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Less Is More: Meta-Analyses of Sensitivity and Attachment Interventions in Early Childhood

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Is early preventive intervention effective in enhancing parental sensitivity and infant attachment security, and if so, what type of intervention is most successful? Seventy studies were traced, producing 88 intervention effects on sensitivity (n = 7,636) and/or attachment (n = 1,503). Randomized interventions appeared rather effective in changing insensitive parenting (d = 0.33) and infant attachment insecurity (d = 0.20). The most effective interventions used a moderate number of sessions and a clear-cut behavioral focus in families with, as well as without, multiple problems. Interventions that were more effective in enhancing parental sensitivity were also more effective in enhancing attachment security, which supports the notion of a causal role of sensitivity in shaping attachment.

The current meta-analytic study focuses on the analysis and synthesis of sensitivity and attachment interventions. Experimental intervention studies that aim at changing parental behavior or children's development are important at least in two ways. First, nonexperimental research designs seem to dominate the field of parenting and child development, and much of the knowledge about parenting and development is derived from descriptive cross-sectional or longitudinal studies. However, experiments may be crucial in determining whether parenting is indeed causally related to child development or whether genetics or nonparental influences are the most powerful force in shaping children's development (Harris, 1998; Rowe, 1994). In this respect, the field of attachment research may serve as an example. Descriptive attachment studies outnumber by far experimental investigations, although their central hypotheses are formulated in causal terms, such as the alleged causal association between parental sensitive behavior and the development of children's attachment relationships. Without critical experimental tests of such hypotheses, progress in this research area will continue to be hampered by unsolved and basically unsolvable disputes about core assumptions.

Second, experimental interventions may also demonstrate optimal ways of changing human attitudes, mental representations, or behavior. One is inclined to think that more is better—that is, that more frequent and more intensive therapeutic or preventive interventions with a longer duration and broader focus may be more effective than shorter interventions with a narrower focus. For example, considerable disagreement still exists between experts in the domain of child psychotherapy about whether brief and cognitive–behavioral approaches would be more effective than long-term and intensive psychosocial treatments (Luborsky et al., 2002; Weisz, Weiss, Han, Granger, & Morton, 1995). After 30 years, the "dodo verdict" (i.e., "everybody has won and all must have prizes") on the equal effectiveness of all types of psychotherapeutic treatment is still as appealing as it is controversial (e.g., Beutler, 2002; Luborsky et al., 2002). In the area of parenting interventions, similar controversies exist. Parent training and family interventions may be shaped anywhere on a continuum between the provision of a 15-min videotape on reciprocal mealtime communication (Black & Teti, 1997) to intensive and weekly individual meetings with the troubled family during the first 3 years after a child's birth (Wasik, Ramey, Bryant, & Sparling, 1990). Is the dodo bird perspective true: Are all types of intervention equal irrespective of their positions on this continuum and irrespective of the specific type of parent or family at which they are aimed? Again, intervention on sensitivity and attachment may be a good case study for testing the idea that all interventions have some effect on parenting and child development and that no specific type of intervention stands out as most effective in some circumstances.

In recent years, the number of preventive interventions has been increasing in an exponential way. Policymakers have emphasized the need for systematic development and evaluation of preventive interventions that aim at enhancing the life expectations of the youngest generations and at stimulating their developmental and educational potentials. The idea that early interventions may be most effective in preventing less optimal or even deviant developmental pathways has led to an increase in efforts to shape and evaluate preventive interventions in the first few years of life, and even in the prenatal stage (e.g., Barnard et al., 1988, Huxley & Warner, 1993; Jacobson & Frye, 1991; Leitch, 1999; Luster, Perlstadt, McKinney, Sims, & Juang, 1996). In particular, the concept of a critical period of fast neuropsychological growth—as well as the possibly irreversible effects of impaired neurological development during the first 3 years after birth—has been important in promoting early interventions. Although the scientific evidence for a critical period of neurological maturation has been rather shallow (Fox, Leavitt, & Warhol, 1999; Shonkoff & Phillips, 2001), it has attracted important human and financial resources to the field of early preventive interventions.

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Since the publication of one of the first comprehensive reviews on the effectiveness of early childhood interventions on cognitive development (Bronfenbrenner, 1974), several reviews and meta-analyses on early interventions have documented their effectiveness in enhancing parental child-rearing attitudes and practices and children's socioemotional development (Beckwith, 2000; Benasich, Brooks-Gunn, & Clewell, 1992; Bradley, 1993; Egeland, Weinfield, Bosquet, & Cheng, 2000; Heinicke, Beckwith, & Thompson, 1988; Lagerberg, 2000; Lojkasek, Cohen, & Muir, 1994; MacLeod & Nelson, 2000; Van IJzendoorn, Juffer, & Duyvesteyn, 1995). In the current meta-analysis, we aim at having the evidence take us one step further. If early preventive intervention is effective in enhancing parental sensitivity and infant attachment security, what type of intervention is most successful? The time has come for a quantitative synthesis of the data that have been collected in a myriad of uncoordinated efforts and to try and come to evidence-based conclusions about what are the best intervention practices.

For example, in individual intervention studies, the best timing of interventions is difficult to evaluate because it requires the comparison of several different age cohorts. Egeland et al. (2000) mentioned the absence of research on the timing of interventions, that is, the optimal time to begin and to end the intervention efforts. Meta-analysis of extant interventions starting at different time points may address this issue and go beyond conclusions drawn from the separate studies. Also, intervention efforts may vary in focus, duration, and method. In their narrative review of 15 attachment-based interventions, Egeland et al. (2000) recently distinguished four types of interventions with different program approaches and goals: (a) programs that seek to enhance parental sensitivity at the behavioral level, (b) programs designed to alter parents' mental representations, (c) programs that provide and enhance social support (beyond the establishment of a supportive relationship between intervenor and parent), and (d) programs designed to enhance maternal mental health and well-being. In fact, the interventions presented in this latter category consisted of three different mixtures of support and other approaches. The authors concluded that long-term and frequent interventions, providing support, behavioral feedback, and discussions about past and present attachment experiences and their representations should be considered most effective, in particular in multirisk families. Moreover, they recommended that interventions should begin before an infant is born.

In contrast, Van IJzendoorn et al. (1995) were more skeptical about long-term and more broadly focused programs in their exploratory meta-analysis of 11 intervention studies on maternal sensitivity and of 12 intervention studies on infant–mother attachment. Interventions seemed rather effective with respect to sensitivity ($d = 0.58$), but a smaller effect on attachment was found ($d = 0.17$). A surprising difference was found between long-term and short-term interventions. Short-term interventions with a confined focus were relatively successful in affecting attachment ($d = 0.48$), whereas long-term interventions seemed to be not effective at all ($d = 0.00$). However, because the number of studies was small, the authors' conclusions regarding this difference were only tentative and to be considered as hypotheses for testing in a large-scale meta-analytic effort. For example, Van IJzendoorn et al. (1995) then hypothesized that short-term interventions with a narrow, behavioral focus may be effective in changing parental sensitive behavior but that they would fail to affect the more robust attachment insecurity in the infant. Furthermore, the differentiation between behavioral and representational interventions is not equivalent to the difference between short-term and long-term interventions. Behavioral interventions may consist of a large number of sessions (e.g., Spiker, Ferguson, & Brooks-Gunn, 1993; Wasik et al., 1990; Zaslow & Eldred, 1998), whereas representational interventions may be rather brief (e.g., Bakermans-Kranenburg, Juffer, & Van IJzendoorn, 1998; Robert-Tissot et al., 1996; Ziegenhain, Wijnroks, Derksen, & Dreisörner, 1999). Lastly, because of the small number of studies in Van IJzendoorn et al. (1995), it was impossible to adequately test the effectiveness of type of intervention in samples with more or less risk factors.

Besides their relevance for prevention, intervention studies are also crucially important for confirming or falsifying causal hypotheses. In the domain of socioemotional development, descriptive cross-sectional and longitudinal investigations are dominant, and experiments are rather scarce. Nevertheless, core developmental issues are framed in causal terms and need to be tested in the most rigorous ways, that is, through experimental manipulation and effect evaluation. For example, a core issue in attachment theory is the purported causal link between parental sensitivity and infant attachment security. Correlational evidence for such a link has been piling up in the past few decades (for a meta-analytic synthesis, see De Wolff & Van IJzendoorn, 1997), but an equally impressive amount of experimental data is still lacking. It is a replicated fact that parental sensitivity is significantly but modestly associated with infant attachment ($r = .24$, $k = 21$, $N = 1{,}099$; in nonclinical samples using the Strange Situation Procedure and observational sensitivity measures; De Wolff & Van IJzendoorn, 1997). This association, however, leaves the possibility open that infant attachment is in fact the causal determinant of parental sensitivity. Furthermore, other factors related to both sensitivity and attachment might be responsible for the (possibly spurious) correlation. In the current study, we investigated for the first time whether changes in parental sensitivity that are brought about by preventive interventions are indeed accompanied by corresponding changes in infant attachment security.

We were able to trace 70 published studies presenting 88 intervention effects on sensitivity ($k = 81$, $n = 7{,}636$) and/or attachment ($k = 29$, $n = 1{,}503$). This was a sufficiently large database to differentiate among the different types of interventions that Egeland et al. (2000) discussed in their narrative review and to go beyond the simplified dichotomy between the short-term behaviorally oriented interventions and the long-term representational interventions that was made in the exploratory meta-analysis by Van IJzendoorn et al. (1995). Furthermore, because of the large number of studies and samples, we were also able to test the effectiveness of interventions in groups with varying risk factors, and to see whether interventions starting earlier in life indeed would be more effective. In the current article, we address the following hypotheses in respect to four main assumptions in the domain of attachment-based interventions:

1. Early intervention on parental sensitivity and on infant attachment security is effective.

In attachment-based interventions, parental sensitivity is a more proximal factor than infant attachment security, which is more

distal to the immediate goals of most programs. We expected larger effects on sensitivity than on attachment.

2. Type and timing of the intervention program make a difference.

We expected that earlier interventions, behaviorally focused interventions, and interventions with fewer sessions would be more effective and efficient than long-term interventions with a nonbehavioral focus.

3. Intervention programs are always and universally effective.

We did not expect this would be true. Rather, we hypothesized that interventions may be more or less effective dependent of the specific population and that samples from at-risk populations may need more intensive interventions than more normative samples.

4. Changes in parental sensitivity are causally related to attachment security.

In this quantitative synthesis of attachment-based intervention research, we tested whether manipulation of parental sensitivity is accompanied with similar changes in infant attachment. We expected that interventions that were more successful in enhancing parental sensitivity would also be more effective in enhancing infant attachment.

Method

Data Collection

Pertinent studies were collected systematically, using at least three different search strategies (Mullen, 1989; Rosenthal, 1991). First, PsycLIT, *Dissertation Abstracts International*, and MEDLINE were searched with the key words *attachment*, *sensitivity* (or related terms such as *responsiveness*), and *intervention* (or related terms such as *preventive* or *therapeutic*). Combinations of terms were: *attachment and intervention**, *attachment and prevent**; *attachment and therapeut**; *sensitiv**, *and parent** (or *mother** or *father**) combined with *intervention** or *prevent** or *therapeut**. (An asterisk indicates that the search contained but was not limited to that word or word fragment.) Second, the references of the collected articles, books, and book chapters were searched for relevant intervention studies. Third, experts in the field were asked to mention intervention studies related to sensitivity or attachment. Our selection criteria were rather broad to include as many intervention studies as possible, regardless of research design qualities. The idea was to test the influence of design features empirically and to not exclude any quantitative studies on a priori grounds (Rosenthal, 1995). We selected interventions that started before children's mean age of 54 months. Brief postnatal interventions with the Brazelton Neonatal Behavioral Assessment Scale were excluded as well (see Das Eiden & Reifman, 1996, for a meta-analysis of this type of intervention). Case studies were excluded, as were unpublished studies or interventions that were reported only at meetings or conferences. Comparing published and unpublished data sources empirically, Rosenthal (1991) concluded that published studies are not strongly biased in their results relative to unpublished studies. However, to estimate the size of the file drawer problem in the current set of meta-analyses, we provided the fail-safe numbers of unretrieved studies with null results that would be needed to cancel out the combined effects found in the retrieved studies (Mullen, 1989).

We included not only intervention studies using the classic Ainsworth sensitivity rating scales (Ainsworth, Bell, & Stayton, 1974) but also studies with posttests based on the Home Observation for Measurement of the Environment (HOME; Caldwell & Bradley, 1984), the Nursing Child Assessment Teaching Scale, (NCATS; Barnard et al., 1988), or the Erickson rating scales for maternal sensitivity and supportiveness (Egeland, Erickson, Clemenhagen-Moon, Hiester, & Korfmacher, 1990; Erickson, Sroufe, & Egeland, 1985). In case of the HOME, we selected the observation scale for maternal sensitivity (emotional and verbal responsivity) if data on separate scales were provided. This was the case in the vast majority of the studies with HOME posttests. We also included studies that assessed maternal interactive behavior with measures other than the above-mentioned instruments, provided that they used observational measures of parental behavior clearly related to sensitivity (e.g., empathic responsiveness, Lieberman, Weston, & Pawl, 1991; sensitive mealtime communication, Black & Teti, 1997). The intervention studies were not restricted to a specific population (i.e., some samples were middle-class families with healthy infants, but we included studies with clinical and at-risk populations as well). Because the current meta-analyses were limited to parental sensitivity and children's attachment security, we do not describe intervention studies that concentrated on the child's cognitive development only (for reviews of cognitively oriented programs, see Farran, 1990; Zigler & Hall, 2000).

We collected intervention studies that aimed at enhancing positive parental behaviors, such as responsiveness, sensitivity, or involvement, on the basis of our presumption that these behaviors are beneficial for a child's concurrent and later social and emotional development, in particular attachment security. Because we were interested in actual changes in parenting behavior rather than parent-reported evaluations or attitudes, we restricted our search to studies that used observational measures. For an overview of the included studies, see Table 1. A detailed description of the studies may be requested from Marinus H. van IJzendoorn (see also Juffer, Bakermans-Kranenburg, & Van IJzendoorn, in press, for a narrative review detailing the various intervention approaches and methods). Compared with the first small-scale and exploratory meta-analysis in this area (Van IJzendoorn et al., 1995), which involved only studies with a combined effort to enhance sensitivity and attachment security, the current meta-analyses included studies with interventions on sensitivity and/or attachment. Compared with the first meta-analysis, which encompassed 11 sensititvity and 12 attachment intervention studies, the current meta-analyses included a much larger number of studies: 70 published intervention studies with 88 interventions directed at either sensitivity or attachment or both. The 88 interventions did not include overlapping samples.

Coding System

We used a detailed coding system to rate every intervention study on design, sample, and intervention characteristics (see Table 2). As design characteristics we coded sample size, randomization, the absence or presence of a control group, and the study's attrition rate. To test the effectiveness of interventions for specific populations, we coded features of both the involved parents (e.g., high or middle vs. low socioeconomic status [SES], adolescence, clinical reference, or at high risk because of a combination of risk factors such as poverty, social isolation, and single parenthood) and their children (e.g., prematurity, irritability, international adoption). When reported, the percentage of insecurely attached children in the control group was included as indicator of the risk for attachment-related problems in the sample. Intervention characteristics were the number of sessions, the age of the child at the start of the intervention, and the status of the intervenor (professional or nonprofessional). Moreover, we coded whether the intervention took place at the parents' home and whether video feedback was used as an intervention tool. Lastly, and according to Egeland et al.'s (2000) taxonomy, we coded whether the intervention aimed at enhancing parental sensitivity, affecting the parents' mental representation, providing social support, or any of the possible combinations of these approaches.

Table 1
Intervention Studies: Sample Characteristics, Focus, and Effect Sizes

Study	Sample	Focus	Posttest *d*	
			Sensitivity	Attachment
Anisfeld et al. (1990)[a]	Low SES	Sn	0.53	0.62
Armstrong et al. (2000)[a]	Multirisk	Sn + Sp	0.61	
Bakermans-Kranenburg et al. (1998, Study 1)[a]	Low SES, insecure AAI	Sn	0.91	0.20 (3-way), −0.20 (4-way)
Bakermans-Kranenburg et al. (1998, Study 2)[a]	Low SES, insecure AAI	Sn + R	0.81	0.00 (3-way), −0.20 (4-way)
Barnard et al. (1988)[a]	Multiproblem, low social support	Sn + Sp	0.34	0.00
Barnett et al. (1987, Study 1)[a]	Highly anxious mothers	Sn + Sp		−0.26
Barnett et al. (1987, Study 2)[a]	Highly anxious mothers	Sp		−0.07
Barrera et al. (1986)[a]	Preterm infants	Sn + Sp	0.52	
Beckwith (1988)[a]	Low-SES, sick preterm infants	Sn + Sp	0.40	0.00
Benoit et al. (2001)	Feeding problems	Sn	1.07	
Black & Teti (1997)[a]	Adolescent mothers	Sn	0.54	
Brinker et al. (1994)	Minority, high risk	Sn	0.58	
Brophy (1997)[a]	Adolescent mothers	Sn + R + Sp	0.00	
Bustan & Sagi (1984)	Preterm infants	Sn	0.66	
Cicchetti et al. (1999)[a]	Depressed mothers	R		0.43
Cohen et al. (1999, Study 1)	Clinically referred infants	Sn + R	0.38 (*M*)	0.14
Cohen et al. (1999, Study 2)	Clinically referred infants	R	0.65 (*M*)	−0.86
Constantino et al. (2001)[a]	Low SES, stressed, ethnically heterogeneous	Sn	0.00	
Cooper & Murray (1997, Study 1)[a]	Clinically depressed mothers (*DSM–III–R*)	Sp	0.00	−0.08 (3-way), −0.10 (4-way)
Cooper & Murray (1997, Study 2)[a]	Clinically depressed mothers (*DSM–III–R*)	R	0.00	−0.01 (3-way), −0.14 (4-way)
Cooper & Murray (1997, Study 3)[a]	Clinically depressed mothers (*DSM–III–R*)	Sn + R	0.00	−0.10 (3-way), −0.18 (4-way)
Dickie & Gerber (1980)	Middle class	Sn + Sp	0.84	
Egeland & Erickson (1993)[a]; Egeland et al. (2000)	Low SES; multiproblem	Sn + R + Sp	0.29	−0.42
Field et al. (1998)	Polydrug-using adolescent mothers	Sn + Sp	0.37	
Field et al. (1980)[a]	Preterm infants; lower-class, Black, teenage mothers	Sn	0.95	
Fleming et al. (1992)	Depressed and nondepressed mothers	Sp + R	0.46	
Gelfland et al. (1996)	Clinically depressed mothers	Sn + Sp	0.07	−0.19
Gowen & Nebrig (1997)	Multiproblem	Sp		0.56
Hamilton (1972)	Poor minority mothers	Sn + Sp	0.90	
Heinicke et al. (1999)[a]	Multiproblem	Sn + R + Sp	0.74	0.53
Huxley & Warner (1993)	High-risk families	Sn + R + Sp	0.69	
Jacobson & Frye (1991)[a]	Low SES, low social support	Sp	0.00	0.96
Juffer et al. (1997)	Internationally adopted infants	Sn	0.11	0.22
Juffer et al. (1997)	Internationally adopted infants	Sn	0.39	0.52
Kang et al. (1995, Study 1)[a]	Preterm infants, middle/high SES	Sn	0.43	
Kang et al. (1995, Study 2)[a]	Preterm infants, low SES	Sn	0.40	
Kang et al. (1995, Study 3)[a]	Preterm infants, low SES	Sn + Sp	0.46	
Kitzman et al. (1997)[a]	Low-SES single mothers	Sn + Sp	0.10	
Koniak-Griffin et al. (1995)[a]	Middle class mothers	Sn	0.00	
Krupka (1995)	Adolescent mothers	Sn	0.68	0.71
Lafreniere & Capuano (1997)[a]	Anxious–withdrawn children	Sn + Sp	0.70	
Lambermon (1991); Lambermon & Van IJzendoorn (1989, Study 1)	Large or small social network	Sn	0.77	
Lambermon (1991); Lambermon & Van IJzendoorn (1989, Study 2)	Large or small social network	Sn	0.58	
Larson (1980, Study 1)	Low SES	Sn + Sp	0.44	
Larson (1980, Study 2)[a]	Low SES	Sn + Sp	0.00	
Leitch (1999)[a]	First-time middle-class mothers	Sn	0.82	
Letourneau (2000)[a]	Adolescent mothers	Sn + Sp	1.13	
Lieberman et al. (1991)[a]	Low-SES insecure children	Sn + R + Sp	0.70	−0.13

Table 1 (*continued*)

Study	Sample	Focus	Posttest *d*	
			Sensitivity	Attachment
Luster et al. (1996)[a]	Low-SES teenage mothers	Sn + Sp	0.46	
Lyons-Ruth et al. (1990)	Low SES, multiproblem	Sn + Sp	0.00	0.70 (3-way), 0.58 (4-way)
Madden et al. (1984)[a]	Low SES	Sn	0.00	
Mahoney & Powell (1988)	Mentally retarded children		0.42	
Meij (1992, Study 1)[a]	Low SES	Sn	−0.21	0.00
Meij (1992, Study 2)[a]	Low SES	Sn	0.47	0.28
Metzl (1980, Study 1)[a]	Middle/high-SES mothers	Sn	0.63	
Metzl (1980, Study 2)[a]	Middle/high-SES couples	Sn	0.63	
Meyer et al. (1994)[a]	Preterm infants	Sn + R + Sp	0.68	
Olds et al. (1986)[a]	Low SES, adolescent, single	Sn + Sp	0.55	
Onozawa et al. (2001)[a]	Postnatal depression	Sn	1.13	
Palti et al. (1984)	Jewish mothers	Sn	0.67	
Parks (1983/1984)	Adolescent mothers	Sn + R + Sp	0.19	
Riksen-Walraven (1978)[a]	Low SES	Sn	1.36	
Riksen-Walraven et al. (1996)	Low SES, cultural minority	Sn	0.38	
Robert-Tissot et al. (1996, Study 1)	Clinically referred infants: sleep/feeding/behavioral disorders	R	0.46	
Robert-Tissot et al. (1996, Study 2)	Clinically referred infants: sleep/feeding/behavioral disorders	Sn	1.82	
Rosenboom, 1994[a]	Internationally adopted infants	Sn	0.18	−0.61
Ross (1984)	Preterm infants, Low SES	Sn	1.05	
Sajaniemi et al. (2001)[a]	Preterm infants	Sn		0.86
Scholz & Samuels (1992)[a]	Middle/high-SES first-time couples	Sn	1.60	
Schuler, Nair, Black, & Kettinger (2000)[a]	Low SES, minority	Sn + Sp	0.75	
Seifer et al. (1991)	Infants with developmental disabilities	Sn	0.56	
Spiker et al. (1993)[a]	Preterm infants	Sn + Sp	0.16	
St. Pierre & Layzer (1999)[a]	Low-SES single mothers	Sn + Sp	0.00	
Tessier et al. (1998)[a]	Preterm infants	Sn	0.08	
Van den Boom (1988, 1994)[a]	Low-SES irritable infants	Sn	2.62	0.73 (3-way), 0.89 (4-way)
Wagner & Clayton (1999, Study 1)[a]	Low SES, minority	Sn + Sp	0.04	
Wagner & Clayton (1999, Study 2)[a]	Low-SES adolescents	Sn + Sp	−0.04	
Wagner & Clayton (1999, Study 3)[a]	Low-SES adolescents	Sp	−0.02	
Wagner & Clayton (1999, Study 4)[a]	Low-SES adolescents	Sn + Sp	−0.07	
Wasik et al. (1990)[a]	Low SES	Sn + Sp	−0.18	
Weiner et al. (1994)	Low SES, interaction problems	Sn	2.12	
Whitt & Casey (1982)[a]	Low SES	Sn	0.68	
Wijnroks (1994)	Preterm infants	Sn	0.44	0.00
Zahr (2000, Study 1)[a]	Low-SES, minority preterm infants	Sn + Sp	−0.04	
Zahr (2000, Study 2)[a]	Low-SES, minority preterm infants	Sn + Sp	−1.12	
Zaslow & Eldred (1998)[a]	Poor adolescent mothers	Sn + Sp	0.19	
Ziegenhain et al. (1999, Study 1)	Adolescent, multiproblem mothers	Sn	0.55	
Ziegenhain et al. (1999, Study 2)	Adolescent, multiproblem mothers	Sn + R	0.33	

Note. Some analyses of effects on attachment security in the meta-analyses were based on the three-way and the four-way attachment classifications of the Strange Situation Procedure and are so noted in the Attachment column. SES = socioeconomic status; Sn = sensitivity; Sp = support; AAI = Adult Attachment Interview; R = representation; *DSM–III–R* = *Diagnostic and Statistical Manual of Mental Disorders* (3rd ed., rev.; American Psychiatric Association, 1987).

[a] Study was included in core set of random studies.

As an example of an intervention aimed at enhancing parental sensitivity, Black and Teti (1997) provided adolescent mothers with a videotape to help them enhance their sensitivity during mealtime. An intervention aimed at affecting parents' mental representation was presented in Cicchetti, Toth, and Rogosch's (1999) study with depressed mothers. The intervention aimed at reconstructing mothers' representation of self in relation to their own parents to enable them to reconstruct new representations of themselves in relation to their child. An intervention focused on support can be found in one of Barnett, Blignault, Holmes, Payne, and Parker's (1987) intervention programs, in which experienced mothers provided

Table 2
Coding System for the Characteristics of the Individual Intervention Studies

Variable	Coding description
Design	
Sensitivity *N*	Sample size for which results on sensitivity and responsiveness were reported If the control group was compared with two or more intervention groups, the control group was split up accordingly (e.g., Bakermans-Kranenburg et al., 1998; Cooper & Murray, 1997).
Attachment *N*	Sample size for which results on attachment were reported If the control group was compared with two or more intervention groups, the control group was split up accordingly (e.g., Bakermans-Kranenburg et al., 1998; Cooper & Murray, 1997).
Random	0 = participants randomly assigned to intervention or control group 1 = participants not randomly assigned to intervention or control group; no randomization reported
Pretest	0 = no pretest assessment of attachment or sensitivity 1 = pretest assessment of attachment or sensitivity
Control	0 = no control group 1 = control group
Attrition	Percentage of attrition = 100 × [(original sample − sample size for which results were reported)/original sample]
Sample	
SES	0 = high or middle 1 = low When no information was available, adolescent mothers were considered low SES.
Clinical	0 = participants not clinically referred 1 = parents clinically referred or fulfilling *DSM–III–R* criteria (e.g., for major depressive disorder; Cooper & Murray, 1997) or clinically referred children (e.g., sleeping, feeding and/or behavioral problems; Cohen et al., 1999)
Age parent	0 = adults 1 = adolescents (majority of sample described as *adolescent* rather than *young*)
Preterm	0 = infants not born prematurely 1 = infants born prematurely (< 37 weeks)
Multirisk	0 = sample not high risk 1 = high-risk parents or infants (e.g., drug dependent, impoverished, socially isolated, minority group, single parents, adopted infants, irritable infants)
Insecurity in control group	Percentage of insecurely attached infants in control group (forced three-way classification)
Intervention	
Sessions	No. of sessions (mean) (Instruction session to introduce videotape or soft baby carrier: sessions = 1; interventions during 1 year, weekly: sessions = 40)
Duration	Duration of the intervention (in months): If this was not explicitly mentioned, we calculated duration as the last intervention month (or posttest month) minus age start (month start intervention). If the intervention consisted of a videotape, book, or baby carrier, the intervening period between provision of the material and the posttest was regarded as duration of the intervention.
Age start	Age of child (in months) at the start of intervention When intervention started at birth, age start = 0; when intervention started during pregnancy, a negative value was assigned. For example, during the third trimester of pregnancy (Heinicke et al., 1999; Jacobson & Frye, 1991), age start = −2; during the second trimester of pregnancy (Egeland & Erickson, 1993), age start = −4.
Intervenor	0 = not in person (e.g., videotape, written information) 1 = layperson (e.g., experienced mother) 2 = professional or graduate student
Home	0 = intervention not at home (e.g., health clinic, group meetings) 1 = intervention at participant's home
Video feedback	0 = no video feedback (videotape with instruction or demonstration may be used; e.g., Lambermon & Van IJzendoorn, 1989, Study 2; Scholz & Samuels, 1992) 1 = video feedback used as intervention method (e.g., Juffer et al., 1997, Study 2)
Focus	Focus of the intervention: 1 = support (e.g., facilitating access to appropriate community services providing clothes, food supply; e.g., Gowen & Nebrig, 1997) 2 = sensitivity (e.g., information on infant development, modeling of touch and massage, video feedback aiming at promoting sensitive responsiveness; e.g., Riksen-Walraven, 1978; Scholz & Samuels, 1992; Van den Boom, 1988)

Table 2 (*continued*)

Variable	Coding description
Intervention (*continued*)	
Focus (*continued*)	3 = representation (e.g., examination of internal working model of parent in relation to infant, reexperiencing of the past; e.g., Cicchetti et al., 1999) 4 = support + sensitivity (e.g., Armstrong et al., 1999; Beckwith, 1988) 5 = support + representation (e.g., Fleming et al., 1992) 6 = sensitivity + representation (e.g., Bakermans-Kranenburg et al., 1998, Study 2) 7 = sensitivity + representation + support (e.g., Egeland & Erickson, 1993; Heinicke et al., 1999)
Outcome	
Sensitivity measure	0 = parental sensitivity not assessed 1 = Ainsworth's rating scale for sensitivity (Ainsworth et al., 1974) or Erickson's rating scales for sensitive and supportive behavior (Egeland et al., 1990; Erickson et al., 1985) 2 = HOME (Caldwell & Bradley, 1984) 3 = NCATS (Barnard et al., 1988) 4 = other
Attachment measure	0 = infant–parent attachment not assessed 1 = Strange Situation Procedure, three-way classification 2 = Strange Situation Procedure, four-way classification (including disorganization) 3 = Attachment Q sort, sorted by trained observer 4 = Attachment Q sort, sorted by mother 5 = other

Note. SES = socioeconomic status; *DSM–III–R = Diagnostic and Statistical Manual of Mental Disorders* (3rd ed., rev.; American Psychiatric Association, 1987); HOME = Home Observation for Measurement of the Environment; NCATS = Nursing Child Assessment Teaching Scale.

support and practical help to highly anxious mothers. Barnett et al.'s second intervention program combined the provision of social support with efforts to enhance maternal sensitivity and was thus coded in the category "sensitivity and support." Intervention studies that combined strategies directed at maternal sensitivity, representation, and support were conducted by, for example, Egeland and Erickson (1993, with the STEEP project; i.e., steps toward effective, enjoyable parenting) and Heinicke et al. (1999). Satisfactory intercoder reliabilities were established ($k = 10$, mean $r = .98$; range = .90, 1.00; mean $\kappa = .95$; range = .78, 1.00). Marian J. Bakermans-Kranenburg and Marinus H. van IJzendoorn coded all studies independently, and disagreements were discussed to consensus.

Data Analysis

Because the studies included in this series of meta-analyses reported various statistics, the outcomes of all studies were recomputed with Mullen's (1989) advanced basic meta-analysis program and transformed into Cohen's *d*. In several cases we had to compute the correct effect sizes on the basis of means and standard deviations provided in the study report (e.g., Bakermans-Kranenburg et al., 1998). When more than one outcome was reported, they were meta-analytically combined into one effect size, Cohen's *d* (e.g., Armstrong, Fraser, Dadds, & Morris, 1999). No study or participant was counted more than once. The resulting set of effect sizes were inserted into Borenstein, Rothstein, and Cohen's (2000) Comprehensive Meta-Analysis (CMA) program that computed fixed as well as random effect model parameters. CMA also computed confidence intervals around the point estimate of an effect size. Because all studies proposed directed hypotheses predicting that the intervention would have a positive effect, we present the 90% confidence intervals (with one-tailed alphas set at .05).

Significance tests and moderator analyses in fixed effects models are based on the assumption that differences among studies leading to differences in effects are not random and that, in principle, the set of study effect sizes is homogeneous at the population level. Significance testing is based on the total number of participants (*N*), but generalization is restricted to other participants that might have been included in the same studies of the meta-analysis (Rosenthal, 1995). Statistical inferences may be regarded as applying only to the specific set of studies at hand (Hedges, 1994). In random effects models this assumption is not made (Hedges & Olkin, 1985); they allow for the possibility that each separate study has its own population parameter. In random effects models, significance testing is based only on the total number of studies, and generalization is to the population of studies from which the current set of studies was drawn (Rosenthal, 1995).

It has been argued that random effects models more adequately mirror the heterogeneity in behavioral studies and use noninflated alpha levels when the requirement of homogeneity has not been met (Hunter & Schmidt, 2000). We decided to present the combined effect sizes and their confidence intervals in the context of random effects models. The *Q* statistics are presented to test the homogeneity of the specific set of effect sizes and to test the significance of moderators (Borenstein et al., 2000; Mullen, 1989; Rosenthal, 1995). In our series of meta-analyses, several data sets were heterogeneous. In those cases, the random effects model parameters (significance, confidence intervals) are somewhat more conservative than the fixed effects parameters, and the moderator tests should be considered to be descriptive of the specific set of studies at hand (Rosenthal, 1995).

The current analyses included 70 studies describing 88 interventions on parental sensitivity or infant attachment. Some of the reports presented more than one intervention, and we computed separate effect sizes for each of the intervention studies (e.g., Cohen et al., 1999; Juffer, Hoksbergen, Riksen-Walraven, & Kohnstamm, 1997; Lambermon & Van IJzendoorn, 1989; Ziegenhain et al., 1999). In a few cases, the intervention was conducted without a control group but with a pre- and posttest design (e.g., Hamilton, 1972; Mahoney & Powell, 1988). When pertinent statistics were unavailable, we computed effect sizes on the basis of pretest and posttest means and standard deviations, using *t* tests for independent groups because *t* tests for dependent groups would require the raw data. These one-group pretest–posttest interventions were not included in our core set of randomized control group studies, but they were included in the set of nonrandomized studies (see below). In some cases, the control group in a

multiple-intervention study had to be divided to prevent participants from being counted more than once.

In the 70 studies, data on 9,957 children and their parents were reported. One of the studies was a multisite intervention study with an exceptionally large sample ($N = 2,799$; St. Pierre & Layzer, 1999). To avoid excessive influence of this outlying sample size, we based analyses for this study on the windsorized number (820; Hampel, Ronchetti, Rousseeuw, & Stahel, 1986). This resulted in a total number of 7,978 children and their parents in our data set, with an average sample size of 91, ranging from 12 (a subset of Ziegenhain et al.'s, 1999, sample) to 820 (St. Pierre & Layzer, 1999). For each of the 88 interventions, the standardized difference between the experimental and control group (or, in case a control group was absent, between pre- and posttest) was computed (Cohen's *d*; Mullen, 1989). For each study, we computed Fisher's *Z* as an equivalent to the correlation coefficient *r* (see Mullen, 1989). Two outlying effect sizes were identified in the set of sensitivity interventions on the basis of standardized *z* values larger than 3.3 or smaller than −3.3 ($p < .001$; Tabachnick & Fidell, 2001). We excluded the intervention studies showing outlying *z* values from further analyses. The effect size of one excluded study was comparable to a correlation of .79 (Van den Boom, 1994). Effect sizes of 0.75 or more may well exceed the combined reliabilities of the outcome measures and the consistency of intervention implementations. In the other outlying intervention (a subsample of Zahr, 2000) parental sensitivity decreased substantially, that is, with more than a standard deviation. The effect size of this excluded study was comparable to a correlation of −.49 (Zahr, 2000), which exceeded our preset limit of $z > -3.3$.

Results

Sensitivity

The current analyses included 81 studies (after removal of two outlying studies, see *Data Analysis* section) presenting intervention effects on parental sensitivity; 7,636 families were involved (after windsorizing, see *Data Analysis* section). Figure 1 is a stem and leaf display in which the effect sizes of all interventions on sensitivity (including the two outliers) are presented on the right side of the stem (Rosenthal, 1995). To estimate the combined effect size in the set of studies with the strongest designs, those intervention studies with a randomized control group design were selected. Because only a small minority of the interventions aimed at both mothers and fathers (three studies: Dickie & Gerber, 1980; one of the two interventions of Metzl, 1980; and Scholz & Samuels, 1992), we decided to select only interventions focusing on maternal sensitivity. A core set of 51 randomized control group studies was established, including 6,282 mothers with their children (see Table 3). In this core set of studies, interventions appeared to be significantly and moderately effective in enhancing maternal sensitivity ($d = 0.33$, $p < .001$). The fail-safe number for this core set of intervention studies was 913. It would take more than 900 unpublished studies without intervention effects to cancel out this combined effect size of the randomized studies (Mullen, 1989). Including the two outlying studies, we found a combined effect size across 53 studies of 0.38 ($n = 6,415$), which was within the confidence interval of the combined effect size for the core set. In the total set of 81 studies, this effect size was 0.44 ($p < .001$, $n = 7,636$). Random studies were significantly less effective than the other studies ($d = 0.61$, $p < .001$, $k = 27$, $n = 1,273$) in our data set ($Q = 28.21$, $p < .001$; note that the three random studies involving fathers were included in this contrast, resulting in $k = 54$ random studies in Table 4). Nonrandomized studies seemed to run the risk of inflated effects. Therefore, we first present the results for the core set of random studies, and then we briefly report on results for the total set of 81 studies.

Attachment	Stem	Sensitivity
	.9	
	.8	
	.7	3,9
	.6	2,7
	.5	6
3	.4	1,2,3,6,7,9,9
9,4,3,3,0	.3	0,0,1,1,2,2,2,2,2,2,3,3,5,5,6,7,8,9
7,5,5,1,1	.2	0,0,0,1,1,2,2,3,3,3,5,6,6,6,7,7,8,8,9
4,1,0	.1	0,0,3,4,6,7,8,9,9,9
7,0,0,0,0,0,0	.0	0,0,0,0,0,0,0,0,0,0,0,2,3,4,5,6,8,9,9
6,5,4,4	-.0	1,2,2,4
3,0	-.1	0
9,1	-.2	
9	-.3	
	-.4	9

Figure 1. Stem and leaf display of the effect sizes (Pearson's *r*) on attachment and sensitivity.

What interventions were most effective in enhancing maternal sensitivity? We classified the interventions into the following seven categories on the basis of their foci (see Egeland et al., 2000): (a) sensitivity; (b) support; (c) representation; (d) sensitivity and support; (e) representation and support; (f) sensitivity and representation; and (g) sensitivity, representation, and support. In the core set of randomized intervention studies, the category of (e) representation and support was not represented; only a few studies were included in the categories (b) support, (c) representation, and (f) sensitivity and representation. Therefore, the contrast based on these seven categories has not been tested. In the core set of randomized studies, the interventions focusing on sensitivity only ($d = 0.45$, $p < .001$); those combining sensitivity and support ($d = 0.27$, $p < .001$); and those using all levels of influence: representation, sensitivity, and support ($d = 0.46$, $p < .001$) showed rather substantial effect sizes. Contrasting the interventions focusing on sensitivity only ($k = 20$) with all other categories of intervention combined ($k = 31$; including interventions aiming at sensitivity and support, which is a broader focus than sensitivity only), we found that interventions focusing on sensitivity only were more effective ($d = 0.45$) than all other types of interventions combined ($d = 0.27$; $Q = 4.73$, $p = .03$).

Other characteristics of the interventions appeared relevant as well in the set of randomized studies. Interventions with video feedback were more effective ($d = 0.44$) than interventions without this method ($d = 0.31$; $Q = 4.08$, $p = .04$). Interventions with fewer than five sessions were as effective ($d = 0.42$) as interventions with 5 to 16 sessions ($d = 0.38$), but interventions with more than 16 sessions were less effective ($d = 0.21$) than interventions with a smaller number of sessions ($Q = 14.11$, $p < .001$). The age of the children at the start of the intervention appeared to be

Table 3
Meta-Analytic Results of Randomized Sensitivity Interventions (k = 51 Study Outcomes)

Characteristic	*k*	*n*	*d*	90% CI	*Q*	*p*
Total set	51	6,282	0.33***	0.25, 0.41	127.82***	
Attrition					15.05	< .001
0%	9	359	0.53**	0.20, 0.87	25.12**	
1%–20%	19	2,469	0.38***	0.26, 0.49	35.05**	
> 20%	23	3,454	0.23***	0.13, 0.33	48.40***	
Focus					4.73	.03
Sensitivity only	20	1,456	0.45***	0.28, 0.63	57.98***	
Other	31	4,826	27***	0.18, 0.35	63.45***	
Specified						
Sensitivity (Sn)	20	1,456	0.45***	0.28, 0.63	57.98***	
Support (Sp)	3	224	−0.01	−0.25, 0.23	0.00	
Representation (R)	1	57	0.00	−0.49, 0.49	0.00	
Sn + Sp	20	4,145	0.27***	0.18, 0.37	48.74***	
Sn + R	2	72	0.27	−0.37, 0.91	1.70	
Sn + R + Sp	5	328	0.46***	0.23, 0.68	5.71	
Intervenor					5.02	.08
Nonprofessional	5	545	0.33*	0.08, 0.58	11.89*	
Professional	42	5,041	0.29***	0.21, 0.36	74.89***	
No intervenor	4	696	0.62*	0.08, 1.17	35.03***	
At home					2.44	.12
No	11	1,298	0.48***	0.25, 0.70	44.56***	
Yes	40	4,984	0.29***	0.21, 0.37	79.97***	
Video					4.08	.04
No	43	5,907	0.31***	0.23, 0.40	119.46***	
Yes	8	375	0.44***	0.27, 0.62	4.03	
Sessions					14.11	< .001
< 5	14	1,146	0.42***	0.21, 0.63	42.64***	
5–16	18	1,274	0.38***	0.22, 0.53	37.85**	
> 16	19	3,862	0.21***	0.13, 0.29	30.52*	
Age start					6.40	.04
Prenatal	8	1,224	0.32***	0.17, 0.48	11.50	
< 6 months	28	4,077	0.28***	0.18, 0.38	66.71***	
> 6 months	15	981	0.44***	0.23, 0.64	41.40***	
Focus × Sessions[a]					15.80	< .001
Sn × < 16 Sessions	18	1,327	0.47***	0.29, 0.66	53.42***	
Other × < 16 Sessions	14	1,093	0.31**	0.14, 0.47	27.70**	
Sn × > 16 Sessions	2	129	0.30	−0.35, 0.96	2.72	
Other × > 16 Sessions	17	3,733	0.21***	0.13, 0.30	27.78*	
Sample						
SES					0.31	.58
Middle/high	16	1,842	0.25***	0.14, 0.36	19.22	
Low	35	4,440	0.35***	0.25, 0.46	108.15***	
Adolescent[b]					0.02	.88
Yes	12	1,127	0.30***	0.15, 0.46	21.39*	
No	38	4,335	0.36***	0.26, 0.45	93.59***	
Preterm					0.17	.68
Yes	9	1,682	0.35***	0.21, 0.49	16.01*	
No	42	4,600	0.32***	0.23, 0.42	111.74***	
Multirisk					0.12	.73
Yes	24	3,533	0.31***	0.21, 0.42	58.71***	
No	27	2,749	0.36***	0.24, 0.48	68.85***	
Clinical					9.57	.002
Yes	8	541	0.46***	0.23, 0.67	15.00*	
No	43	5,741	0.31***	0.22, 0.39	102.28***	
Outcome					15.22	.002
Ainsworth/Erickson	9	311	0.38**	0.19, 0.57	7.56	
HOME	15	2,447	0.21***	0.11, 0.30	21.16	
NCATS	8	1,707	0.25**	0.09, 0.41	16.74*	
Other	19	1,817	0.45***	0.28, 0.62	64.11***	

Note. Effect sizes (*d*s) were calculated with one-tailed alpha set at .05. *k* = fail-safe number of studies; *n* = total number of participants; CI = confidence interval; SES = socioeconomic status; HOME = Home Observation for Measurement of the Environment; NCATS = Nursing Child Assessment Teaching Scale.
[a] Contrast was tested without subgroup of $k < 4$ studies. [b] Contrast was tested without study with 35% adolescents (St. Pierre & Layzer, 1999).
* $p < .05$. ** $p < .01$. *** $p < .001$.

Table 4
Meta-Analytic Results of Sensitivity Interventions (k = 81 Study Outcomes)

Characteristic	*k*	*n*	*d*	90% CI	*Q*	*p*
Total set	81	7,636	0.44***	0.35, 0.52	281.59***	
Random/control					28.21	< .001
Yes[a]	54	6,363	0.36***	0.28, 0.44	145.42***	
No	27	1,273	0.61***	0.39, 0.82	94.75***	
Attrition					22.30	< .001
0%	23	845	0.63***	0.46, 0.79	34.61*	
1–20%	27	2,893	0.43***	0.31, 0.54	56.17***	
> 20%	31	3,898	0.34***	0.20, 0.49	165.43***	
Focus					26.76	< .001
Sensitivity only	35	2,124	0.64***	0.46, 0.82	167.57***	
Other	46	5,512	0.28***	0.21, 0.35	73.43***	
Specified						
Sensitivity (Sn)	35	2,124	0.64***	0.46, 0.82	167.57***	
Support (Sp)	3	224	−0.01	−0.25, 0.23	0.00	
Representation (R)	4	144	0.17	−0.22, 0.56	1.00	
Sn + Sp	27	4,426	0.28***	0.19, 0.37	53.74***	
R + Sp	1	127	0.46*	0.15, 0.77	0.00	
Sn + R	3	106	0.20	−0.20, 0.61	1.76	
Sn + R + Sp	8	485	0.40***	0.23, 0.57	8.71	
Intervenor					1.01	.60
Nonprofessional	7	660	0.32**	0.12, 0.51	12.90*	
Professional	70	6,280	0.44***	0.35, 0.54	232.93***	
No intervenor	4	696	0.62*	0.08, 1.17	35.03***	
At home					3.35	.07
No	27	1,963	0.52***	0.38, 0.66	57.66***	
Yes	54	5,673	0.40***	0.29, 0.50	220.81***	
Video					30.58	< .001
No	65	6,887	0.36***	0.28, 0.44	160.23***	
Yes	16	749	0.74***	0.44, 1.04	73.96***	
Sessions					16.65	< .001
< 5	20	1,309	0.51***	0.32, 0.70	56.41***	
5–16	34	1,979	0.44***	0.33, 0.56	56.02**	
> 16	27	4,348	0.38***	0.24, 0.53	152.35***	
Age start					18.78	< .001
Prenatal	10	1,308	0.35***	0.21, 0.50	13.93	
< 6 months	42	4,722	0.35***	0.26, 0.45	105.35***	
> 6 months	29	1,606	0.58***	0.37, 0.79	132.25***	
Fathers included					8.67	.003
Yes	3	81	10.05***	0.53, 1.58	3.79	
No	78	7,555	0.42***	0.33, 0.50	266.06***	
Sample						
SES					0.01	.94
Middle/high	33	2,540	0.42***	0.31, 0.53	53.27**	
Low	48	5,096	0.43***	0.31, 0.54	228.19***	
Adolescent[b]					1.48	.22
Yes	16	1,317	0.29***	0.17, 0.42	22.06	
No	64	5,499	0.48***	0.38, 0.58	235.15***	
Preterm					0.02	.89
Yes	13	1,857	0.43***	0.28, 0.58	29.81**	
No	68	5,779	0.44***	0.34, 0.54	251.53***	
Multirisk					0.01	.90
Yes	34	4,053	0.42***	0.28, 0.56	179.71***	
No	47	3,584	0.44***	0.33, 0.54	101.88***	
Clinical					7.01	.008
Yes	19	1,115	0.45***	0.31, 0.60	26.00	
No	62	6,521	0.43***	0.33, 0.53	249.04***	
Outcome					32.59	< .001
Ainsworth/Erickson	17	613	0.34***	0.20, 0.48	10.64	
HOME	21	2,756	0.31***	0.20, 0.42	43.08**	
NCATS	9	1,736	0.23**	0.08, 0.39	16.88*	
Other	34	2,531	0.60***	0.43, 0.78	166.98***	

Note. Effect sizes (*d*s) were calculated with one-tailed alpha set at .05. *k* = fail-safe number of studies; *n* = total number of participants; CI = confidence interval; SES = socioeconomic status; HOME = Home Observation for Measurement of the Environment; NCATS = Nursing Child Assessment Teaching Scale.
[a] The three random studies involving fathers were included in this contrast. [b] Contrast tested without study with 35% adolescents (St. Pierre & Layzer, 1999).
* $p < .05$. ** $p < .01$. *** $p < .001$.

significantly associated with effect size. Interventions starting later ($d = 0.44$) were more effective than interventions starting prenatally ($d = 0.32$) or in the first 6 months of life ($d = 0.28$; $Q = 6.40$, $p = .04$). Unexpectedly, the four studies that did not use personal contact as a means of intervening ($d = 0.62$, $p < .05$) tended to show the largest effect size. These studies relied on the provision of soft baby carriers (Snugglies, carriers that hold the infant close to the parent's chest; Anisfeld, Casper, Nozyce, & Cunningham, 1990) or introducing the kangaroo method (holding the infant close to the parent's chest; Tessier et al., 1998), a workbook on responsiveness (Riksen-Walraven, 1978), or a videotape (Black & Teti, 1997). It is somewhat paradoxical that the interventions with the smallest investments in terms of time and money tended to be more effective. However, the difference was not significant, the subset of studies was rather small, and the 90% confidence interval around the point estimate was rather large (0.08, 1.17); thus, no practical conclusions should be drawn. The effect of interventions conducted at parents' homes ($d = 0.29$) was not significantly different from the effect of interventions conducted elsewhere ($d = 0.48$; see Table 3).

There might be an association between the focus of the intervention and the number of intervention sessions (focused interventions might generally be briefer), obscuring whether high effect sizes should be ascribed to the focus of the intervention (sensitivity), to the number of sessions (fewer than 16), or to both. We formed a composite variable of four categories, combining focus (sensitivity only vs. the others) and number of sessions (fewer vs. more than 16; see Table 3). The contrast was significant ($Q = 15.80$, $p < .001$): Short interventions focusing on sensitivity only were most effective ($d = 0.47$).

We found it surprising that most characteristics of the samples, such as SES, prematurity, adolescent motherhood, and the presence of multiple risk factors, were not associated with significant differences in effect sizes between the studies. The only difference we found was between clinical and nonclinical samples: Interventions conducted with clinically referred samples were more effective ($d = 0.46$) than interventions with other groups ($d = 0.31$; $Q = 9.57$, $p = .002$).

Lower effect sizes were found for studies with the HOME ($d = 0.21$) or the NCATS ($d = 0.25$) as outcome measures than for studies with Ainsworth's or Erickson's rating scales ($d = 0.38$) and for studies with other outcome measures ($d = 0.45$; $Q = 15.22$, $p = .002$). Some of these latter studies used outcome measures that were rather closely related to the focus of the intervention (e.g., Black & Teti, 1997).

It should be noted that attrition was significantly related to effect size ($Q = 15.05$, $p < .001$). In studies without attrition, the combined effect size ($d = 0.53$) was significantly larger than in studies in which participants were lost ($d = 0.38$ for attrition between 1%–20% and $d = 0.23$ for attrition of more than 20% of the participants). Attrition may have had differential effects on the experimental and the control group, in favor of the latter, because the worst cases may disappear most readily from the control group. In that case, it would be more difficult for the experimental group to outperform the control group.

In the total set of 81 studies, we found similar results as in the core set of randomized studies (see Table 4). In this the total set the effect size of the interventions involving fathers could be compared with interventions focusing on mothers only—albeit only exploratively, as the subgroup of interventions involving fathers was small. The three studies involving fathers were significantly more effective ($d = 1.05$) than studies without fathers ($d = 0.42$). This difference was statistically significant ($Q = 8.67$, $p = .003$), but, again, it was based on only 81 people participating in the studies involving fathers, and the point estimate in this subset showed a broad confidence interval (0.53, 1.58).

Multivariate Approach

To test whether sample characteristics and/or intervention characteristics accounting for the differences in effect sizes were confounded, we conducted a multiple regression analysis with unbiased Hedges's *d*s as study outcomes in the total set of studies. At the first step, we introduced the design characteristic "randomization" to control for the influence of randomization on the size of the effects. Sample characteristics (low SES, multiproblem or clinically referred sample, adolescent mothers) were added at Step 2. Intervention characteristics—focus (sensitivity only vs. other), infant age at the start of the intervention (in months), and number of sessions—were added at Step 3. Last, we added the interactions between focus of the intervention and number of sessions. (We standardized both variables before multiplication so that sensitivity-focused interventions with the lowest number of sessions received the lowest scores.) The regression included eight predictors, and the ratio of predictors to studies was 1:10, which is considered adequate (Tabachnick & Fidell, 2001). The hierarchical multiple regression selected two significant predictors: focus of the intervention ($b = .26$, $p = .03$) and child's age at start of the intervention ($b = .23$, $p = .04$). Sample and design characteristics did not significantly predict effect size (Hedges's *d*). The number of sessions did not significantly contribute to the regression, but it tended to be associated in the expected direction ($b = -.16$, $p = .19$). The Focus of Intervention × Number of Sessions interaction did not change the regression equation. The regression was significant, $F(7, 73) = 2.91$, $p = .01$, and the predictors explained 22% of the variance in study effect sizes. Sensitivity-focused interventions and a later start of the intervention predicted higher effect sizes, even after controlling for characteristics of the sample.

Are shorter and behaviorally focused interventions also effective in groups with multiple risks, or do troubled families require more intensive interventions? We replicated the previous meta-analyses on program characteristics for the subset of randomized samples suffering from multiple problems (multirisk and/or clinically referred; $k = 30$, $n = 4{,}119$; see Table 5). In this set of multiproblem samples, the interventions focusing on sensitivity only were more effective ($d = 0.48$) than all other categories of intervention combined ($d = 0.25$; $Q = 5.83$, $p = .02$). We found similar effect sizes for the interventions focusing on sensitivity and support ($d = 0.26$) and on sensitivity, representation, and support ($d = 0.52$). Again, the most effective interventions consisted of fewer than 16 sessions. Interventions with fewer than 5 sessions ($d = 0.33$) were as effective as interventions with 5 to 16 sessions ($d = 0.36$), but they were both more effective than interventions with more than 16 sessions ($d = 0.20$; $Q = 9.92$, $p = .007$). To our surprise, nonprofessional intervenors ($d = 0.42$) did better than professional intervenors ($d = 0.26$) in terms of intervention effects. The contrast was significant ($Q = 9.02$, $p = .003$). One study (Black & Teti, 1997) that did not use personal contact but a

Table 5
Meta-Analytic Results of Sensitivity Interventions: Multiproblem Samples (Random Studies)

Characteristic	*k*	*n*	*d*	90% CI	*Q*	*p*
Multiproblem Sample total	30	4,119	0.29***	0.20, 0.39	69.94***	
Focus					5.83	.02
Sensitivity only	8	353	0.48***	0.26, 0.70	10.64	
Other	22	3,766	0.25***	0.15, 0.35	52.39***	
Specified						
Sensitivity (Sn)	8	353	0.48***	0.26, 0.70	10.64	
Support (Sp)	3	224	−0.01	−0.25, 0.23	0.00	
Representation (R)	1	57	0.00	−0.49, 0.49	0.00	
Sn + Sp	14	3,176	0.26***	0.14, 0.39	41.01***	
Sn + R	1	57	0.00	−0.49, 0.49	0.00	
Sn + R + Sp	3	252	0.52**	0.26, 0.78	2.90	
Intervenor[a]					9.02	.003
Nonprofessional	4	435	0.42**	0.17, 0.68	6.98	
Professional	25	3,625	0.26***	0.16, 0.36	52.30***	
No intervenor	1	59	0.54*	0.10, 0.98	0.00	
At home					0.46	.50
No	4	416	0.36*	0.06, 0.67	6.32	
Yes	26	3,703	0.29***	0.19, 0.39	63.11***	
Video					3.07	.08
No	25	3,812	0.27***	0.17, 0.38	64.01***	
Yes	5	307	0.40***	0.21, 0.59	2.74	
Sessions					9.92	.007
< 5	4	189	0.33*	0.08, 0.58	1.39	
5–16	11	923	0.36**	0.14, 0.57	30.95***	
> 16	15	3,007	0.22***	0.12, 0.32	26.25*	
Age start					3.38	.18
Prenatal	7	1,205	0.30***	0.15, 0.46	9.82	
< 6 months	14	2,250	0.25**	0.09, 0.41	48.27***	
> 6 months	9	664	0.34***	0.20, 0.48	8.47	

Note. Effect sizes (*d*s) were calculated with one-tailed alpha set at .05. *k* = fail-safe number of studies; *n* = total number of participants; CI = confidence interval.
[a] Contrast was tested without subgroup of $k < 4$ studies.
* $p < .05$. ** $p < .01$. *** $p < .001$.

videotape as a means of intervening was quite effective ($d = 0.54$) as well.

Attachment

We found 29 intervention studies aiming at attachment security. These attachment interventions involved 1,503 participants. In Figure 1, the effect sizes of all interventions on attachment security are presented on the left side of the stem in the stem and leaf display (Rosenthal, 1995). Most studies reported intervention effects on attachment security as observed in the standard Ainsworth Strange Situation Procedure (Ainsworth, Blehar, Waters, & Wall, 1978); one study used the Preschool Assessment of Attachment (Crittenden, 1992), and three studies used the Attachment Q sort (AQS; Vaughn & Waters, 1990) or a related outcome measure (AQS security items; Jacobson & Frye, 1991) as the only assessment of attachment. Nineteen studies observing attachment security in the Strange Situation Procedure reported findings with three-way (ABC) classifications; for 14 studies, the four-way (ABCD, including disorganized attachment) classifications were available. For all studies with the Strange Situation Procedure, we computed effect sizes by combining the insecure groups and comparing this combined insecure group (non-B) with the secure group (B). In cases in which both three-way and four-way classifications were available ($k = 10$), we computed effect sizes on the basis of the forced three-way classifications, as is customary (the secondary A, B, or C classifications of children classified as disorganized: A for D/A, B for D/B, and C for D/C) to optimize the comparability with other outcome measures and because the interventions did not explicitly aim at reducing infant attachment disorganization. The combined effect size for attachment security was small but significant ($d = 0.19$, $p < .05$). Twenty-three studies ($n = 1,255$) presented randomized control group experiments, which showed a similar effect size ($d = 0.20$, $p < .05$). In the subset of studies presenting the four-way classifications ($k = 14$, $n = 747$), the combined effect size for secure versus insecure (including disorganized) attachment was 0.19 ($p = .19$). The 90% confidence intervals ranged from −0.05 to 0.42 in a heterogeneous set of studies ($Q = 36.80$, $p < .001$). The fail-safe number for the randomized core set of attachment intervention studies was 191, meaning it would take more than 190 unpublished studies without intervention effects on attachment security to cancel out the combined effect size of 0.20 (Mullen, 1989).

What interventions were most effective in enhancing infant attachment security? Parallel to the procedure used regarding sensitivity, we used the core set of randomized intervention studies to address our questions regarding infant attachment security. Taking into account the small number of studies ($k = 23$), we excluded moderators with fewer than four studies in one of the

cells (i.e., adolescence, prematurity, status of the intervenor, and whether the intervention took place at the mother's home). The classification of the intervention studies into the seven categories on the basis of focus was preserved as a specification of the contrast between interventions focusing on sensitivity only and other interventions, with an eye to the theoretical significance of these categories (see Table 6); however, we only tested the contrast between interventions focusing on sensitivity ($k = 10$) and all other interventions combined ($k = 13$). Interventions aiming at enhancing sensitivity (without focusing on support or representa-

Table 6
Meta-Analytic Results of Randomized Attachment Interventions (k = 23 Study Outcomes)

Characteristic	*k*	*n*	*d*	90% CI	*Q*	*p*
Total set	23	1,255	0.20*	0.04, 0.35	55.21***	
Attrition					5.54	.06
0%	7	298	0.19	−0.14, 0.52	14.19*	
1–20%	11	635	0.09	−0.13, 0.30	22.40*	
> 20%	5	322	0.42*	−0.09, 0.75	12.50**	
Focus					10.99	< .001
Sensitivity only	10	463	0.39**	0.16, 0.62	18.63*	
Other	13	792	0.06	−0.12, 0.24	24.91*	
Specified						
Sensitivity (Sn)	10	463	0.39**	0.16, 0.62	18.63*	
Support (Sp)	3	144	0.28	−0.31, 0.87	7.89*	
Representation (R)	2	120	0.24	−0.12, 0.60	1.27	
Sn + Sp	3	205	−0.04	−0.28, 0.19	0.46	
Sn + R	2	72	−0.08	−0.51, 0.35	0.03	
Sn + R + Sp	3	251	−0.03	−0.52, 0.46	9.86**	
Video					5.35	.02
No	16	923	0.25**	0.09, 0.41	30.66***	
Yes	7	332	0.07	−0.29, 0.43	18.63**	
Sessions					3.06	.22
< 5	9	385	0.27*	0.01, 0.52	15.36	
5–16	4	217	0.13	−0.19, 0.45	5.12	
> 16	10	653	0.18	−0.07, 0.43	31.70***	
Age start					6.53	.04
Prenatal	4	340	0.23	−0.26, 0.72	20.87***	
< 6 months	7	371	−0.03	−0.15, 0.22	5.41	
> 6 months	12	544	0.31**	0.09, 0.52	22.06*	
Focus × Sessions[a]					8.68	.01
Sn × < 16 Sessions	9	415	0.33*	0.09, 0.58	16.28*	
Other × < 16 Sessions	4	187	−0.06	−0.33, 0.21	0.07	
Sn × > 16 Sessions	1	48	0.86**	0.37, 1.34	0.00	
Other × > 16 Sessions	9	605	0.11	−0.14, 0.35	24.49**	
Sample						
SES					0.46	.50
Middle/high	10	492	0.11	−0.12, 0.33	17.52*	
Low	13	763	0.27*	0.04, 0.49	37.14***	
Multirisk					0.05	.83
Yes	11	736	0.22	−0.04, 0.47	41.66***	
No	12	519	0.19*	0.02, 0.36	13.50	
Clinical					0.05	.82
Yes	6	369	0.15	−0.04, 0.34	5.52	
No	17	886	0.22*	0.01, 0.43	49.60***	
Insecure[b]					16.66	< .001
≤ 33%	11	593	−0.09	−0.26, 0.07	12.53	
34%–50%	5	227	0.28*	0.06, 0.50	2.97	
≥ 51%	6	389	0.45**	0.17, 0.74	14.09*	
Outcome					5.80	.02
SSP	19	1,046	0.12	−0.04, 0.29	39.40**	
Other	4	209	0.52*	0.12, 0.92	9.12*	
Sensitivity effect size					18.60	.001
≤ 0.15	6	302	0.17	−0.11, 0.45	9.41	
0.16–0.40	5	384	−0.12	−0.40, 0.16	9.97*	
≥ 0.41	8	378	0.45***	0.26, 0.65	8.52	

Note. Effect sizes (*d*s) were calculated with one-tailed alpha set at .05. *k* = fail-safe number of studies; *n* = total number of participants; CI = confidence interval; SES = socioeconomic status; SSP = Strange Situation Procedure.
[a] Contrast was tested without subgroup of $k < 4$ studies. [b] Contrast was tested without missing data.
* $p < .05$. ** $p < .01$. *** $p < .001$.

tion) were the only interventions that showed a significant effect size. That is, these sensitivity-focused interventions managed to significantly affect infant attachment security ($d = 0.39$, $p < .01$, $k = 10$, $n = 463$), and the difference with the combination of all other intervention categories was significant ($Q = 10.99$, $p < .001$). Short interventions (fewer than five sessions; $d = 0.27$), interventions starting after the age of 6 months ($d = 0.31$), and interventions that did not make use of video feedback ($d = 0.25$) yielded significant effect sizes on infant attachment security (see Table 6), but the difference with other interventions was significant only for interventions starting later and for interventions without video feedback.

Analogously to the procedure we used regarding sensitivity, we constructed a composite variable based on the combination of focus and number of sessions of the interventions (see Table 6). The contrast was significant ($Q = 8.68$, $p = .01$): Again, short interventions focusing on sensitivity only were most effective ($d = 0.33$). Interventions with other foci were less effective (fewer than 16 sessions: $d = -0.06$; more than 16 sessions: $d = 0.11$).

Most characteristics of the samples (SES, clinical referrals, and the presence of multiple risk factors) were not significant moderators and were not associated with differences in effect sizes between the studies (see Table 6). More insecurity in the control groups was associated with larger effect sizes ($Q = 16.66$, $p < .001$). A high percentage of insecurity may make it easier for the intervention group to outperform the control group; it prevents a ceiling effect from dampening the intervention effectiveness.

Higher effect sizes were found for intervention studies with other outcome measures ($d = 0.52$) than for intervention studies with Ainsworth's Strange Situation Procedure ($d = 0.12$; $Q = 5.80$, $p = .02$). Measures with continuous attachment scores may be more sensitive to modest changes and thus more powerful to show an intervention effect. Although attrition may endanger the outcomes of intervention studies (as it did in the case of sensitivity), there was no significant difference in effect sizes for attachment in studies with more or less attrition (see Table 6).

In the larger set of studies ($k = 29$), including nonrandom intervention studies, similar results were found (see Table 7). Only sensitivity-focused interventions managed to significantly affect infant attachment security ($d = 0.35$, $p < .01$). Intervention studies that did not use video feedback ($d = 0.24$) were more effective than interventions involving video feedback ($d = 0.06$; $Q = 5.33$, $p = .02$). The difference in effect size between interventions that started early in an infant's life and interventions that started after the age of 6 months was, however, not significant in the larger set of studies. It should be noted that the random studies were not significantly less effective in promoting attachment security than the other studies in our data set (random studies: $d = 0.20$; nonrandom studies: $d = 0.13$; $Q = 0.17$, $p = .68$).

Are shorter and behaviorally focused attachment interventions also more effective in groups with multiple risks, or do troubled families require more intensive interventions? We repeated the meta-analyses on the effectiveness of intervention modalities in the subset of randomized studies of multiproblem families ($k = 15$, $n = 971$); the combined effect size was similar to the effect size in the total set of randomized attachment studies ($d = 0.19$, $p < .05$). Again, interventions focusing on sensitivity only appeared most effective ($d = 0.34$, $p < .05$), and the difference with other interventions ($d = 0.10$) was significant ($Q = 6.24$, $p = .01$). Results for video feedback, number of sessions, and children's age at start were comparable to the outcomes in the total set of randomized attachment interventions (see Table 8). Behaviorally focused attachment interventions and interventions with a relatively late start appeared most effective in enhancing children's attachment security.

Are successful sensitivity interventions also more effective in enhancing infant attachment security? The effect sizes for sensitivity were categorized into the following three categories: less than or equal to 0.15, between 0.16 and 0.40, and greater than or equal to 0.41. In the set of randomized studies, we found a significant difference ($Q = 13.59$, $p = .001$) among the three categories (see Table 6). The studies with the largest effect sizes for sensitivity ($d > 0.40$) were also the most effective in enhancing the children's attachment security ($d = 0.45$, $p < .001$). In the total set of attachment interventions, a similar significant difference was found ($Q = 10.21$, $p = .006$): The most effective sensitivity interventions ($d > 0.40$) showed a larger effect size for attachment ($d = 0.35$; see Table 7). In fact, the only intervention studies that yielded a significant effect size on attachment security were the studies with a large effect size on sensitivity (when data on sensitivity were available).

Discussion and Conclusions

In sum, the current meta-analytic evaluation shows the effectiveness of various types of interventions for enhancing maternal sensitivity and—to a lesser extent—infant attachment security. In particular, less broad interventions that only focus on sensitive maternal behavior appear rather successful in improving insensitive parenting as well as infant attachment insecurity. The most effective interventions did not always use a large number of sessions with the families, and they did not necessarily start early in life or even before birth. On the contrary, we found evidence for fewer contacts being somewhat more effective, and the same was true for interventions starting 6 months after birth or later. It should be noted that the very large effect on sensitivity of Van den Boom's (1994) brief intervention study, although excluded as an outlier on preset criteria, was consistent with these meta-analytic results. Highly effective intervention modalities were effective regardless of the presence or absence of multiple problems in the family. Overall, most sample characteristics were not associated with effectiveness of the interventions, with two notable exceptions: (a) In samples with a higher percentage of insecurity in the control group, the interventions achieved a larger effect on infant attachment than in more normative samples and (b) in clinical groups interventions had a greater effect on parental sensitivity. This larger effect size for clinical groups may be explained by the fact that in these samples the problems were more or less restricted to one area (e.g., maternal depression, anxious–withdrawn children), which the interventions could effectively handle; moreover, these interventions may have involved highly motivated parents.

On the whole, interventions that were more effective in the total randomized set were also more effective in the subset of clinical and high-risk samples. This indicates that the effectiveness of certain types of interventions was not confounded with characteristics of the various samples. The multivariate approach further highlighted the effectiveness of sensitivity-focused interventions (and a later start of the intervention), even after controlling for

Table 7
Meta-Analytic Results of Attachment Interventions (k = 29 Study Outcomes)

Characteristic	*k*	*n*	*d*	90% CI	*Q*	*p*
Total set	29	1,503	0.19*	0.05, 0.33	62.88***	
Random/control					0.17	.68
Yes	23	1,255	0.20*	0.04, 0.35	55.21***	
No	6	248	0.13	−0.18, 0.45	7.49	
Attrition					3.43	.18
0%	9	365	0.13	−0.17, 0.44	16.62*	
1–20%	13	723	0.11	−0.08, 0.31	25.53*	
> 20%	7	415	0.34*	−0.07, 0.62	16.92**	
Focus					8.89	.003
Sensitivity only	11	513	0.35**	0.13, 0.57	20.79*	
Other	18	990	0.08	−0.08, 0.25	32.65*	
Specified						
Sensitivity (Sn)	11	513	0.35**	0.13, 0.57	20.79*	
Support (Sp)	4	176	0.34	−0.10, 0.79	8.32*	
Representation (R)	3	153	0.10	−0.34, 0.54	3.40	
Sn + Sp	4	266	−0.08	−0.28, 0.13	0.72	
Sn + R	3	106	−0.05	0.46, 0.35	0.11	
Sn + R + Sp	4	289	0.13	−0.34, 0.59	14.31**	
Video					5.33	.02
No	21	1,121	0.24**	0.09, 0.39	38.29**	
Yes	8	382	0.06	−0.25, 0.36	18.66**	
Sessions					2.25	.32
< 5	10	435	0.24*	0.01, 0.47	16.58	
5–16	6	284	0.08	−0.21, 0.36	6.87	
> 16	13	784	0.20	−0.01, 0.42	37.21***	
Age start					3.26	.20
Prenatal	4	340	0.23	−0.26, 0.72	20.87***	
< 6 months	8	409	0.09	−0.10, 0.28	8.41	
> 6 months	17	754	0.23*	0.05, 0.42	30.22*	
Sample						
SES					2.37	.12
Middle/high	14	670	0.05	−0.13, 0.23	20.57	
Low	15	833	0.31**	0.10, 0.51	39.63***	
Multirisk					0.50	.48
Yes	13	806	0.27*	0.04, 0.50	44.67***	
No	6	697	0.12	−0.03, 0.27	17.65	
Clinical					0.28	.60
Yes	10	535	0.12	−0.06, 0.30	11.37	
No	19	968	0.22*	0.03, 0.41	51.1***	
Insecure[a]					15.61	< .001
≤ 33%	12	643	−0.09	−0.24, 0.06	12.69	
34–50%	6	259	0.32**	0.11, 0.52	3.51	
≥ 51%	8	488	0.39**	0.13, 0.66	20.15**	
Outcome					5.96	.01
SSP	25	1,294	0.13	−0.02, 0.27	46.88**	
Other	4	209	0.52*	0.12, 0.92	9.12*	
Sensitivity effect size[a]					15.14	< .001
≤ 0.15	8	401	0.17	−0.08, 0.41	13.45	
0.16–0.40	6	418	−0.11	−0.37, 0.15	10.14	
≥ 0.41	10	461	0.35**	0.14, 0.56	13.94	

Note. Effect sizes (*d*s) were calculated with one-tailed alpha set at .05. *k* = fail-safe number of studies; *n* = total number of participants; CI = confidence interval; SES = socioeconomic status; SSP = Strange Situation Procedure.
[a] Contrast was tested without missing data.
* $p < .05$. ** $p < .01$. *** $p < .001$.

characteristics of the sample. Because of the number of studies required for this approach, the multivariate test was possible only in the total set of studies on sensitivity. It should be noted, however, that in this test randomization was used as a predictor and did not play a part in predicting effect size.

The meta-analyses were based on the three-way attachment classifications (ABC) and did not address attachment disorganization explicitly because the interventions did not aim at preventing or changing disorganization. However, Lyons-Ruth and Jacobvitz (1999) argued that attachment disorganization may result not only from a frightening or frightened parent but also from an extremely insensitive or neglecting parent and that, if so, interventions aiming at enhancing sensitivity may reduce disorganization. Although we found that interventions did affect attachment insecurity, they

Table 8
Meta-Analytic Results of Attachment Interventions: Multiproblem Samples (Random Studies)

Characteristic	*k*	*n*	*d*	90% CI	*Q*	*p*
Multiproblem Sample total	15	971	0.19*	0.01, 0.38	44.61***	
Focus					6.24	.01
Sensitivity only	5	274	0.34*	0.06, 0.73	14.03**	
Other	10	697	0.10	−0.12, 0.31	24.09**	
Specified						
Sensitivity (Sn)	5	274	0.34*	0.06, 0.73	14.03**	
Support (Sp)	2	104	0.44	−0.43, 1.30	6.29*	
Representation (R)	2	120	0.24	−0.12, 0.60	1.27	
Sn + Sp	2	165	0.00	−0.26, 0.26	0.00	
Sn + R	1	57	−0.10	−0.59, 0.39	0.00	
Sn + R + Sp	3	251	−0.03	−0.52, 0.46	9.86**	
Video					5.77	.02
No	11	707	0.24*	0.05, 0.43	21.11*	
Yes	4	264	0.03	−0.50, 0.56	17.24***	
Sessions					3.08	.21
< 5	4	229	0.24	−0.24, 0.72	12.90**	
5–16	4	217	0.13	−0.19, 0.46	5.12	
> 16	7	525	0.17	−0.12, 0.46	23.48***	
Age start					5.98	.05
Prenatal	4	340	0.23	−0.26, 0.72	20.87***	
< 6 months	4	242	−0.04	−0.27, 0.19	0.10	
> 6 months	7	389	0.29	0.01, 0.58	17.40**	

Note. Effect sizes (*d*s) were calculated with one-tailed alpha set at .05. *k* = fail-safe number of studies; *n* = total number of participants; CI = confidence interval.
* $p < .05$. ** $p < .01$. *** $p < .001$.

were not significantly effective in the subset of studies presenting the four-way classifications, which included the disorganized category. It may be noted that no intervention focused on influencing parents' frightened or frightening behavior toward their children as established determinants of disorganized attachment (Van IJzendoorn, Schuengel, & Bakermans-Kranenburg, 1999).

Highly intensive interventions with numerous sessions focusing on sensitivity, representation, and support show disappointingly small or even negative effect sizes on attachment security (combined $d = -0.03$). In particular, studies with negative outcomes show that well-intended interventions may be counterproductive and even produce psychologically iatrogenic effects. It should be noted, however, that the number of studies with negative outcomes was very small (see Figure 1), as was the set of randomized studies that combined behavioral, representational, and supportive interventions. Only three studies were included in this subset (Egeland & Erickson, 1993; Heinicke et al., 1999; Lieberman et al., 1991), and they were conducted with multiproblem families. One of them, Heinicke et al.'s (1999) intervention, was effective. Long-term and broadly focused support of multiproblem families in coping with their daily hassles may be badly needed to enable the intervenor to subsequently focus on sensitivity and representation. This broadband effort may, however, take too much time and energy away from a potentially effective, goal-directed intervention approach. Nevertheless, broadband interventions may have been effective on outcome measures that were not included in our analyses (e.g., parental satisfaction, perceived social support, or quality of the marital relationship).

A particular obstacle for long-term interventions in families with multiple problems is differential attrition. It may be difficult for the control group to remain motivated because control participants face serious problems but do not experience support from the intervention project, and there is a real risk of the most problematic control families to drop out from the investigation. Thus, the comparison with the experimental group is jeopardized. Differential attrition could be part of the explanation of the negative intervention effect in Barnard et al.'s (1988) and Beckwith's (1988) studies, as the attrition in their control groups (47% and 36%, respectively) was much larger than the dropout rate in their intervention groups (20% and 5%, respectively).

To counteract differential attrition, the possibility of providing the control group with a dummy treatment that is largely similar to the intervention group's program but lacks only one or two ingredients should be seriously considered. For example, the influence of a sensitivity intervention may be tested in combination with supporting the families in coping with their daily hassles, both in the experimental and in the control group. The effectiveness of the sensitivity component of the intervention can thus be tested without failing one's duties toward the control group. This approach combines two advantages that characterize adequate designs: First, if the intervention is more successful than the dummy treatment, one would know more specifically what ingredient was effective. Even broadband interventions would become more easily evaluated. Second, differential attrition could be prevented because control participants would feel their problems were being taken seriously, and ethical problems with abstaining from support to at-risk families would not arise (see Van IJzendoorn & Bakermans-Kranenburg, 2002). In some of the intervention studies we reviewed, no control group was used because withholding at-risk families a potential beneficial program was considered ethically problematic (Brinich, Drotar, & Brinich, 1989). Although the ethical rationale is understandable and commendable, the resulting

research designs are seriously flawed because they lack a baseline with which to compare the intervention group.

Broadband interventions in families with multiple problems also run the risk that a significant treatment effect holds only for the participants of that specific sample and not for a larger group with similar problems. Especially when experimenters experience serious difficulties recruiting participants for their intervention, the sampling may not be representative. As a result, generalizability to a larger group may be compromised. In the same vein, generalizability might be restricted because of the use of specific measures, observers, or intervenors. This issue may be especially relevant for intervention studies in which the person who implemented the intervention is also the principal investigator (e.g., Scholz & Samuels, 1992; Van den Boom, 1988, 1994). In medical science, the ideal design is a rigorous double-blind procedure, in which both researchers and participants are uninformed of the memberships of the experimental and control groups. It is well-documented in the experimental literature how knowledge of what is expected to happen in an experiment can work like a self-fulfilling prophecy (Rosenthal, 1994), even when investigators, intervenors, and participants have the best intentions. It is of crucial importance to replicate a successful intervention with other or more intervenors to control for personal factors and expectancy effects. Expectancies may also seriously inflate effect size when coders of outcome behaviors are aware of the intervention or control condition of the participants (as in Weiner, Kuppermintz, & Guttmann, 1994).

In the total set of studies, interventions involving fathers appear to be significantly more effective than interventions focusing on mothers only. This surprising outcome is based on an analysis with only three studies with fathers, comprising 81 participants. Several authors have argued that families should be considered a system (e.g., Cowan, 1997) and that interventions should use the system characteristics of the family to enhance the effectiveness of interventions. Egeland et al. (2000), for example, suggested that family interventions should involve mothers as well as fathers to strengthen the intervention's influence and to stimulate family support for changes in maternal behavior. Although the division of child-rearing tasks and roles is rather skewed even in modern families with two breadwinners (VanDijk & Siegers, 1996), fathers take part in rearing their children and may benefit from interventions as much as mothers do. Furthermore, fathers involved in preventive interventions may motivate their partners to continue participation and to practice new behaviors at home. It should be noted, however, that paternal involvement may be counterproductive as far as the mothers are concerned. In two of the three studies involving fathers, the effects on paternal sensitivity were large, but similar effects on maternal sensitivity were absent. In fact, in one study the mothers showed much less improvement in their sensitivity than the fathers (Scholz & Samuels, 1992), and in the other study the intervention effects were even negative for mothers (Dickie & Gerber, 1980). In the third study, separate effect sizes for mothers and fathers could not be computed (Metzl, 1980). Several explanations for these findings may be considered. First, if fathers are included in the intervention efforts, less attention might be paid to the mothers' needs and abilities. Second, when fathers are also involved in the intervention, mothers may underestimate the importance of their own practicing new child-rearing insights and skills. Alternatively, if both parents are assessed in the same interaction setting, fathers who are more active than their counterparts might provide less opportunity for mothers to show their new skills acquired during the intervention. Replications may shed light on the positive as well as the possible negative outcomes of interventions involving fathers and mothers.

In 24 intervention studies ($n = 1{,}280$), both maternal sensitivity and children's attachment security were assessed as outcome measures. On the basis of the moderately strong association between sensitivity and attachment security (De Wolff & Van IJzendoorn, 1997), our hypothesis was that more effective sensitivity interventions would also be somewhat more effective in enhancing children's attachment security. We posited that if maternal sensitivity was an important determinant of infant attachment, stimulating sensitivity would lead to changes in attachment security and that parallel changes in sensitivity and attachment could be therefore expected. The association between sensitivity and attachment effect sizes confirms the hypothesis of a causal link between sensitivity and attachment security. In the set of randomized studies as well as in the larger set of attachment intervention studies, we found that sensitivity interventions with rather large effect sizes ($d > 0.40$) were also most effective in enhancing infant attachment security (ds $= 0.45$ and 0.35, respectively). Less effective sensitivity interventions did not manage to bring about changes in attachment security.

In general, attachment insecurity is more difficult to change than maternal insensitivity, as is documented by the differences in effect sizes for sensitivity ($d = 0.33$) and attachment ($d = 0.20$). However, when an intervention is rather successful in enhancing maternal sensitivity, this change appears to be accompanied by a parallel positive change in infant attachment security. The reason we did not find similar correspondences in the lower ranges of sensitivity effect sizes may be that interventions usually aim at enhancing maternal sensitivity but assess sensitivity and attachment outcomes at about the same time, that is, shortly after the intervention sessions. Small but significant changes in maternal sensitivity may not have had the chance to affect infant attachment security. A sleeper effect on attachment security might remain undetected. Further research on this issue of a time lag between changes in sensitivity and attachment is needed and should take this possibility of a sleeper effect into account. Longer term follow-up studies of early interventions may provide information on such sleeper effects and/or the permanency of any intervention effects.

Intervention studies sometimes suffer from methodological flaws, such as an unclear focus of intervention, a diffuse implementation of the program, or the absence of a specific protocol for implementing the intervention. Small samples may lack the statistical power to detect a moderate intervention effect, and randomization with a small number of participants may result in systematic differences between control and experimental groups. Sometimes a ceiling effect is evident, when relatively high scores in the control group prevent any intervention effects from being detected (e.g., Egeland & Erickson, 1993). Also, sleeper effects may make it difficult to correctly evaluate the program's effectiveness, as only long-term effects are to be expected in cases of deeply rooted relational or representational characteristics. The ideal intervention study emerges as a randomized design with a dummy-treated control group and a pretest to detect and compensate for possible randomization failures. The intervention should be carefully described in a protocol, and implementation and

evaluation of the intervention should be independent. The current meta-analytic data suggest that interventions with a clear focus and a modest number of sessions are preferable. The dodo bird verdict is not appropriate for the field of sensitivity and attachment interventions. Interventions with an exclusively behavioral focus on maternal sensitivity appear to be most effective not only in enhancing maternal sensitivity but also in promoting children's attachment security.

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