiments are not too convincing: two studies with negative findings, two with partial positive results. Furthermore, it is not implausible to suppose that the partial results of Wijnroks and Juffer could (at least partly) be the result of ‘demand characteristics’ (Orne, 1970; Koops, 1993; Juffer, 1993). The increase in mother’s sensitivity may be artificial in that subjects may not really change their sensitivity but simply comply with experimenter demands: they sense the experimenter’s expectations and behave accordingly. In Juffer’s video condition the mothers directly received verbal and visual feedback on their own interaction with their baby, while this information was much less personal (and not visualized) in the booklet version of
Juffer’s experimental manipulation. So, the clearest ‘demand characteristics’ were to be found in the video version, that indeed was more effective. Furthermore, in Juffer’s experiment, the same assistant was present in the pre- and posttest situation, which may have served as an extra cue to remember the experimenter’s expectations. The effect of several of these factors is well-known from the classic study of Rosenthal and Rosnow (1969).

In Wijnroks’ case comparable design aspects exist: the video demonstrations he gave on object play were personalized and concrete, the demonstrations on ‘instructions’ were less personalized and more abstract.

What do the experiments teach us about the indirect effect on the quality of attachment? In three of the four studies (those of Mey, Rosenboom and Wijnroks) no effect on security of attachment was found. Only in Juffer’s study a positive effect on attachment security was obtained in one of the two conditions of the experiment, that is in the video condition, not in the ‘booklet’ condition.

The most convincing experimental support for the sensitivity-attachment hypothesis has been offered by Van den Boom (1988; 1991; 1994). Because Van den Boom used an elegant four group design adapted from Solomon and Lessac (1968) she was able to control for effects of pretesting. Interestingly enough she found a significant pretest-effect on several relevant dependent variables (f.i. in families in which pretest observations were performed mothers were significantly more visually attentive). This finding once more stresses the importance to check for subtle experimenter effects. On the other hand Van den Boom’s data are convincing with respect to the effect of her intervention on the sensitivity of the mothers from her experimental group as well as on the attachment behavior of the children. The only difficulty is however, that we cannot easily generalize Van den Boom’s findings to any other group of children then her first-born, irritable infants from lower-class families. It is interesting to note that Van den Boom found a base rate of 28% of securely attached infants. This is exceptionally low and demonstrates the extreme ‘at risk’ character of Van den Boom’s sample as well as the consequence that her study was not plagued by ceiling effects.

In a recent review paper Van IJzendoom, Juffer and Duyvesteyn (1995) presented a meta-analysis of 12 intervention studies. Part of these studies aimed at changing mothers’ representations (‘working models’) in long term therapeutic interventions, partly aimed at changing mothers’ sensitivity by short term preventive interventions. The results of the meta-study show that interventions

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6 In a recent review paper, Van IJzendoom et al. (1995) wrongly considered Mey’s (1992) intervention study as a replication of Van den Boom’s study. However, the studies differ in two essential aspects: Van den Boom studied irritable children from lower-class families, Mey studied normal children from lower-class families; furthermore Van den Boom’s intervention program differed in several respects from that of Mey. The paper of Van IJzendoom et al. suggests that Van den Boom’s results are less convincing because Mey could not ‘replicate’ them. This is not correct, the authors should have concluded that Juffer’s results (by the authors accepted as supporting the sensitivity-attachment hypothesis) should be relativized, since neither Mey (1992), nor Wijnroks (1994) could replicate the findings. The latter three studies used the same intervention programs, as well as the same research designs, agreed upon within the ELO-project.
succeeded in positively affecting parental sensitivity, but the effect on attachment security turned out to be negligibly small. Furthermore the therapeutic, cognitive, interventions were far less successful than the preventive, behavioral, interventions. And last but not least: ‘effectual changes at the behavioral level may not necessarily lead to changes in insecure mental representations of the parents involved.’ (Van IJzendoom et al., 1995, p.153) These results once more fit in quite well with the demand characteristics interpretation: interventions with respect to specific sensitive responsive behavior inform better about the kind of behavior the mother is expected to display in the criterion situation than more general cognitive therapeutic interventions; complying with demand characteristics does not really change the mothers’ behavior and therefore hardly has a positive effect on attachment security, nor on the mothers’ insecure mental representations.

We conclude that it is still not justifiable to infer from the existing experimental studies that sensitivity is the, or even a, variable causing attachment security. Van IJzendoom et al. appear to be in agreement with this conclusion by writing: ‘Sensitivity is, however, not a necessary nor a sufficient condition for attachment security’ (Van IJzendoom et al., 1995, p.153). We add that even the direct effect of the intervention on mothers’ actual sensitive behavior may be no more apparent than real.

4.2 Alternative approaches

Several proposals for alternative approaches have been done for a long time. Van den Boom & Hoeksma offer a nice overview of the already existing methodological alternatives for the unidirectional, mother-centered attachment approach: suffice it here to refer to what they have reviewed under the headings of ‘The bidirectional approach’ and ‘The Systemic Approach’. The models Van den Boom and Hoeksma discuss have been relatively less successful than the traditional attachment approach. This is for sure not the consequence of statistical/methodological problems. To mention just one example: Gottman and Ringland’s (1981) bivariate autoregressive time series model was introduced more than 15 years ago. It is an excellent tool to analyze the relative input of the mother and the child in an ongoing series of interactions. But we know of only one attempt to apply this method to the area of sensitive responsiveness and attachment behavior (Hoeksma & Koomen, 1991). This also holds for more recent developments in the applications of nonlinear dynamic systems models. Even Van Geert, one of the most productive advocates of these models (Van Geert, 1991; 1994), did not try to model caregiver-child relationships prior to this colloquium, from which this book is a result. The main reason may be a certain poverty of theoretical accounts of caregiver-child interaction, accounts that involve the richness of the data from the attachment theoretical tradition, but are at the same time innovative enough to ask
new precise questions about the multiple pathways to social relationships, about the contributions of several significant others in the child’s social network, and about developmental processes. We will shortly comment on four contributions to this book, that offer alternative ideas for the sensitivity-attachment hypothesis.

Let us first look at a proposal by Kalverboer. Kalverboer suggests that we trade in the global concept of sensitivity for well-defined ‘mechanisms involved in early caretaker-infant interaction’. His preferred candidate is visual attention. Kalverboer’s reasoning is as follows. Visual attentional processes are crucial in early interaction and learning processes: orienting reactions indicate children’s focus of interest; mothers pay a lot of effort to attract children’s attention; exploratory activity develops out of face to face contact: via mutual gazing the infant’s attention is shifting from the mother’s face to more remote aspects of the environment. Impairment of visual attention would have adverse consequences for the mother-child bond.

The study of such a basic mechanism is very attractive: it generates precise questions of the phenomena to be observed and it offers a possibility to analyze the central mechanism of sensitive responsiveness. However, there are some complications in making the choice for the relevant phenomena involved in the so-called mechanism. Kalverboer himself suggests as alternative causes for the impairment of the orienting behavior: lack of postural control, impairments in oculomotor functions, visual acuity or attentional capacities. Some researchers consider postural control as an even more basic mechanism (Hopkins, personal communication). Van Beek (1993) reports some evidence for linkages between the development of head and arm postures and looking behavior. Fogel et al. (1992) studied the effect of postural position of 3-to-6-month-old infants on the duration of gaze at mother. But still a lot of research has to be done before we can understand the functioning of the central mechanism Kalverboer is suggesting.

An interesting approach of the cognitive side of attachment theory, the so-called working model, is offered by Harris. He makes clear how the research tradition, referred to as ‘The Child’s Theory of Mind’ can be used to study the early development of the child’s ‘working model’. This indirectly offers an interesting research line to study the cognitive aspects of the bidirectional relationship between mother and child. From research on the child’s theory of mind it is clear that by three years of age, children recognize the goal-directedness of many human actions. They can understand that a caretaker will be happy or distressed depending on whether these goals are met. It is clear that this early cognitive competence complicates the caregiver-child interaction. Merging the two research traditions could elucidate how basic cognitive representations develop against the background of early caregiver-child interactions on the one hand, and how the growing Child’s Theory of Mind produces consistency and continuity in the child’s social interactions on the other hand.

A general alternative theoretical approach of attachment theory is offered by Thompson. Thompson just turns the attention away from the original psychoanalytic roots to the bio-ecological perspective. The consequences are tremendous. First of all ‘attachment patterns are viewed as facultative adaptations that are
contextually-sensitive, biologically-based adaptations to different styles of parental care’. These adaptations then are to be examined in their cultural variations, and are considered as changing over time, in function of ecological changes and the child’s own development. This bio-ecological view is free of the original biases: sensitivity is reduced to one of the possible determinants, the mother is seen as just one element in a complex social network, continual change is conceived of as a consequence of changing ecological conditions. However, the weak side of this conceptualization is the lack of basic ecological knowledge, and the difficulty to select relevant variables on the basis of such knowledge. First of all we need - to quote Thompson again - ‘...a descriptive mapping of elements of he child’s social ecology (and its changes with the child’s age) in terms of its influence on continuity and change in psychosocial growth...’.

Fogel proposes an even more general theoretical alternative. He offers the conceptualization for a dynamic relational model of the parent-infant dyad. This conceptualization is quite ambitious: it aims at ‘a model of the parent-infant relationship that preserves its history over time as a dynamically changing relationship system in the context of the community and the culture’. With the help of the basic concepts of ‘frame’ (semi-stable features of relationships) and ‘co-regulation’ (organization and change of the relationship, emergent from the co-actions of the participants) Fogel redefines the episodes of the Strange Situation procedure as a series of experimentally manipulated relationship frames. However, future analyses and subsequent investigations should make clear how these exciting reformulations could be connected with empirical data. How can one investigate empirically f.i. Fogel’s suggestion that the insecure resistant pattern rests on ‘lack of experience to cope with transitions between frames’, while the insecure avoidant pattern rests on ‘lack of a model for how to elaborate frames’? Perhaps we should connect Van Geert’s proposal for modeling attachment as a non linear dynamic system with Fogel’s. Linking Fogel’s conceptualizations with Van Geert’s models could be a step forward to the ultimate, but still, remote goal of understanding the corresponding empirical reality.

4.3 Concluding remarks

Experimental manipulation of sensitivity has not brought conclusive results with respect to the effects on attachment security. And even the result that experimental induction of maternal sensitivity successfully raises the level of sensitivity is doubtful. Positive effects found thus far are small and the meaning of these effects are not clear (real increases or experimenter effects?).

There are several promising possibilities to amend, refine and enrich the traditional approach of attachment theory. Theoretical links with The Child’s Theory of Mind (Harris), with biopsychology (Kalverboer) and with ecological theory (Thompson) should be explored. Methodologically, future research should seriously consider to include studies of bi- and multidirectionality of caregiver-child interactions (Van den Boom & Hoeksma). Finally we would recommend to explore carefully the possibilities and limitations of a dynamic systems approach.
5. Developmental change

In this section we will first demonstrate (section 5.1) that the traditional attachment approach is non-developmental. To explain what is meant by this expression, we refer to Wohlwill’s foundational book on the task and methodology of developmental psychology (Wohlwill, 1973). In section 5.2 we will provide examples and discuss the significance of developmental studies in caregiver-child interactions. Section 5.3 offers some conclusions.

5.1 The hourglass of traditional attachment research

It is not necessary to quote pieces of the papers for this colloquium to demonstrate that very often authors, who are interested in development and change, actually limit themselves to investigate and to discuss determinants and consequences of attachment, as measured in the Strange Situation. A nice illustration is offered by the title of a review paper of Van IJzendoorn (1988): ‘The development of attachment relationships: determinants and effects’. In this and many other review papers the essential reasoning follows the scheme, once depicted by Riksen-Walraven (1983), to summarize attachment research in schematic form (see Figure 1). The form of such a schematic representation indeed resembles that of an hourglass. There are many determinants at the top, the small bottleneck of the Strange Situation Procedure in the middle, and a series of later consequences at the bottom. This is what Connell and Goldsmith (1982) called ‘the hour-glass methodology’ of attachment research.

Of course, this research has led to many insights and taught us a lot about the relevance of early psychological aspects of mother-child interaction, as well as about associations of attachment classifications with later social behavior. On the other hand, many questions cannot be answered by such an approach. It was already made clear that for example the causality of the relationship of mother’s sensitivity with attachment security cannot be answered, if age is treated as a design variable (Van den Boom & Hoeksma) and if change over time is not included in the research design. We argue that the attachment tradition should free itself from this pinching hourglass and orient itself towards developmental questions.

In his Presidential Address to the APA in 1957 Cronbach saw psychology as divided into two separate disciplines: one differential, the other experimental (Cronbach, 1957). This is precisely the division we can make between two types of traditional attachment studies: the differential studies specifying A, B, C and/or D types and relating these types with psychologically significant variables; the experimental and quasi-experimental studies manipulating aspects of parental care (e.g., sensitivity) and investigating the effects on attachment security. It was Wohlwill (1973), who more than 20 years ago made clear that Cronbach’s dichotomy was mistaken: there are not two, but at least three disciplines of psychology, the third being the developmental one.
Fig. 1. The 'hourglass' of traditional attachment research (Figure after Riksen-Walraven, 1983).
Developmental questions cannot be reduced to differential or experimental questions, they ask for a special methodology, singularly fitted for answering developmental questions.

According to Wohlwill ‘the developmental psychologist considers changes in behavior with age as the basic datum of his discipline’ (Wohlwill, 1973, p.22). This should be reached by shifting age from the usual status of an independent variable to one which forms part of the definition of the dependent variable. In the next section we will demonstrate this Wohlwillian approach in the domain of attachment research, by describing examples and possibilities.

5.2 Examples of a Wohlwillian approach of attachment development

Van den Boom and Hoeksma explain that non-interactive mother-child interaction research is generally non-developmental, and that in unidirectional research age is usually treated as a design variable, suggesting antecedent-consequent relations, that are fixed by design. An example is offered by the usual design from the attachment research tradition, in which early maternal sensitivity is linked to later attachment security. Developmental studies according to Wohlwill’s definition are extremely rare. Furthermore, Van den Boom and Hoeksma’s review also demonstrates that in bidirectional as well as in systemic studies, developmental orientations are even more exceptional. If this way of approaching phenomena is so little in demand among researchers of caregiver-child interactions then the question arises whether such an approach makes sense. Our answer to that question is affirmative, and we will try to demonstrate why.

First of all we should be willing to develop operationalizations to longitudinally measure the relevant variables. Hoeksma, Van den Boom, Koomen and Koops point to an interesting paradox in classic attachment research: because of the supposed relative stability of the mother’s sensitivity a few occasions to measure it would suffice, whereas a large number of occasions is needed to measure the, theoretically expected, changing attachment behavior of the child. Paradoxically, however, the available research offers the opposite picture. This, of course, is a consequence of the one-sided interest in sensitivity as the early cause of later attachment security. Hoeksma et al. propose a longitudinal parallel measurement of both variables, to investigate the interconnectedness of the developmental changes in both variables. It is interesting to see that the authors’ piece of research actually suggests that developmental changes in both variables are connected (r=.40). This is, as far as we know, the first time that it is demonstrated that sensitivity and attachment are developmentally connected, and not just seemingly, because of an arrangement by design. To accept this result one should accept of course the use of the authors’ instrument to measure the child’s attachment behavior. And it can be expected that this causes some difficulties, because the Strange Situation procedure can hardly be replaced by any alternative, according to present attachment theorists. However, the use of alternative operationalizations is not entirely a question of conceptualization at the theoretical level, but scientific openness requires empirical
psychometrical support for the approval and disapproval of operationalizations.

A nice example of an alternative procedure for the Strange Situation has been offered by Koomen and Hoeksma: ‘the induced stress at home’ (IHS) procedure. With the help of this procedure it was possible to infer security of attachment at home, and because of its independence of stranger anxiety it could be used earlier than the Strange Situation. With the help of this type of innovative, carefully validated procedure, it is possible to investigate developmental questions. The data offer some support for the more fundamental developmental view put forward by Waters et al. (1990), to make a distinction between the onset of secure base behavior by the age of 1-year and consolidation of secure base behavior in the second or third year.

To truly investigate the developmental processes involved in attachment behaviors, while still profiting from the huge amount of available data, alternative operationalizations should be validated against already existing ones, as well as against the crystallized theoretical attachment framework. These alternatives are needed to broaden the classic approach in such a way as to be able to include research questions about developmental change. A promising way of trying out possibilities to investigate the developmental dynamics of attachment consists of course in simulation research. Van Geert offers interesting examples by applying a dynamic growth model. The dynamic systems approach can be considered as an extension of the Wohlwillian paradigm: it interconnects developmental functions with series of other developmental functions, in one complex dynamic system, that includes - to make it even more complicated - different time levels: age, chronological time, evolutionary time, etc. However, the playful simulations of Van Geert mainly serve a heuristic function. We should realize that in more than 20 years Wohlwill’s plea for the construction of simple developmental functions was hardly successful, in particular within the domain of mother-child interactions. We think therefore that it is unrealistic to expect that the community of researchers in our particular field, will -as it were by a sudden jump- embrace a model that is in terms of complexity exponentially enlarged. We are afraid that such a sudden jump would even be unfruitful. We should start by thinking in terms of the simple Wohlwillian approach and we should construct developmental functions before we go into modeling entire dynamic systems. First things first.

5.3 Concluding remarks

We can conclude that it appears impossible to find answers to developmental questions, if we seek them with the help of differential or experimental psychological designs. Therefore, we should answer these questions by creating developmental models, to be tested on the basis of longitudinal data. For that purpose we have to invest time and energy in developing and validating measurement devices, suited for the task at hand. To prepare for the modeling task, it is useful to run simulations, and to connect the models as closely as possible with the theoretically relevant and plausible variables.
7. Outline of the book

The next 14 chapters are based on the presentations during the colloquium. They are ordered according to the line of reasoning in this introductory chapter.

The first three chapters discuss theory and research from the attachment tradition. Chapter 2 contains Van Dijken and Van der Veer’s historical research on the early work of John Bowlby; chapter 3 offers an up-to-date review of research that pertains to the role of maternal, infant and social-contextual influences on the development of secure vs. insecure infant-mother attachments by Belsky; the importance of traditional mother-child interaction research for child psychiatry is discussed by De Château in chapter 4.

After these three general chapters on attachment theory and research, 4 chapters report on the results of the Dutch ELO-project and related research. Riksen-Walraven and Van Aken report on the long-term effects of their early intervention programs upon children’s cognitive and personality development at 7, 10 and 12 years (chapter 5); Juffer et al. discuss their findings on the effects of their interventions on attachment security in adoptive families (chapter 6); Wijnroks and Kalverboer summarize their research on early mother-child interaction and quality of attachment in preterm infants (chapter 7); Koomen and Hoeksma discuss their data on differential age-effects of brief early hospitalization (chapter 8).

The next 5 chapters offer alternative views on early mother-child interaction and attachment. Chapter 9, by Van den Boom and Hoeksma, reviews the different conceptualizations and methodologies in mother-child interaction research.

The authors make clear that a truly developmental view is missing, and that interesting innovations are possible with the help of available statistical tools for studying bi- and multidirectionality in mother-child interactions. Hoekema et al. concentrate on an analysis of their data on the mother’s sensitivity as longitudinally related to the child’s attachment behavior. A model is proposed, that takes constancy and change of sensitive responsiveness and attachment behavior during the first year of life into account (chapter 10). Another alternative is discussed by Kalverboer, who suggests to reduce the global concept of ‘sensitivity’ to basic mechanisms involving postural control and visual attention (chapter 11). Harris suggests to study the young child’s belief-desire representations as the roots of the child’s working model as it has been conceptualized in attachment theory (chapter 12). Thompson offers a bio-ecological view to reinterpret the available data and to suggest research possibilities that include the ecological context (chapter 13). Fogel tries to get rid of the limitations of the traditional approach by conceptualizing mother-child interaction as a dynamic system (chapter 14), while Van Geert offers examples of modeling mother-child interactions by using mathematics of nonlinear dynamics (chapter 15).

The book concludes with twelve short reports on ongoing research in the field. These short reports are based on the posters discussed during the colloquium. The first five reports are closely linked to core questions of traditional attachment theory: Clarke-Stewart et al. offer some data on the California Attachment Profile, an alternative operationalization for the Strange Situation (1); Hey-
mans offers some psychometrics on the validity of the A,B,C classification (II); Verwey-Tijsterman et al. compare the relation between attachment classification and mother’s interactive behavior in day-care and homereared infants (III); Zimmermann and Grossmann report on a longitudinal study on early attachment and adaptation in adolescence (IV); Zimmermann et al. report on developmental continuities and discontinuities in attachment representations from infancy to adolescence (V).

The next three reports offer examples of research aimed at analyzing developmental mechanisms at the base of mother-child interaction: Van Beek and Verschoor report on early responsivity and language development in preterm infants (VI), Genta and Brighi analyze loss of control and negative behaviors in two month old infants (VII), Ruel offers new data on maternal regulation of visual attention in 2- to 4-month-old infants (VIII).

The last four reports address contextual analyses of early mother-child interaction: Chasiotis et al. offer empirical support for an evolutionary theory of socialization in cross-cultural comparison (IX); de Jong et al. analyze mother-child interaction at home, related to school achievement (X); Olthof explores possibilities of neural network simulations of dyadic interaction (XI); Zevalkink et al. investigate the context of mother-child interaction in two urban communities in Bandung, Indonesia (XII).

References


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