Narrowing the Transmission Gap: A synthesis of three decades of research on intergenerational transmission of attachment

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Abstract

Twenty years ago, meta-analytic results (*k* = 19) confirmed the association between caregiver attachment representations and child-caregiver attachment (Van IJzendoorn, 1995). A test of caregiver sensitivity as the mechanism behind this intergenerational transmission showed an intriguing "transmission gap". Since then, the intergenerational transmission of attachment and the transmission gap have been studied extensively, and now extend to diverse populations from all over the globe. Two decades later, the current review revisited the effect sizes of intergenerational transmission, the heterogeneity of the transmission effects, and the size of the transmission gap. Analyses were carried out with a total of 95 samples (total *N* = 4,819). All analyses confirmed intergenerational transmission of attachment, with larger effect sizes for secure-autonomous transmission (*r* = .31) than for unresolved transmission (*r* = .21), albeit with significantly smaller effect sizes than two decades earlier (*r* = .47 and *r* = .31, respectively). Effect sizes were moderated by risk status of the sample, biological relatedness of child-caregiver dyads, and age of the children. Multivariate moderator analyses showed that unpublished and more recent studies had smaller effect sizes than published and older studies. Path analyses showed that the transmission could not be fully explained by caregiver sensitivity, with more recent studies narrowing but not bridging the "transmission gap". Implications for attachment theory as well as future directions for research are discussed.

Keywords: attachment, intergenerational transmission, meta-analysis, caregiver sensitivity, transmission gap

Continuities across generations have intrigued researchers investigating multiple domains of human functioning, such as parenting (e.g., Kovan, Chung, & Sroufe, 2009), psychopathology (e.g., Kim, Capaldi, Pears, Kerr, & Owen, 2009), and attachment (Main, Kaplan, & Cassidy, 1985). Although parents transmit genetically based traits to their offspring, it is clear that environmental mechanisms are involved as well. Regarding cross-generational continuity in patterns of attachment, defined as a “lasting psychological connectedness between human beings” (Bowlby, 1969/1982, p.194), little evidence supports genetic transmission, judging on the basis of behavioral genetic (e.g., Bokhorst et al., 2003; Fearon et al., 2006; Roisman & Fraley, 2008) as well as molecular genetic studies (e.g., Bakermans-Kranenburg & van IJzendoorn, 2004; Luijk et al., 2011). Rather, attachment theory provides a psychological –and environmental—account of intergenerational transmission. Bowlby hypothesized that attachment experiences are carried forward as people adapt to the affective impact of those experiences by forming internal working models of attachment relationships. These models guide perceptions and responses in existing and future relationships (Ainsworth, Blehar, Waters, & Wall, 1978; Bowlby, 1969/1982). This idea was further refined by Main, Kaplan, and Cassidy (1985), who proposed that adults organize attachment-relevant information in a mental representation of attachment, which provides “a set of conscious and/or unconscious rules for the organization of information relevant to attachment and for obtaining or limiting access to that information” (Main et al., 1985, p. 67). They showed that qualitative differences among adults’ narratives about their attachment experiences, presumably caused by their attachment representation, were closely associated with the quality of attachment relationships with their own children.

A meta-analysis of the first wave of studies (k = 19) on intergenerational transmission of patterns of attachment strongly supported the ideas of Main and her colleagues, although important questions remained about the actual parent-child interactions that could explain this transmission (Van IJzendoorn, 1995). Two decades and many studies later it is time to revisit the robustness of this fundamentally important scientific question by taking into account the possible presence of publication bias and a decline effect (diminishing effect sizes over time; Schooler, 2011). The sheer volume of research now in existence enables testing the universality of intergenerational transmission across normative and risk populations, biological and non-biological dyads, and different ages of the children, as well as testing with precision the role of parental sensitivity as a key factor in this transmission.

Main and her colleagues (1985) distinguished adult attachment representations from the quality of attachment experiences that adults may or may not remember from their youth. Rather, the affective and cognitive adaptation to favorable or unfavorable attachment experiences was identified as the focus of investigation of mental representations of attachment. A primary indicator of this psychological adaption was narrative coherence of an individual's discourse during a standardized interview about early and current attachment experiences (Hesse, 2008). Coherent narratives were characterized by being believable and not contradictory, complete yet succinct, relevant with respect to the questions, and readily understandable to the listener. Parents with a secure-autonomous attachment representation, who openly value attachment, have access to detailed memories, and appear relatively free from defensive bias, more often had secure attachment relationships with their children, characterized by openly seeking reassurance from their caregivers in times of distress which facilitates children's exploration of the environment, than parents with insecure non-autonomous narratives (See Table 1). Two forms of insecure non-autonomous attachment representations were also identified, dismissing and preoccupied. Dismissing attachment representations are indicated by minimizing the importance of attachment experiences, idealization, or blocked access to childhood attachment memories. Preoccupied attachment representations are indicated by current anger, confusion, and preoccupation with current or past attachment experiences. Main and colleagues (1985) found that parents with dismissing attachment representations more often had avoidant attachment relationships with their children, in which children limited the expression of attachment signals and shifted attention away from their caregivers. Parents with preoccupied representations more often had resistant attachment relationships with their children, in which children were highly vigilant about their attachment figures' whereabouts, easily distressed and angered, and difficult to sooth after disruptions in contact with attachment figures. In addition to these categories of organized attachment representations, disorganized or disoriented speech during the discussion of physical or sexual abuse by attachment figures or losses of attachment figures indicated the existence of unresolved-disorganized representations of these experiences (Main & Hesse, 1990). Parents' unresolved representations were associated with disorganized attachment relationships with their children, characterized by temporary lapses and contradictions in their patterns of attachment behavior (Main & Solomon, 1990). The distinctions between autonomous and non-autonomous representations and between organized and disorganized representations have been combined in various ways in the extant research, giving rise to four-way distributions of categories (Secure-Autonomous, Dismissing, Preoccupied, and Unresolved) as well as three-way forced distributions (Secure-Autonomous, Dismissing, and Preoccupied) when disorganization is disregarded (See Table 1).

As attachment representations were hypothesized to guide caregivers' perceptions and behavior in relationships, caregivers' sensitivity in response to their children was thought of as the mechanism behind attachment transmission. In one of the earliest studies, Ainsworth and colleagues (1978) showed that mothers who responded more sensitively were more likely to form secure attachment relationships with their children when compared to less sensitively responsive mothers. This result was later confirmed meta-analytically (De Wolff & Van

IJzendoorn, 1997). Conceptually, caregivers' ability to respond sensitively to their children's needs was thought to be rooted in their attachment representations. In the case of secure-autonomous representations, caregivers would be least prone to bias regarding the signals of their children. In contrast, caregivers with dismissing representations may downplay or disregard signals and would be limited in their responses, and caregivers with preoccupied representations would be more likely to miss or misinterpret signals and thus be inconsistently responsive. These hypotheses have been partly supported by research (Whipple, Bernier, & Mageau, 2011). Further, recent research has uncovered some of the neural mechanisms related to these differences in response patterns. Specifically, there are individual differences across attachment representations in the activation of brain structures involved in the processing of emotions, threat recognition, and reward processing (e.g., Lenzi et al., 2013; Riem, Bakermans-Kranenburg, van Ijzendoorn, Out, & Rombouts, 2012; Strathearn, Fonagy, Amico, & Montague, 2009), which are part of a complex neural circuit that is thought to drive sensitive parenting (Swain, 2011).

Ten years after the initial theory on intergenerational transmission of attachment was proposed (Main et al., 1985), Van IJzendoorn (1995) meta-analytically confirmed the associations between caregiver representations and child-caregiver attachment, with the combined effect sizes for the maternal and infant attachment pairings ranging from $r = .47$ for forced autonomous classifications ($k = 18$) to $r = .19$ for the four-way preoccupied classifications ($k = 9$). The role of caregiver sensitivity in attachment was also examined. Results showed that caregiver sensitivity was associated with both attachment representations and attachment relationships, but it could explain only part of the intergenerational transmission, thus leaving a "transmission gap" (Van IJzendoorn, 1995, p. 398).

Explaining the heterogeneity of effect sizes: possible moderators of intergenerational transmission

The meta-analysis in 1995 showed considerable heterogeneity in effect size of the intergenerational transmission of attachment. However, limited diversity in the studies included in this meta-analysis precluded thorough examination of between-study heterogeneity at the time. This heterogeneity may be explained by two types of factors: (1) substantive factors that may affect the phenomenon of intergenerational transmission itself, and (2) methodological factors that determine how closely studies approach the phenomenon. Based on theory and previous research, four substantive factors that could affect intergenerational transmission of attachment were identified.

Risk status of a sample. Over the last decades, considerable interest has been devoted to studying attachment in non-normative populations, such as mothers suffering from clinical depression. Meta-analytic results have shown that children of depressed mothers were more likely to develop insecure attachment (Atkinson et al., 2000) as well as disorganized attachment relationships with their mothers (van IJzendoorn, Schuengel, & Bakermans-Kranenburg, 1999). Prevalence rates of non-autonomous and unresolved attachment representations were also higher in clinical samples than in non-clinical samples (Bakermans-Kranenburg & van IJzendoorn, 2009). Similarly, samples with known risks, such as children with adolescent mothers, those born prematurely, and samples with child or caregiver psychopathology, had a higher prevalence of insecure attachment than normative samples (Cyr, Euser, Bakermans-Kranenburg, & van IJzendoorn, 2010; Goldberg, 1997). Accordingly, their caregivers' attachment representations were more often non-autonomous or unresolved (Bakermans-Kranenburg & van IJzendoorn, 2009). Cyr and colleagues (2010) have suggested that, regardless of caregivers' attachment representations, several pathways could lead to insecure and disorganized attachment in high-risk families. First, caregivers may be more consumed by other aspects of their lives (e.g., depression, financial stresses, teenage parenthood) and are therefore less likely to provide consistently sensitive care toward their children. Another pathway is through exposure to domestic violence or abuse, which occurs more often in samples with known risk (Coohey & Braun, 1997) and places children at heightened risk of developing disorganized attachment (Owen & Cox, 1997). Risk status of a sample may therefore moderate the strength of intergenerational transmission of attachment. A meta-analytic examination of this variable as moderators is novel for the field, as the initial meta-analytic synthesis by van IJzendoorn (1995) had a very limited number of studies with known risks.

Biological versus non-biological caregivers. Similarly, studies of foster care and adoption dyads may yield differing estimates for intergenerational transmission (Bernier & Dozier, 2003; Jacobsen, Ivarsson, Wentzel-Larsen, Smith, & Moe, 2014; van Londen-Barentsen, 2002). A recent meta-analysis that compared attachment in adopted children, foster children, and non-adopted children indicated that the rates of insecure and disorganized attachment of foster children and adopted children are both higher than these rates in non-adopted children (van den Dries, Juffer, van IJzendoorn, & Bakermans-Kranenburg, 2009). As previous studies have shown a consistent lack of genetic effects on early attachment (Bakermans-Kranenburg & van IJzendoorn, 2004; Bokhorst et al., 2003; Fearon et al., 2006; Luijk et al., 2011; Roisman & Fraley, 2008), the absence of genetic similarity between caregivers and their children seems an unlikely, though not inconceivable, explanation for the differences between biologically related and non-biologically related child-caregiver dyads. Perhaps more likely explanations concern study- and population characteristics, for instance, the shorter history the dyads have had with each other compared to birth parents. Later placement was found to be a significant predictor of attachment insecurity (van den Dries et al., 2009). Also, the fact that these children often had negative previous experiences with attachment (Lionetti, Pastore, & Barone, 2015; Milan & Pinderhughes, 2000) may cause the intergenerational transmission of attachment to be less prominent in non-biologically related dyads. On average, these samples also include somewhat older children, which may affect the transmission effect. This confound can only be disentangled using multivariate analyses of moderator effects, which is only possible when the number of eligible studies is sufficient.

Gender of the parent. Intergenerational transmission of attachment may be different for fathers than for mothers. In Van IJzendoorn's (1995) meta-analysis of the transmission of forced autonomous classifications, maternal effects were considerably stronger than paternal effects ($r = .55$ vs $r = .37$). The association between sensitivity and attachment has also been shown to be weaker in fathers than in mothers (De Wolff & Van IJzendoorn, 1997), and the effect size of this association appeared unchanged for fathers over the last 3 decades (Lucassen et al., 2011). These findings suggest that the effect size of intergenerational transmission of attachment may still be smaller for fathers than for mothers.

Children's age. Children's age may impact effect sizes of intergenerational transmission of attachment. A central tenet of attachment theory is that the quality of attachment experiences is rooted in the history of interactions between the two partners in the relationship. This implies that if attachment representations remain a stable influence on those interactions, the strength of intergenerational transmission must increase with age of the child. However, Van IJzendoorn's meta-analysis (1995) showed smaller effect sizes in several analyses of intergenerational transmission for studies conducted with older children, although the number of older samples was small. Over the last decades, intergenerational transmission has been studied in more age-diverse samples, enabling the current review to revisit this hypothesis.

Besides these substantive factors that could affect attachment transmission, many methodological factors might also affect the effect size of attachment transmission. Especially important are methodological factors that might be associated with substantive factors, because this may lead to spurious attributions of moderation. Study design may impact intergenerational transmission of attachment, because the meta-analysis in 1995 showed that several effect sizes were higher for concurrent than for longitudinal designs. Longer time-intervals between the assessment of sensitivity and attachment have also been associated with smaller effect sizes (De Wolff & Van IJzendoorn, 1997), but not in a recent meta-analysis with fathers (Lucassen et al., 2011). This methodological factor may confound the effect of the substantive factor of child age. Training status of the coders was hypothesized to impact effect size, because attachment measures typically require intensive training; therefore, larger measurement error (accompanied by lower effect sizes) are to be expected in studies in which coders lack official training. This methodological factor may be associated with research conducted in more applied, clinical settings, potentially confounding the moderating effect of risk status.

Revisiting the "transmission gap"

As stated above, Van IJzendoorn (1995) reported that effect sizes linking attachment representations to parental sensitivity and parental sensitivity to quality of attachment only accounted for a surprisingly small portion of the intergenerational transmission effect. The mediating pathway explained 25% of the association between caregiver attachment representation and caregiver-child attachment. However, in order to test this mediation pathway, the transmission model had to be estimated using path coefficients derived from non-overlapping sets of studies. This left open the possibility of both underestimation and overestimation of the transmission gap, because path coefficients were estimated with varying precision given the varying number of studies (ranging from k = 10 to k = 18) and participants (ranging from N = 389 to N = 854). In addition, the path analyses were based on correlation coefficients that were attenuated due to imperfect measurement reliability. Multiplying those attenuated coefficients in the path analyses could lead to further deflation of the mediating pathway. One could thus argue that the transmission gap might be spurious due to measurement imprecision, although it is interesting to note that more recent studies that examined the entire mediation model have also found limited mediation (e.g., Pederson, Gleason, Moran, & Bento, 1998; Tarabulsy et al., 2005). The current meta-analysis addresses the issues above by using reliability data of the variables under study to investigate whether correcting the path coefficients for attenuation as well as the considerably larger number of studies (and participants) changes the transmission gap. Furthermore, due to the burgeoning literature in the past 20 years examining the transmission gap, there are now a sizable number of studies that included all three variables of interest, further reducing error and increasing confidence in any consistently observed mediation effects.

The Current Study

The primary aims of the current study are to synthesize 30 years of research on intergenerational transmission of attachment in a series of comprehensive meta-analyses in order to thoroughly evaluate the status of the intergenerational transmission of attachment. To gain insight into publication bias, we will investigate whether differences in effect size are apparent for published versus unpublished studies, as research in many fields has shown a publication bias towards significant results (Fanelli, 2010, 2012). The possibility of a "decline effect", with more recent studies reporting smaller effect sizes than older studies, will also be tested (Schooler, 2011). Furthermore, we will examine the effects of sample characteristics that might moderate the magnitude of the transmission effect. The current study will also examine the mediating role of caregiver sensitivity by synthesizing the research into a path model of mediation containing effect sizes of all studies that included caregiver sensitivity in addition to caregiver attachment representations and child-caregiver attachment. Further, we examine the specific possibility that measurement error for sensitivity and attachment affects the transmission gap by correcting paths for attenuation based on meta-analytic estimates of inter-coder reliability and test-retest reliability.

Method

Data collection

Identification of studies, screening, and assessment of eligibility for inclusion in the meta-analytic dataset are depicted in Figure 1. Four methods were used to identify studies. The first method was to search in the bibliographic databases of PsychINFO, Web of Science, ERIC, and CINAHL. The search terms were: *("adult attachment" OR "adult attachment interview" OR "mother attachment" OR "father attachment" OR "parent* attachment" OR "maternal attachment" OR "paternal attachment") AND ("attachment relationship" OR "infant-mother attachment" OR "infant-father attachment" OR "mother-child relationship" OR "parent-child relationship" OR "strange situation" OR "attachment Q-set" OR "disorganized attachment" OR "attachment Q-sort").* The second method was to peruse the reference lists of retrieved papers and existing literature reviews for other relevant publications. The third method was to search for dissertations and unpublished articles in PsychEXTRA and dissertation databases (www.proquest.com, www.narcis.nl, www.ndltd.org, www.dart-europe.eu, www.dissonline.de) based on the search criteria used in the first method. The fourth method was to go over the proceedings of several conferences on child development (e.g., the Society for Research in Child Development, the International Attachment Conference, the International Society of Infant Studies). Of the studies identified in this way, the authors were searched in computerized databases to find published versions of conference papers. If no published version was available, authors were contacted to share the data. Studies available through these methods for the period until July 2014[1] were included.

Titles and abstracts of identified studies were screened and included for further assessment of eligibility if they reported on the association between caregiver representation regarding their attachment experiences with their own caregivers and child-caregiver attachment. Consistent with the focus on attachment experiences and representations involving primary caregivers, only studies using the Adult Attachment Interview (AAI;George, Kaplan, & Main, 1984, 1985, 1996) were included. Studies with the Adult Attachment Projective (George & West, 2004), Secure Base Script measures (H. S. Waters & Rodrigues-Doolabh, 2004), and adult attachment questionnaires (e.g., Experiences in Close Relationships questionnaire; Brennan, Clark, & Shaver, 1998) were excluded because these measures are non-specific with respect to the type of relationship experiences that might or might not be transmitted to the relationship with children, whereas the focus in intergenerational transmission of attachment research is on past and current experiences with attachment figures. Furthermore, lack of convergence between the Adult Attachment Interview and the Adult Attachment Projective indicates that these instruments measure different construct (Jones-Mason, 2011). For similar reasons, studies were only included if child-caregiver attachment was assessed in infancy or early childhood using a behavioral coding measure of the attachment relationship, such as the Strange Situation Paradigm (SSP; Ainsworth et al., 1978), the Attachment Q-Sort (AQS; E. Waters, 1995), the Main-Cassidy attachment classification system (Main & Cassidy, 1988), or the Preschool Attachment Assessment (PAA; Crittenden, 1994). Caregiver-sorted AQS scores were excluded for two reasons. First, in such studies both the AAI and the AQS would be completed by the same informant, which introduced bias in earlier studies (e.g., Stevenson-Hinde & Shouldice, 1995). Second, caregiver sorts have been shown to be less valid than observer sorts (Van IJzendoorn, Vereijken, Bakermans-Kranenburg, & Riksen-Walraven, 2004). Representational measures (e.g., Attachment Story Completion Task; Bretherton, Ridgeway, & Cassidy, 1990) and questionnaire measures of child attachment (e.g., Child-Parent Relationship Scale; Pianta, 1992) were excluded because these yield classifications that are not specific to the quality of the attachment relationship with one particular caregiver and because these measures are less well validated than the behavioral coding measures (Kerns, 2008).

For inclusion in the path-analysis of the mediating effect of caregiver sensitivity, studies had to report on caregiver sensitivity in addition to the association between caregiver attachment representation and child-caregiver attachment. If studies reported on all three concepts in the same manuscript without providing correlations between the variables, authors were contacted to provide correlational data. Studies were included if they reported on caregiver sensitivity or caregiver responsiveness, in accordance with the inclusion criterion for a recent review of observational instruments measuring parental sensitivity (Mesman & Emmen, 2013). Studies using slightly different terminology (i.e., parental behavior) measured with an instrument described in the Mesman and Emmen (2013) review were also included.

To assess eligibility of the studies included during the screening process, several more exclusion criteria were applied. If multiple publications reported on the same or overlapping samples, studies that reported on the largest number of participants were included (e.g., Dozier, Stovall, Albus, & Bates, 2001; Raval et al., 2001). When studies reported on interventions, data were only included if potential moderating effects of intervention were controlled or demonstrated to be absent, or if data for the control group were presented separately.

Studies were excluded if the manuscript did not contain sufficient information to calculate an effect size (e.g., the control group described in Simonelli & Vizziello, 2002) and the authors could not be contacted or did not respond to queries regarding the data. When it was unclear whether results reported on forced (i.e., three-way) or four-way attachment classifications or when parental four-way classifications were used as predictor of child-caregiver forced classifications (Bernier, Matte-Gagné, Bélanger, & Whipple, 2014; Posada, Waters, Crowell, & Lay, 1995), authors were also contacted. One study was excluded because the AAI was conducted jointly with both parents (Crittenden, Partridge, & Claussen, 1991). Two studies were excluded because the SSP was performed without any modifications with 24-month-old children (Domaille, Steele, & Steele, 2013) and 36-month-old children (Manassis, Bradley, Goldberg, Hood, & Swinson, 1994).

As can be seen in Figure 1, over 6,000 studies were identified and screened based on titles and abstracts. Eligibility screening was done by the first author and a research assistant. Of these studies, the 78 studies that were finally included in the meta-analyses (and studies that were excluded because of overlapping data) are presented in Table 2. Many studies contained either data on forced classifications or four-way classifications but not both. Authors were contacted to provide the data of the missing types of classifications. In the end, 68 studies (total N = 4,102) met the inclusion criteria of the meta-analysis of forced classification, 51 studies (total N = 3,439) were included in the four-way meta-analyses, and 17 studies (total N = 1,213) were included in the path-analyses assessing the mediating effect of caregiver sensitivity.

Coding of study variables

A standard coding form that included both study variables and possible moderating variables was used for data extraction. Data extraction was done by MLV. Initially, MLV and MHvIJ coded the data of six studies, after which they discussed the coding of these studies to ensure coding accuracy. Then, MLV coded all other studies. A random set of 15 (15%) studies was then also coded by MO to assess interrater reliability. The intercoder agreement across all categorical variables was on average $\kappa = .79$, based on 6 variables, and inter-coder agreement on continuous variables was on average ICC = 1.00, based on 4 variables.

The assessments methods of both caregiver attachment representation and child-caregiver attachment relationship were extracted from the studies. The associations between caregiver attachment representation and child-caregiver attachment relationship quality were extracted in as much detail as possible and noted either as cross tabulations of intergenerational transmission of four-way, forced, and secure-insecure (based on both four-way and forced classifications), as correlations between continuous scores, or as group means and SDs in the case of a combination of classifications and continuous scores. If studies reported on caregiver sensitivity in the association between caregiver attachment representation and child-caregiver attachment, correlations between attachment representation and sensitivity and between sensitivity and child-caregiver attachment were extracted. Extracted data or associations were uncorrected for third variables (i.e., not adjusted for any covariates).

Additionally, four sample characteristics were examined as substantive moderators of the association between caregiver attachment representation and child-caregiver attachment relationship quality: (a) risk status of the sample, (b) biological versus non-biological caregiver, (c) caregiver gender, and (d) age of the child at attachment assessment. Furthermore, data on two methodological factors that may impact the effect size of intergenerational transmission of attachment were extracted from the papers: (e) study design and (f) training status of the coders. Authors were contacted when information was not reported in the papers. A table containing an overview of study variables, the substantive moderators, and the methodological factors per study can be found in the supplemental materials on the website of the journal.

Data analysis

Cross tabulation of attachment patterns. Echoing Van IJzendoorn (1995), the correspondence between caregiver attachment representations and caregiver-child attachment was examined using two cross tabulations, one for three-way forced classifications and one for four-way classifications. The cross tabulation contained the attachment transmission patterns at the level of the caregiver-child dyad for all of the studies that reported on attachment classifications. Overall correspondence between caregiver attachment representations and caregiver-child attachment was calculated using kappa values. Adjusted standardized residuals > 2.0 and < -2.0 for each transmission pattern indicate whether this pattern is significantly more or significantly less likely to occur than other patterns. The purpose of these cross tabulations is to examine whether the expected patterns of intergenerational transmission of attachment (e.g., autonomous to secure, dismissing to avoidant, preoccupied to resistant, and unresolved to disorganized) are more likely to occur than non-expected patterns.

Calculation of effect sizes. The use of a conventional meta-analytical approach (Borenstein, Hedges, Higgins, & Rothstein, 2009; Hedges & Olkin, 1985; Hedges & Vevea, 1998) was chosen over the use of a psychometric approach to meta-analysis (Hunter & Schmidt, 2004), because our aim was to synthesize the population of effect sizes instead of constructing 'true' effect sizes on the basis of measurement error coefficients for the different study populations (Kepes, McDaniel, Brannick, & Banks, 2013). Furthermore, due to the diverse nature of the studies included in the current meta-analyses, multiple reliability indices exist, such as interrater reliability, test-retest reliability, and internal consistency, and these may vary across populations (e.g., general population or clinical populations). Correcting for all these without knowing the exact amount of error is problematic and may result in less accurate estimates than uncorrected effect size estimates and spurious differences between study populations. Confidence intervals for all point estimates are therefore presented to account for error in the effect size estimates.

Cumulative meta-analyses ordered by year of first publication were performed using the Comprehensive Meta-Analysis (CMA; version 3.2) software (Borenstein, Hedges, Higgins, & Rothstein, 2014). Cumulative meta-analysis provides an updated combined effect size for each study that is added to the literature (Borenstein et al., 2009). This method is used to visualize changes in effect size over time, as was deemed appropriate in this study, because the study aimed to examine a possible decline effect. If the cumulative meta-analysis indicated an effect of publication year, publication year was included in the multivariate analyses.

For the transmission meta-analyses, seven separate cumulative meta-analyses were conducted. The first three meta-analyses focused on the intergenerational transmission of attachment in the forced distributions: (1) autonomous versus non-autonomous (dismissing and preoccupied) AAI classifications were cross tabulated against secure versus insecure (resistant and avoidant) child-caregiver attachment; (2) dismissing versus non-dismissing AAI classifications were cross tabulated against avoidant versus non-avoidant child-caregiver attachment; and (3) preoccupied versus non-preoccupied AAI classifications were cross tabulated against resistant versus non-resistant child-caregiver attachment classifications. The same three analyses were conducted for the studies reporting on four-way classifications, with the addition of a meta-analysis in which unresolved versus non-unresolved AAI classifications were cross tabulated against disorganized versus non-disorganized child-caregiver attachment classifications (4). For these different meta-analyses, several types of data were used to calculate study effect sizes. Most often, χ^2 values or one-tailed Fisher's exact probability values of cross-tabulation data of intergenerational transmission of attachment were calculated using the statistical program Fisher 3.1 (Verbeek & Kroonenberg, 1990) and entered in the CMA program. If $N > 35$, then χ^2 was used in the meta-analysis. If $N < 35$, then one-tailed Fisher exact probability value was used. In the case of continuous data, Pearson's *r* was extracted. In some cases, studies reported on a combination of classifications and continuous data (e.g., AAI classifications and AQS continuous scores). In these cases, means, SDs, and sample sizes per group were directly entered to calculate effect sizes. All data were recalculated into the Fisher's *Z* statistic by the CMA software, which is a transformed correlation coefficient (*r*), because of its superior distribution compared to other statistics such as *r* and Cohen's *d* (Mullen, 1989). To increase readability of the paper, Fisher's *Z* values were transformed back to *r* for reporting.

For each meta-analysis, outlying effect sizes and *N*s were identified based on standardized Fisher's *z*-effect size values. Studies with values larger than 3.29 or smaller than -3.29 were considered outliers; therefore, effect sizes of Solomon & George (2011) were winsorized in four of the analyses, the effect size of Ward and Carlson (1995) was winsorized in the four-way preoccupied-resistant meta-analysis, and the effect size of Ainsworth and Eichberg (1991) was winsorized for the unresolved-disorganized meta-analysis. As the largest sample included in these meta-analyses, Haltigan et al. (2014) had an outlying *N* for all analyses and the *N* was winsorized to the next largest value ($N = 137$)[3]. Weighted combined effect sizes were calculated with the cumulative meta-analysis procedure in CMA. All analyses were performed using random effects models (Borenstein et al., 2009). Because the current meta-analyses included studies that differed on many aspects (e.g., risk status of population, age of the children, caregiver gender), the studies were not expected to reflect one underlying true effect size. We assessed heterogeneity of effect size using *Q* statistics (Borenstein et al., 2009). Additionally, 95% confidence intervals (*CI*s) around the point estimate of each set of effect sizes were calculated.

Publication bias. In addition to retrieving both published and unpublished studies on the intergenerational transmission of attachment and the mediation effect of caregiver sensitivity, the possibility that some data remained in the file drawer was evaluated using three methods: (1) the "trim and fill" method (Duval & Tweedie, 2000); (2) Egger's regression intercept (Egger, Smith, Schneider, & Minder, 1997); and (3) *p*-curve analysis (Simonsohn, Nelson, & Simmons, 2014). The trim-and-fill method uses a funnel plot to calculate the associations between effect size and sample size. In case of an underrepresentation of small effect sizes, the trim and fill procedure imputes studies to balance the funnel plot. The combined effect size is then recalculated to include the imputed studies, reflecting the effect size without publication bias. Egger's regression intercept also uses a funnel plot, but calculates the association between the standardized effect size and the variances of the effect sizes of the different studies. Ideally, these variances should be normally distributed; if publication bias is present, high variances would be associated with large effect sizes. Asymmetry of the funnel plot is assessed with a two-sided significance test. The last method employed to measure publication bias was *p*-curve analysis (Simonsohn et al., 2014). *p*-Curve analysis uses the distribution of statistically significant *p*-values for all studies in a meta-analysis. The method is based on the principle the *p*-values reflect the underlying distribution of the true effect. That means that if an effect truly exists, low significant *p*-values (e.g., $p < .01$) should be more prominent than high significant *p*-values (e.g., $p = .04$). If high significant *p*-values are more prominent than low significant *p*-values, this is likely due to *p*-hacking, the selective reporting of significant results. The presence of right-skew is assessed by means of a *Z*-test of the probability of the *p*-values in the distribution. In addition to conducting these publication bias analyses, the difference between the effect sizes of published and unpublished data was tested. If the difference between published and unpublished data was significant, publication status was included in the multivariate analyses.

Moderator analyses. First, univariate effects of all moderator variables were tested. Effects of categorical moderators (e.g., risk status of the sample) were evaluated in mixed effects models using *Q* statistics for heterogeneity, at $\alpha = .05$ level of significance. This was done with separate estimates of the variance component (τ^2) per subgroup[4], because the true between-studies dispersion was expected to differ between subgroups. Consistent with other meta-analyses (i.e., Bakermans-Kranenburg, van IJzendoorn, & Juffer, 2003), each level of the moderator variable needed to contain at least four studies to be included in moderator analyses; levels containing fewer than four studies were excluded. To test the continuous moderator children's age at attachment assessment, meta-regression analyses of effect size on the continuous moderator were performed. Moderators were considered significant if the slope of the regression (*b*) differed from zero at the $p = .05$ level of significance.

Second, correlations between the substantive moderators (e.g., risk status and caregiver gender) and the methodological factors were calculated to test if the substantive characteristics were associated with the study characteristics (i.e. if studies in risk samples pose restrictions on the study design). If the correlation was significant at $p >.10$ level, the methodological factor was included in the analyses to control for the confounding effect of this factor.

Finally, meta-regression analyses were used to compare the differences in the amount of explained variance in effect size (ΔR^2) between three nested models. The full model contained publication year[5] and publication status in addition to the substantive moderators. The first reduced model contained publication status and the substantive moderators, but not publication year. The second reduced model contained only the substantive moderators. Including publication year and publication status in the full model enabled testing for a possible "decline effect" (e.g., Ioannidis, 2005) of the effect sizes over the years and for effects of possible publication bias while taking into account the substantive moderators that might be associated with publication status and publication year (e.g., more recent studies being conducted more often in clinical groups). Using an *F*-test, ΔR^2 of the reduced models and the full model was tested. When the full model explained significantly more variance in effect size than the reduced models, the full model was retained as the best prediction model of effect size. When ΔR^2 was not significant, the reduced model was adopted. Significant moderators of effect size in the final model are reported. Using *Q* statistics, it was assessed whether moderators explained the heterogeneity in effect sizes.

Path analysis on mediation effect of caregiver sensitivity. In order to perform the path-analysis to assess the mediating role of caregiver sensitivity in the intergenerational transmission of attachment, three separate meta-analyses were conducted to obtain effect size estimates for each of the pathways: one on the association between caregiver attachment representation and child-caregiver attachment in the studies included in the path model, one on the association between caregiver attachment representation and caregiver sensitivity, and one on the association between caregiver sensitivity and child-caregiver attachment. In all three meta-analyses, the correlations between the two variables reflecting the pathway were used as effect sizes. If the correlations between these variables were missing from the paper, means, SDs, and sample sizes per group were entered to calculate the effect size. For papers that did not report the correlation for the pathway between caregiver attachment representations and child-caregiver attachment, the effect size used in the transmission meta-analysis was inserted in the meta-analysis to obtain the coefficient for this pathway. If the paper did not present enough information to calculate an effect size, authors were contacted for additional information.

All effect size data were recalculated into the Fisher's *Z* statistic using the CMA software. For each of the pathways in the path model, weighted combined effect sizes were calculated using random effects models (Borenstein et al., 2009). Fisher's *Z* values were then transformed back to *r* for reporting.

To investigate the mediating role of caregiver sensitivity in the intergenerational transmission of attachment and to test whether the transmission gap still existed, the meta-analytically derived correlations of each pathway were used to calculate the amount of variance in the association between caregiver attachment representations and child-caregiver attachment that could be explained by caregiver sensitivity. Based on the multiplication rule of Wright (1934) used to discover the transmission gap (Van IJzendoorn, 1995), the indirect effect of caregiver attachment representation on caregiver-child attachment through caregiver sensitivity was calculated by multiplying the coefficients of the pathways from caregiver attachment representation to caregiver sensitivity and from caregiver sensitivity to caregiver-child attachment. This indirect effect was then extracted from the total effect of caregiver attachment representation on caregiver-child attachment to obtain the amount of unexplained variance, which reflects the transmission gap (e.g., Madigan et al., 2006; Van IJzendoorn, 1995).

To test the alternative hypothesis that measurement error accounts for the transmission gap, two methods for disattenuation of measurement error were employed. First, all correlations were corrected for attenuation by taking into account the interrater reliability of all assessments (Charles, 2005). Weighted average values of ICCs and kappa of all measures in the path model were computed, which were used to recalculate the coefficients in the path model. The second method used the test-retest reliability coefficients derived from other studies to correct for attenuation. These test-retest reliability coefficients were $\kappa = .63$ for caregiver attachment representations (Bakermans-Kranenburg & Van IJzendoorn, 1993), $r = .36$ for child-caregiver attachment (Pinquart, Feußner, & Ahnert, 2012), and $r = .49$ for caregiver sensitivity (based on multiple assessments with the Maternal Behavior Q-Sort; Lindhiem, Bernard, & Dozier, 2011).

Results

Study variables

Sample size ranged from 12 to 203 (winsorized in analyses to 137), with a median sample size of 42. Ninety-one (96%) studies used AAI classifications; of the remaining four studies, two (2%) used continuous coherence scores and two (2%) used Kobak's Q-sort continuous score (Kobak, 1993). Seventy-three (77%) studies measured child-caregiver attachment with the SSP, twelve (13%) used the AQS, two used a modified SSP, four (4%) used the Main and Cassidy attachment classification system, and four (4%) used the Preschool Attachment Assessment. The average age of children at the moment of the child-caregiver attachment assessment was 20.98 months (*SD* = 15.53, range = 12-74 months). Twenty-three (24%) and 69 (73%) studies assessed caregiver attachment representations prenatally (third trimester of pregnancy) and postnatally (*M* child age = 27.72 months, *SD* = 26.84, range 0.5-138 months), respectively. Forty-two samples (44%) originated from North America, 44 (46%) were from Europe, and nine (9%) from non-Western countries. Sixteen samples (17%) were considered to have low SES, 34 (36%) were at-risk , 13 (14%) derived from clinical samples. Eight (8%) studies focused exclusively on fathers, six (6%) on non-biological caregivers.

Intergenerational transmission of attachment: Forced classifications

Three-way forced classifications of caregiver attachment representation and child attachment were cross tabulated for 2,666 child-caregiver dyads (Table 4). Correspondence for the entire cross tabulation was 58% ($\kappa = .28$, $p < .001$). Adjusted standardized residuals revealed that caregivers with an autonomous representation were significantly more likely to have secure attachment relationships with their children and less likely to have either avoidant or resistant attachment relationships with their children. Caregivers with dismissing representations were more likely to have avoidant attachment relationships and less likely to have secure attachment relationships; however, they were not significantly less likely than caregivers with autonomous or preoccupied representations to be in a resistant attachment relationship. Similarly, preoccupied representations were associated with more resistant and fewer secure attachments, but not with fewer avoidant attachments.

Autonomous to secure intergenerational transmission[6]. The cumulative meta-analysis of the autonomous versus non-autonomous classifications (k = 83, N = 4,102) yielded a significant combined effect size of r = .31 (95% CI [.26, .37]), showing a decrease over time (See Table 5). Thus, caregivers with an autonomous representation were more likely to develop secure attachment relationships with their children than caregivers with non-autonomous representations. There was a significant difference in effect size between published data (k = 40, r = .38, 95% CI [.31, .44]) and unpublished data (k = 43, r = .25, 95% CI [.18, .32]), even though analyses of publication bias in the subset of published data showed no publication bias. Published samples were on average smaller than unpublished samples (resp. mean N = 44.0 vs mean N = 54.5). The full set of studies contained evidence for publication bias: Egger's regression intercept was significant, (one-tailed p = .01) and trim-and-fill statistics showed that 11 studies needed to be trimmed and filled, leading to an adjusted effect size of r = .24 (95% CI [.21, .27]). p-Curve analysis did not indicate p-hacking in the either full set of studies or the set of published studies (both $p < .001$).

There was significant between-study variability in effect sizes (Q = 254.64, $p < .001$)(see Table 6). The univariate moderator analyses showed that effect size was larger for samples considered not at risk (k = 54, r = .38, 95% CI [.32, .44]) than for samples considered at risk (k = 29, r = .18, 95% CI [.10, .26]). Significant intergenerational transmission was found in studies with biological caregivers (k = 79, r = .32, 95% CI [.27, .37]) but not in studies with non-biological caregivers (k = 4, r = .14, 95% CI [-.01, .29).

Of the methodological factors, study design was significantly associated with age at attachment assessment (r = -.54, $p < .001$) and thus retained in the analyses; training status of the coders was not associated with any of the substantive moderators ($p > .35$). Comparisons of the nested models indicated that the full model including publication year[5] and publication status in addition to the moderators did explain significantly more variance (R^2 = .36) than the first reduced model including publication status and the significant moderators (ΔR^2 = .06, *F* (1,72) = 6.75, *p* = .01), therefore, the full model was retained as the best fitting model. Risk status of the sample was a significant independent moderator of effect size were (*b* = -0.17, 95% CI [-.31, -.07], *p* = .002). Publication year[3] was a marginally significant predictor of effect size (*b* = -0.01, 95% CI [-.01, .00], *p* = .06). Effect sizes remained heterogeneous (*Q* = 167.86, *p* < .001).

Intergenerational transmission of attachment: Four-way classifications

The second set of meta-analyses concerned the transmission of caregiver attachment representations to the infant-caregiver attachment relationship using the four-way classification schemes. The cross tabulation of the four-way classifications is presented in Table 7. Correspondence for the four-way cross tabulation was 51% (κ = .26, *N* = 2,774, *p* < .001). Patterns were in line with the cross tabulation of the forced classifications, with the addition that caregivers with unresolved representations were more likely to have disorganized attachment relationships with their children and less likely to have both secure and avoidant attachment relationships, but not less likely to have resistant attachment relationships.

Autonomous to secure intergenerational transmission[6]. The cumulative meta-analysis of the autonomous versus non-autonomous classifications (*k* = 59, *N* = 3,226) yielded a significant combined effect size of *r* = .31 (95% CI [.25, .37]) and the effect size decreased over time (Table 8). A significant difference in effect size was apparent between published data (*k* = 32, *r* = .40, 95% CI [.33, .47]) and unpublished data (*k* = 27, *r* = .21, 95% CI [.11, .30]), although publication bias indicators did not suggest this in the subset of published data. Average sample size was larger for unpublished samples than for published samples (resp. mean *N* = 59.8 vs mean *N* = 50.4). Evidence of publication bias was present in the full set of studies: trim-and-fill statistics showed that 10 studies needed to be trimmed and filled, leading to an adjusted effect size of r = .24 (95% CI [.20, 27]). Egger's regression intercept was not significant (one-tailed p = .07). p-Curve analysis gave no indication of p-hacking in both the full set of studies and the published studies (both $p < .001$).

Significant heterogeneity was found (Q = 205.80, $p < .001$). Moderator analyses are displayed in Table 9. Studies in not at risk samples had larger effect sizes (k = 37, r = .37, 95% CI [.30, .44]) than studies in at risk samples (k = 22, r = .21, 95% CI [.10, .32]).

Correlations between the substantive moderators and the methodological factors showed that only study design was associated with age at attachment assessment (r = -.49, $p < .001$); training status of the coders was excluded from the analyses ($p > .15$). Comparison of the nested models showed that the full model including publication year and publication status did not explain significantly more variance (R^2 = .35) than the first reduced model including publication status and the significant moderators (ΔR^2 = .04, F (1,48) = 2.95, p = .09). The first reduced model did explain more variance (R^2 = .31) than the second reduced model including only the substantive moderators (ΔR^2 = .15, (F (1,47) = 18.85, $p < .001$), therefore, the first reduced model was retained as the best fitting model. Publication status was the only significant moderator of effect size in this model (b = 0.20, 95% CI [.05, .34], p = .01), although risk status was a marginally significant predictor (b = -0.13, 95% CI [-.28, .02], p = .09). The remaining variance between effect sizes remained heterogeneous (Q = 135.30, $p < .001$).

Unresolved to disorganized intergenerational transmission. The meta-analysis of the unresolved versus non-unresolved classifications (k = 47, N = 2,945) yielded a significant combined effect size of r = .21 (95% CI [.16, .26]), which decreased with time (See Table 10). Effect sizes were larger for published data (k = 25, r = 0.28, 95% CI [.20, .35]) than for unpublished data (k = 22, r = .14, 95% CI [.09, .19]), even though publication bias was not indicated by funnel plot, Egger's test, and trim-and-fill method. Published samples were on average smaller than unpublished samples (resp. mean *N* = 56.7 vs mean *N* = 69.5). There was no indication of *p*-hacking ($p < .001$).

Significant heterogeneity was found in the effect sizes of the different studies (*Q* = 80.99, *p* = .001)(see Table 11). Univariate moderator analyses indicated that effect sizes increased with children's age at attachment assessment (*k* = 47, *b* = 0.004, *p* = .047).

Of the methodological factors, only study design was associated with age at attachment assessment (*r* = -.58, $p < .001$); training status of the coders was excluded from further analyses ($p > .20$). Comparison of the nested models revealed that the full model did not explain more variance ($R^2 = .30$) than the first reduced model ($\Delta R^2 = .03$, *F* (1,40) = -1.71, *p* = .20). The proportion of explained variance was larger for the first reduced model (R^2 = .33) than for the second reduced model including only the substantive moderators (ΔR^2 = .15, (*F* (1,41) = 11.01, $p < .01$) and the first reduced model was retained. Publication status was a significant moderator of effect size (*b* = 0.11, 95% CI [.001, .21], *p* = .04), as well as age at caregiver-child attachment assessment (*b* = 0.005, 95% CI [.000, .01], *p* = .04). Effect sizes remained heterogeneous (*Q* = 59.12, *p* = .03).

Intergenerational transmission of attachment: Mediation by caregiver sensitivity

Meta-analytically derived correlation coefficients between caregiver attachment representation and child-caregiver attachment, between caregiver attachment representation and caregiver sensitivity, and between caregiver sensitivity and child-caregiver attachment are presented in Table 12. Figure 2 shows the results of the path analysis of the mediating effect of caregiver sensitivity in the association between caregiver attachment representations and child-caregiver attachment. The proportion of the association between caregiver attachment representation and child-caregiver attachment explained by caregiver sensitivity equaled (.20*.35) = .07, which is approximately 25% of the raw correlation *r* = .25 (c in Figure 2). The unexplained association remained *r* = .18 (c' in Figure 2), thus leaving a transmission gap between caregiver attachment representation and child-caregiver attachment ($p < .001$). Testing this transmission model with normative samples only (k = 12), the transmission gap remained $r = .20$ ($p < .001$).

The correlations corrected for attenuation based on interrater reliability and test-retest reliability are shown in parentheses in Figure 2. The transmission gap after correction for interrater reliability was $r = .23$, which is larger than before the corrections, because the correlation between caregiver attachment representations and child-caregiver attachment was most attenuated due to higher measurement error in these variables compared to caregiver sensitivity. The transmission gap after correction for test-retest reliability remained $r = .22$ ($p < .001$), which left less than half of the association unexplained.

In an attempt to explore the change in the transmission gap over the years, the path analyses for mediation were repeated with a median split for publication year (median = 2006). Although the path coefficients did not differ significantly between the two models (Figure 3), the coefficient for the transmission gap (c') was significantly smaller for newer studies than for older studies ($z = -2.31$, $p = .01$).

Discussion

The addition of attachment representations as proposed by Main and colleagues (1985) has elucidated intergenerational transmission of attachment across a large number of populations. However, the phenomenon has turned out as less strong than suggested by the earlier review of the evidence in 1995 (Van IJzendoorn, 1995), indicative of a decline effect. Furthermore, weaker transmission was found in unpublished studies compared to published studies. Our comprehensive synthesis of available data revealed that the intergenerational transmission of attachment varied in strength depending on the presence of other psychosocial risks in the population and depending on the age of the children at which intergenerational transmission is assessed. The weaker transmission found in unpublished studies, as well as those published more recently, could not be fully explained by increasing diversity in sample characteristics, suggesting that there are additional unknown moderators of intergenerational transmission. The existence of the transmission gap was confirmed, making it unlikely that it was an artifact in the 1995 meta-analysis (Van IJzendoorn, 1995).

Specificity of intergenerational transmission of attachment

The results of the cross tabulations of the attachment patterns indicate that autonomous to secure transmission is more likely than autonomous to insecure transmission, whereas the specificity in transmission of non-autonomous representations is less pronounced. Although the expected transmission patterns also occur more often for the non-autonomous representations than the non-expected patterns, the cross-over of patterns (e.g., dismissing to resistant and preoccupied to avoidant) does not occur significantly less often than expected based on chance. This finding of lowered specificity for non-autonomous attachment representations was, although slightly less pronounced, already found by Van IJzendoorn in 1995. The issue of specificity of transmission also relates to the discussion whether individual differences in attachment representations and relationships are best conceptualized and operationalized as categorical or dimensional differences (Haydon, Roisman, Owen, Booth-LaForce, & Cox, 2014; Roisman, Fraley, & Belsky, 2007; van IJzendoorn & Bakermans-Kranenburg, 2014). Resolution of these issues requires that authors start to report transmission effects both using categories as well as dimensional scores.

Explaining the heterogeneity of effect sizes: moderators of intergenerational transmission

Effect sizes in all meta-analyses were heterogeneous and several substantive moderators emerged to explain between-study variability: risk status, non-biological caregiver dyads, and age of the children at attachment assessment. Each of these moderators will be discussed in turn. Risk status was a sample characteristic that negatively affected the effect sizes in both forced and four-way autonomous transmission. As in other studies (Atkinson et al., 2000; Bakermans-Kranenburg & van IJzendoorn, 2009), non-autonomous representations were more prevalent in adults (forced 56%, four-way 62%) and insecure attachment relationships more in children (forced 45%, four-way 54%) in the at-risk samples compared to the normative samples (adults: 43% and 46%; children: 38% and 44%). There could be three possible explanations for the inhibition of intergenerational transmission in samples at risk: lower transmission of security, lower transmission of insecurity, and lower transmission of both security and insecurity. Comparing the transmission rates in our data led to mixed results, with lower transmission rates for forced insecurity (51%) compared to security (63%) and slightly lower transmission rates for four-way security (58%) compared to insecurity (62%). However, the general picture is that caregiver non-autonomous representations were more prevalent than child-caregiver insecure attachment, as is the case in the general population, which indicates that some non-autonomous caregivers manage to build secure attachment relationships with their children regardless of their own non-autonomous representations, or that some children might be more resilient against negative influences from the environment (Bakermans-Kranenburg & van IJzendoorn, 2015; Belsky, 1997). This result does not support the hypothesis that stress factors brought on by the risk status cause more insecure attachment (Cyr et al., 2010). Perhaps this can be explained by protective factors, such as having a partner with an autonomous representation. Mismatches in attachment representations do occur in partner selection (Owens et al., 1995) and there is only modest concordance in attachment relationships of both partners with their children (Fox, Kimmerly, & Schafer, 1991; van IJzendoorn & De Wolff, 1997). Thus, attachment representations of one caregiver are not entirely deterministic of the quality of the child-caregiver attachment relationship. Future studies should focus on the identification of underlying mechanisms that may explain discontinuity in the intergenerational cycle of insecurity; these factors may be found in the caregiver, in the child, or in the environment. The identification of these mechanisms is essential for identifying targets in intervention efforts.

Effect sizes were substantially lower, even showing a lack of forced autonomous transmission, in samples with non-biological child-caregiver dyads than in biologically related samples. This effect was only found in the univariate analyses, because in the multivariate analysis it was overshadowed by the effects of risk status. Caution is warranted in interpreting this result, as the absence of intergenerational transmission is based on only four studies with non-biological child-caregiver dyads (total N =168), of which two studies reflected late placement of the children in their new families (Nowacki, Bovenschen, Spangler, & Roland, 2010; Pace & Zavattini, 2011), which is associated with difficulty in developing secure attachment (Milan & Pinderhughes, 2000). Due to the lack of evidence regarding heredity of attachment in early childhood (e.g., Bakermans-Kranenburg & van IJzendoorn, 2004; Bokhorst et al., 2003; Roisman & Fraley, 2008), it seems unlikely that the lower attachment transmission rates are explained by the absence of biological relatedness of the dyads (but see Fearon, Shmueli-Goetz, Viding, Fonagy, & Plomin, 2014 for significant heritability of attachment representations in adolescence). Future research should examine the intergenerational transmission of attachment in non-biologically related samples more closely to identify circumstances in which transmission does and does not take place.

Another sample characteristic that moderated effect sizes was gender of the parent. Mothers' effect sizes for forced autonomous transmission were considerably larger than fathers' effect sizes (r = .55 vs r = .37) two decades ago (Van IJzendoorn, 1995), whereas mothers' effect sizes in the current study were similar (r = .31) to fathers' effect sizes (r = .33). The lack of significant differences in effect sizes between mothers and fathers appears to be the result of a decrease over time in effect size for mothers, rather than change in the effect size for fathers. A meta-analysis on the association between father sensitivity and father-child attachment showed no change in the association over time (Lucassen et al., 2011), which seems consistent with our results on fathers. However, our path analyses comparing older and newer studies indicated that the effect size of the association between sensitivity and child-caregiver attachment did not change over time for the whole set of studies, not just for paternal samples. As this meta-analysis consisted largely of maternal samples, the cause of a lower effect size for transmission in mothers can hardly be found in the association between sensitivity and attachment. Perhaps the decreasing effect size in women is due to secular changes in the role of women within family life over time and the gradually more equal division of care tasks between partners, but that hypothesis remains to be investigated in future studies.

Age of children at the child-caregiver attachment assessment was positively associated with effect size in several of the meta-analyses, contradicting Van IJzendoorn’s finding of smaller effect sizes in studies with older children (1995). Our finding is in line with the increasing strength of the association between sensitivity and attachment with age found by De Wolff and Van IJzendoorn (1997). This positive association between attachment representations and caregiver-child attachment is consistent with the attachment theoretical tenet that quality of attachment relationships is gradually formed by the history of dyadic interactions. Because the content of these interactions change and become more differentiated over the course of children’s development, it will be important to investigate whether intergenerational transmission is the result of quality of interactions that center around one particular domain (e.g., sensitive responsiveness) or an increasing number of related domains (e.g., scaffolding). It should be noted that attachment in older children was measured with different measures (e.g., the Main-Cassidy classification system) than in younger children (e.g. the Strange Situation procedure). It may be that the effects of age are confounded with the effects of measure used to assess attachment and this should be examined more closely in the future. Also, it should be taken into account that the effects of age were found with study aggregate values for age instead of individual participant data. As there might be considerable within-sample variation in this characteristic, using individual participant data may have shown slightly different results than the results brought about by this traditional meta-analysis (Stewart & Tierney, 2002). For all the reasons mentioned above, this finding should be interpreted with caution and future research is needed to more fully understand it.

Publication bias and the decline effect: A base for winner's curse

Effect sizes for unpublished data were smaller than effect sizes for published data, which confirms publication bias. However, it is important to note that in most of our analyses, our statistical bias indicators (e.g., trim and fill) did not detect the presence of publication bias, not even when only published data were included in the publication bias analyses. This shows that publication bias effects may not always be found by the statistical indicators intended to reveal this bias in meta-analyses. Therefore, it is vital to include unpublished work in meta-analyses to most accurately reflect the true effect sizes of phenomena. However, even the best efforts of including unpublished work in meta-analyses will not guarantee a true reflection of the field, as in some domains 65% of the studies finding null results may never even be written up (Franco, Malhotra, & Simonovits, 2014). Referring study results to the file drawer instead of journals is problematic, because it has been shown that publishing of all study results provides a more accurate estimation of population effects than selective publishing of significant results (van Assen, van Aert, Nuijten, & Wicherts, 2014).

The cumulative meta-analyses supported a gradually declining effect size over time. The “decline effect” (e.g., Ioannidis, 2005; Schooler, 2011) may explain at least part of the decrease in effect size. This effect is found often, because studies that introduce a certain idea are often small and therefore more likely to find exaggerated effects, whereas replication studies tend to report on larger and more diverse samples. The overestimation of an effect based on incomplete information, due to publication bias and selective reporting of early studies before the decline set in, reflects the winner's curse (Young, Ioannidis, & Al-Ubaydli, 2008). This common phenomenon in the field of the social sciences (for a drastic example, see Molendijk et al., 2012) may explain the overestimation of the effect size during the first decade of studies of intergenerational transmission of attachment (Van IJzendoorn, 1995). However, it should be kept in mind that more recently, it became more common to study intergenerational transmission in samples with lower transmission rates, such as at risk samples and non-biologically related samples. This could also explain part of the decline effect. Nonetheless, the multivariate analyses have shown that the decline could not be fully explained by these sample characteristics. Other explanations thus remain to be investigated. In pursuing explanations for the declines in intergenerational transmission seen in research from the last two decades, researchers may look into technical aspects that have not been measured in this meta-analysis, such as intercoder reliability or collective coder drift from the coding criteria as originally devised. Importantly, the decrease in effect size found in the current study was found with publication year of the first publication ever to present the data as a proxy of the time frame in which the data collection had taken place. Although this is the closest approximation of the data collection period that we could use, it may not always match the data collection period perfectly.

It should be noted that publication status and publication year were moderately associated ($r \approx -.30$), with newer studies being more likely to be unpublished. This association can be explained in two ways. First, new studies might still be in the process of getting published. Second, if an effect has been found in many prior studies, studies confirming the result may be less likely to be published due to the law of diminishing returns, whereas studies with non-significant results may be met with caution by the academic community, which can in turn lead to the file-drawer effect. It is important to note that simultaneous analysis of both the effects of publication status and publication year may have obscured some of the effects (e.g., the effect of publication year might have been invisible due to the shared variance with publication status), but this can only have led to more conservative treatment of the data, as the effect of publication year was always examined as an addition to the nested model containing publication status and the moderating study and sample characteristics.

The smaller effect sizes of the current meta-analyses compared to the effect sizes in the Van IJzendoorn (1995) meta-analysis two decades ago have significant implications for research on attachment theory. Attachment research is characterized by the use of labor-intensive measures, requiring thorough coder training and ample coding time. As a practical consequence of laborious methodology, most studies have small samples, with the largest sample to date including 203 participants (Haltigan et al., 2014). With the current effect sizes, substantial samples are required to show the intergenerational transmission effects. For example, to be able to show secure/insecure transmission (r = .31) with a power of .80, samples should consist of roughly eighty child-caregiver dyads. To investigate transmission of unresolved representations (r = .21), an even larger sample of 180 child-caregiver dyads is required. Moreover, still larger samples are required when researchers want to look at transmission of the full range of attachment classifications instead of an isolated examination of autonomous/non-autonomous and/or unresolved/not unresolved representations contrasts, or when studying at risk samples. These results indicate that of the 97 samples included in this meta-analysis, 80 samples were underpowered for secure/insecure transmission, whereas only one sample (Haltigan et al., 2014) had enough power to examine the transmission of unresolved representations.

Perhaps even more important are the considerations for attachment theory implied by these lower effect sizes. Two decades ago, the meta-analysis by Van IJzendoorn (1995) showed an effect size for autonomous transmission of r = .48, meaning that almost 25% of the variance in child attachment could be explained by attachment representations of the caregiver. However, the current effect size of r = .31 shows that closer to 10% of the variance in child attachment security can be explained by autonomous representations of attachment figures. This means that other antecedents of child attachment are likely to be of great importance, which should create fresh impetus to efforts to determine the causal influence on infant attachment security beyond those captured by the AAI. The diminishing returns of studies on intergenerational transmission thus raise the bar for new study questions and hypotheses related to the role of caregivers' mental representations of attachment.

Revisiting the transmission gap

Taking into account the lower effect size for the association between attachment representations and attachment relationships, a smaller, albeit significant, transmission gap was found than in the previous meta-analysis (Van IJzendoorn, 1995). This transmission gap could not be explained by attenuation of associations due to measurement unreliability. These results support the idea that the transmission gap is not a spurious finding and that alternatives to caregiver sensitivity in explaining the gap need to be pursued. Researchers have focused on many other mediating mechanisms during the past decades, such as family functioning and the quality of the couple relationship (Cowan & Cowan, 2009; Das Eiden, Teti, & Corns, 1995; Dickstein, Seifer, & Albus, 2009) and more cognitive constructs (i.e., mind-mindedness; Arnott & Meins, 2007; Bernier & Dozier, 2003; and reflecting functioning; Slade, Grienenberger, Bernbach, Levy, & Locker, 2005). Even though these studies rarely found full mediation of the association between attachment representations and child-caregiver attachment, most studies identified these constructs as partial mediators, indicating that intergenerational transmission may depend on multiple pathways besides caregiver sensitivity and on multiple levels besides the behavioral level (e.g., the cognitive level). On top of these possible mediators, ecological factors were found to moderate the mediating effects of caregiver sensitivity (Tarabulsy et al., 2005), supporting a more comprehensive view on intergenerational transmission of attachment as well. Recently, Bernier et al. (2014) found full mediation in a model simultaneously examining the mediating role of caregiver sensitivity and autonomy support, a construct measuring parental behavior in times of exploration. Following this trend of more integrative research, integrative models of the mechanisms behind attachment transmission should be studied, including ecological factors, family factors, and even biological or genetic indicators. These models might also consider the differential susceptibility that children display with respect to their rearing environments (e.g., Bakermans-Kranenburg & van IJzendoorn, 2011, 2015). The examination of these integrative models may take place within large study designs, but also by use of meta-analytic structural equation modeling (MA-SEM; Beretvas & Furlow, 2006; Cheung, 2008, 2013). MA-SEM is an emerging technique that combines meta-analysis and structural equation modeling to synthesize findings from different studies to examine complex models. An advantage of this approach is that data from studies examining parts of the model can be combined, thus enabling optimal use of the data and decreasing the need for complex study designs.

Explanatory factors for the intergenerational transmission of autonomous to secure attachment have received most attention in research, most prominently maternal sensitivity. Additionally, atypical and frightening maternal behavior has been studied as the driving force behind the transmission of unresolved representations to disorganized attachment (Lyons-Ruth, Bronfman, & Parsons, 1999; Madigan et al., 2006; Main & Hesse, 1990). However, the mechanisms behind the transmission of organized insecure attachment categories have received much less attention. The exception to this rule is a study that showed that mothers with a more dismissing representation were less sensitive to their children, whereas mothers with a more preoccupied representation provided less autonomy support to their children than mothers without high scores on these dimensions (Whipple et al., 2011). These findings support the assumptions underlying the Strange Situation Procedure with respect to the parental behaviors that cause avoidant and resistant attachment. As an extension to this study, examining whether a combination of these behavioral patterns and attachment representations are associated with avoidant and resistant attachment would be useful. Future studies should also investigate whether caregivers following behavioral patterns more congruent with their attachment representations more often show intergenerational transmission than caregivers who are less inclined to display these stereotypical behavioral patterns. Gaining insight into the mechanisms behind the intergenerational transmission of these insecure attachment patterns besides the transmission mechanisms of attachment security could enhance opportunities of intervening with these negative behaviors.

Because the existing model of intergenerational transmission of attachment via caregiver sensitivity cannot account for all findings of the current meta-analysis, we propose a revised framework (Figure 4). The model shows that attachment representations lead to individual differences in the quality of attachment relationships through the quality of dyadic interactions, consistent with theory (Bowlby, 1969/1982; Main et al., 1985; Van IJzendoorn, 1995) and the current meta-analytical evidence. Also in line with our expectations, the path analyses showed that caregiver sensitivity can only partially account for this transmission, leaving room for other possible mediating mechanisms in the child-caregiver interaction, such as pathways involving social-cognitive constructs (Fonagy & Target, 2005; Meins, 1999) and constructs pertaining to autonomy support (Bernier et al., 2014). The most important additions to the existing model are the effects of the context in which the interaction takes place, because our results showed that several sample characteristics, such as risk status, predicted the effect size of the intergenerational transmission of attachment. In future studies, more contextual factors could be tested, such as family functioning, the couple relationship, and support. Also, research on neural mechanisms behind attachment and parenting should remain a focus of research. Likewise, the effects of differential susceptibility to child-caregiver interaction for children differing in genetic make-up or temperamental characteristics on the intergenerational transmission of attachment should be examined, because studies have shown differential effects of rearing environments on children (e.g., Bakermans-Kranenburg & van IJzendoorn, 2011; Belsky, 1997).

In summary, the association between caregiver attachment representations and child-caregiver attachment has been confirmed as a robust and universal effect by this new series of meta-analyses, albeit smaller than in the initial studies. The current study extended the findings of Van IJzendoorn (1995) by assessing a variety of sample and study characteristics as potential moderators of the effect size of the intergenerational transmission of attachment. Several sample characteristics explained a proportion of the heterogeneity in effect sizes, but the remaining variability in effect sizes is still heterogeneous, even after examining the combined effects of all moderators in nested models including publication status and publication year. Publication status and publication year were consistent predictors of effect sizes, with smaller effect sizes for unpublished and newer studies, but explanations for the decreasing effect size remain to be investigated. These findings underscore the importance of considering the winner's curse and the decline effect for evaluating the status of theories in psychological science. Lastly, the attachment theoretical account of intergenerational transmission needs to be revised in order to accommodate not only the additional mediating pathways, but also the multiple conditions that determine whether transmission occurs.

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Notes.

[1] The third method of study identification, the search of dissertation databases and unpublished works, included studies available until January 2014.

[2] With the exception of Pace and Zavattini (2011), because this study measured child-caregiver attachment in a late-adoption sample at forty days after placement and six months after placement. Only the second measurement of child-caregiver attachment was included in the meta-analysis.

[3] Sensitivity analyses with both the full sample size of the Haltigan study and the winsorized sample size were performed. The effect sizes differed negligibly (differences of r = .005), and the results of the current study were not materially affected by these differences.”

[4] Data on the estimates of the variance component (τ^2) per subgroup are available upon request from the authors.

[5] As data collection and coding in attachment research are time-consuming tasks and data are often reported on in multiple papers, publication year of the first study containing the data was tested as a moderator variable, because this was the closest approximation of the time frame in which the data collection had taken place.

[6] Due to space limitations, the meta-analyses of the dismissing and preoccupied representations can be found in the supplemental materials provided on the website of the journal.

[7] As a result of only including studies that measured caregiver sensitivity as well as attachment representations and caregiver-child attachment, these meta-analytically derived path coefficients may not generalize to studies measuring single pathways.

Table 1. Parallel classifications of caregiver attachment representations and caregiver-child attachment

Attachment representations	Caregiver-child attachment
Secure-Autonomous	Secure
Dismissing	Avoidant
Preoccupied	Resistant
Only in Four-way distribution[a]:	
Unresolved	Disorganized

[a] In three-way forced classifications, individuals with Unresolved attachment representations and Disorganized caregiver-child attachment classifications are forced into their most likely organized category of attachment.

Table 2

Studies included in meta-analyses and studies excluded due to overlapping data[a]

Study name	Publication year	*N*	Measures	Forced, four-way, both?	Sensitivity studied?
Ainsworth & Eichberg (1991)	1991	45	AAI, SSP	Both	No
Eichberg (1987)	1987	45			
Ammaniti & Speranza (1995)	1995	12	AAI, SSP	Both	No
Ammaniti et al. (1996)	1996	20			
Ammaniti et al. (2005; clinical)	2005	19	AAI, SSP	Both	No
Ammaniti et al.(2005; control)	2005	23			
Arnott & Meins (2007; mothers)	2007	18	AAI, SSP	Both	No
Arnott & Meins (2007; fathers)	2007	15	AAI, SSP	Both	No
Arnott (2006)	2006	18			
Aux (2000)	2000	53	AAI, AQS	Forced	No
Bailey et al. (2007)	2007	98	AAI, SSP, AQS, MBQS	Both	Yes
Madigan et al. (2006)	2006	82			
Madigan et al. (2007)	2007	64			
Evans (2008)	2008	50			
Gleason (2001)	2001	50			
Behrens et al. (2007)	2007	43/41	AAI, SRP	Both	No
Behrens (2005)	2005	42			
Behrens et al. (in prep)	in prep[b]	66	AAI, SSP	Both	No
Benoit & Parker (1994; mothers)	1994	85/88	AAI, SSP	Both	No
Madigan et al. (2011; fathers)	2011	31	AAI, SSP	Both	No
Madigan, 2011 (mothers)	2011	31			
Pederson et al. (1998)	1998	60	AAI, SSP, MBQS	Both	Yes
Gleason (1995)	1995	44			
Raval et al. (2001)	2001	96	AAI, SSP, divided attention task	Both	Yes
Goldberg et al. (2003)	2003	197			
Crawford & Benoit (2009)	2009	35			
Atkinson et al. (2005)	2005	112			
Atkinson et al. (2009)	2009	102			
Jamieson (2004)	2004	179			
Bernier & Dozier (2003)	2003	64	AAI, SSP	Four-way	No
Dozier et al. (2001)	2001	50			
Stovall & Dozier (2000)	2000	10			
Stovall-McClough & Dozier (2004)	2004	20			
Bernier et al. (2014)	2014	130	AAI, AQS, MBQS	Forced	Yes
Berthelot et al. (2015)	2015	57	AAI, SSP	Both	No
Brisch et al. (2005)	2005[c]	66	AAI, SSP	Both	No
Bus & Van IJzendoorn (1992)	1992	32	AAI, SRT revised	Forced	No
Cassibba et al. (2004; asthma)	2004	30	AAI, AQS	Forced	No
Cassibba et al. (2004; control)	2004	30	AAI, AQS	Forced	No
Cassibba et al. (2011; clinical)	2011	20	AAI, SSP, EAS	Both	Yes
Cassibba et al. (2011; control)	2011	20	AAI, SSP, EAS	Both	Yes
Chin (2013)	2013	104	AAI, AQS, MBQS	Forced	Yes
Coppola et al. (2010)	2010[d]	22	AAI, SSP	Forced	Yes
Coppola et al. (2014)	2014	40	AAI, AQS	Forced	No
Costantini (2006; control)	2006	20	AAI, SSP, EAS	Both	Yes
Costantini (2006; preterm)	2006	20	AAI, SSP, EAS	Both	Yes
Costantino (2007; control)	2007	19		Forced	No
Costantino (2007; social disadvantage)	2007	25		Forced	No
Dedrick (1993)	1993	73	AAI, AQS, Ainsworth scales	Forced	No
DeKlyen (1996; clinical)	1996	25	AAI, PAA	Both	No
DeKlyen (1996; control)	1996	25	AAI, PAA	Both	No
DeKlyen (1992)	1992	2x25			

Dickstein et al. (2009)	2009	81	AAI, SSP	Both	No
Evans (2008)	2008	66	AAI, SSP	U-D	No
Finger (2006; control)	2006	86	AAI, SSP	Both	No
Finger (2006; methadone)	2006	62	AAI, SSP	Both	No
Honde (2007)	2007	149			
Fonagy et al. (1991; mothers)	1991	96	AAI, SSP	Forced	No
Steele et al. (1996; mothers)	1996	96	AAI, SSP	Four-way	No
Steele et al. (1996; fathers)	1996	90	AAI, SSP	Both	No
Steele et al. (2008)	2008	63			
Steele et al. (1999)	1999	63			
Gaffney et al. (2000)	2000	20	AAI, SSP	Forced	No
George & Solomon (1996)	1996	32	AAI, SRP	Both	No
Gloger-Tippelt et al. (2002)	2002[e]	27	AAI, SSP	Both	No
Gomille & Gloger-Tippelt (1999)	1999	28			
Gojman et al. (2012; urban)	2012	35	AAI, SSP, Ainsworth scales	Both	Yes
Gojman et al. (2012; rural)	2012	31	AAI, SSP, Ainsworth scales	Both	Yes
Grossmann et al. (1988; sample a)	1988[f]	20	AAI, SSP	Forced	No
Grossman et al. (1988; sample b)	1988[g]	45	AAI, SSP	Forced	No
Haltigan et al. (2014)	2014[h]	203	AAI, SSP	Both	No
Hautamaki et al. (2010a)	2010	33	AAI, SSP, PAA	Forced	No
Hautamaki et al. (2010b)	2010	32			
Head (1996)	1996	42	AAI, PAA	Forced	No
Howes et al. (2011)	2011[i]	60	AAI, AQS	Forced	No
Hughes et al. (2001)	2001	106	AAI, SSP	U-D	No
Turton et al. (2004)	2004	52			
Jacobsen et al. (2014; control)	2014	42	AAI, modified SSP	Four-way	No
Jacobsen et al. (2014; foster)	2014	60	AAI, modified SSP	Four-way	No
Jongenelen et al. (2006)	2006	40	AAI, SSP	Forced	No
Kazui et al. (2000)	2000	50	AAI, AQS	Forced	No
Kolar (1993)	1993	66	AAI, SSP	Both	No
Leigh et al. (2004)	2004	30	AAI, SSP	Forced	No
Lionetti (2014)	2014[j]	30	AAI, SSP	Both	
Lyons-Ruth et al. (2005)	2005[k]	41	AAI, SSP	U/CC-D	No
Lyons-Ruth et al. (2003)	2003	45			
Atwood (1995)	1995	20			
Yellin (2001)	2001	35			
McFarland-Piazza et al. (2012)	2012[l]	97	AAI, SSP, ICS	Both	Yes
McMahon et al. (2006)	2006	111	AAI, SSP	Both	No
Fihrer & McMahon (2009)	2009	111			
Murray et al. (2006; control)	2006[m]	51	AAI, SSP	Both	No
Murray et al. (2006; clinical)	2006[m]	38	AAI, SSP	Both	No
Nowacki et al. (2010)	2010	55	AAI, AQS, NICHD measure	Forced	Yes
Pace & Zavattini (2011; adopted)	2011	28	AAI, SRP	Both	No
Pace & Zavattini (2011; control)	2011	12	AAI, SRP	Both	No
Pace et al. (2012)	2012	20			
Posada et al. (1995)	1995	49	AAI, AQS	Forced	No
Priddis & Howieson (2009)	2009	29	AAI, PAA	Forced	No
Raby et al. (2015)	2015	55	AAI, SSP	Both	No
Radojevic (2005)	2005	44	AAI, SSP	Both	No
Radojevic (1992)	1992	44			
Riva Crugnola et al. (2004)	2004	23	AAI, SSP	Both	No
Sagi et al. (1997; communal sleeping)	1997[n]	20	AAI, SSP	Forced	No
Sagi et al. (1997; home-based sleeping)	1997[n]	25	AAI, SSP	Forced	
Aviezer et al. (1999)	1999	43	AAI, SSP, EAS	Forced	Yes
Saunders, et al. (2011)	2011[l]	106	AAI, SSP	Both	No

Schuengel et al. (1999)	1999	85	AAI, SSP	Both	No
Schuengel (1997)	1997	85			
Bakermans-Kranenburg et al. (1999)	1999	85			
Van IJzendoorn & Bakermans-Kranenburg (2006)	2006	85			
Schwartz (1991; preterm)	1991	25/26	AAI, SSP	Both	No
Schwartz (1991; fullterm)	1991	38/39	AAI, SSP	Both	No
Sette (2013; fathers)	2013[o]	15	AAI, SSP	Forced	No
Sette (2013; mothers)	2013[o]	15	AAI, SSP	Forced	No
Shah (2010)	2010	49	AAI, SSP	Four-way	No
Sherman (2009)	2009	81	AAI, SSP	Both	No
Simonelli (2002)	2002	16	AAI, SSP	Four-way	No
Slade et al. (2005)	2005	40	AAI, SSP	Four-way	No
Dermer (1996)	1996	8			
Solomon & George (2011)	2011[p]	59	AAI, SRP	Both	No
Tarabulsy et al. (2005)	2005	64	AAI, AQS, MBQS	Forced	Yes
Van IJzendoorn et al. (1991; mothers)	1991	26	AAI, SSP, Ainsworth scales	Forced	No
Van IJzendoorn et al. (1991; fathers)	1991	29	AAI, SSP, Ainsworth scales	Forced	No
Van Londen – Barentsen (2002)	2002	55	AAI, SSP, Ainsworth scales	Both	Yes
Verhage (2013)	2013	137	AAI, SSP	Both	No
Viziello et al. (1995)	1995	23	AAI, SSP	Forced	No
Von der Lippe et al. (2010)	2010	40	AAI, SSP, Care Index	Four-way	Yes
Ward & Carlson (1995)	1995	74	AAI, SSP, Care Index	Both	Yes
Levine et al. (1991)	1991	42			
Levine (1990)	1990	42			
Goodrich (2002)	2002	70			
Ward et al. (2000)	2000	59	AAI, SSP	Both	No
Wong et al. (2009; fathers)	2009	59	AAI, SSP, competing demand task	Four-way	Yes
Wong et al (2009; mothers)	2009	68	AAI, SSP, competing demand task	Four-way	Yes
Zeanah (1993)	1993	57	AAI, SSP	Forced	No

Notes. AAI = Adult Attachment Interview, SSP = Strange Situation Procedure, AQS = Attachment Q-Sort, SRP = Separation Reunion Procedure, PAA = Preschool Attachment Assessment.

[a] studies including overlapping data are shown in the indented lines and are not included in the meta-analysis.

[b] Partial study data were first presented in (Bahm & Behrens, 2013).

[c] Partial study data were first presented in Buchheim et al. (2000).

[d] Partial study data were first presented in (Coppola, Aureli, Grazia, & Garito, 2008).

[e] Study data were first presented in Gomille (1996).

[f] Partial study data were first presented in Grossmann, Grossmann, Spangler, Suess, and Unzner (1985).

[g] Partial study data were first presented in Escher-Graub and Grossmann (1983).

[h] Partial study data were first presented in (Leerkes & Gudmundson, 2011).

[i] Partial study data were first presented in Howes, Guerra, and Zucker (2007).

[j] Partial study data were first presented in Lionetti and Barone (2013).

[k] Partial study data were first presented in Lyons-Ruth, Connell, Grunebaum, and Botein (1990).

[l] Partial study data were first presented in Riggs and Jacobvitz (Riggs & Jacobvitz, 2002).

[m] Partial study data were first presented in Murray, Fiori-Cowley, Hooper, and Cooper (1996).

[n] Partial study data were first presented in Sagi, Donnell, Van IJzendoorn, Mayseless, and Aviezer (1994).

[o] Partial study data were first presented in Sette and Cassibba (2010).

[p] Partial study data were first presented in Solomon, George, and De Jong (1995).

Table 3

Coding system of moderators

Variable	Coding description
Substantive moderators	
Risk status	0 = non-risk sample 1 = at-risk caregivers or children (e.g. teenage motherhood, preterm birth, adoptive families)
Biological vs non-biological caregiver	0 = Biological parent 1 = Foster parent or adoptive parent
Caregiver gender	0 = Female 1 = Male
Age of child at child-caregiver attachment assessment	Age of the child during the child-caregiver attachment assessment in months. In the case of multiple attachment assessments at different time points, the first measurement was selected[2].
Methodological moderators	
Study design	0 = concurrent 1 = longitudinal
Coder training	0 = No official coder training on AAI and/or SSP 1 = Coders completed official coder training on AAI and SSP

Table 4

Cross tabulation of forced classifications of caregiver state of mind and child-caregiver attachment

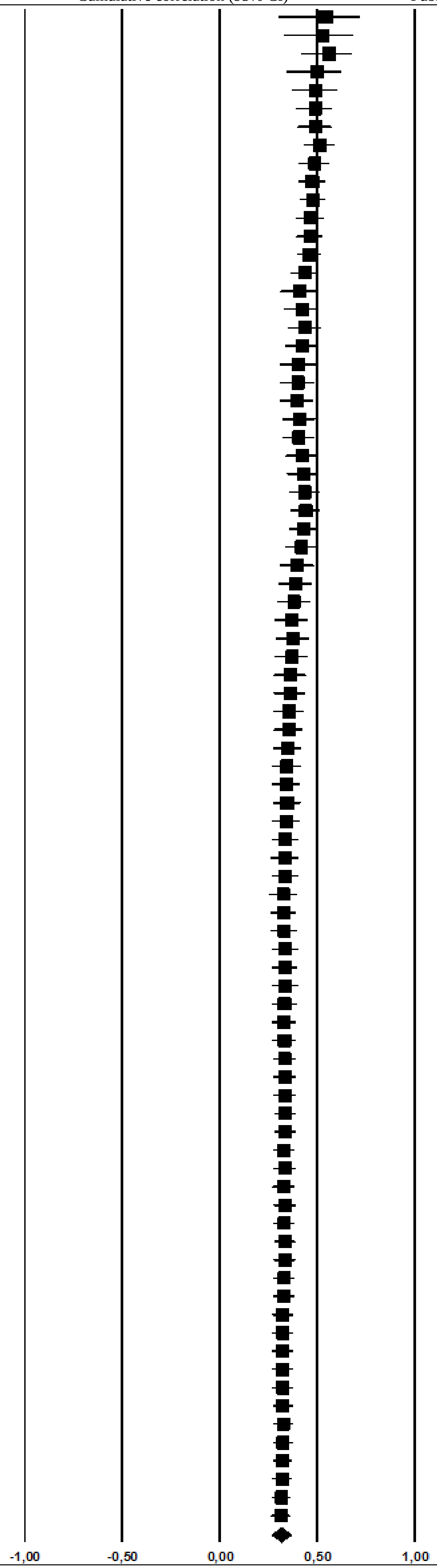
Child attachment	Secure	Avoidant	Resistant	Total	%
Adult attachment					
Autonomous	**1,079**	191	166	1,436	53.9
	(16.9)	(-12.3)	(-8.3)		
Dismissing	314	**309**	133	756	28.4
	(-12.5)	**(14.3)**	(0.4)		
Preoccupied	216	100	**158**	474	17.8
	(-7.3)	(-0.8)	**(10.3)**		
Total	1,609	600	457	2,666	
%	60.4	22.5	17.1		

Note. Adjusted standardized residuals within brackets.

Table 5

Meta-analytic data of the association between autonomous caregiver attachment representations and secure child-caregiver attachment (forced classifications)

		Autonomous		Cumulative meta-analysis	
Study	*N*	Statistic	Correlation *r*	Cumulative correlation (95% CI)	Publication year[3]
Grossmann et al. (1988; Regensburg)	45	13.340[a]	.54		1983
Grossmann et al. (1988; Bielefeld)	20	.0135[b]	.49		1985
Ainsworth & Eichberg (1991)	45	16.411□[a]	.60		1987
Van IJzendoorn et al. (1991; fathers)	29	.1169[b]	.23		1990
Van IJzendoorn et al. (1991;mothers)	26	.0097[b]	.46		1990
Fonagy et al. (1991)	96	22.537[a]	.48		1991
Schwartz (1991; full term)	38	.49[c]	.49		1991
Schwartz (1991; preterm)	26	.73[c]	.73		1991
Steele et al. (1996; fathers)	90	12.91[a]	.38		1991
Ward & Carlson (1995)	74	12.827a	.42		1991
Bus & Van IJzendoorn (1992)	32	0.52[c]	.52		1992
DeKlyen (1996; clinical)	24	.3376[b]	.09		1992
DeKlyen (1996; control)	24	.0212[b]	.42		1992
Radojevic (2005)	44	9.031[a]	.45		1992
Dedrick (1993)	63	0.325[d]	.15		1993
Kolar (1993)	66	0.062[a]	- .03		1993
Zeanah (1993)	57	20.216[a]	.60		1993
Benoit & Parker (1994)	85	32.239[a]	.62		1994
Madigan et al. (2011; fathers)	31	.3123[b]	.09		1994
Sagi et al. (1997; communal sleeping)	20	.2145[b]	- .19		1994
Sagi et al. (1997; home-based sleeping)	25	.1008[b]	.26		1994
Ammaniti & Speranza (1995)	32	.0540[b]	.29		1995
Pederson et al. (1998)	60	21.472[a]	.60		1995
Posada et al. (1995)	49	0.699[d]	.34		1995
Solomon & George (2011)	48	25.50[aw]	.73		1995
Vizziello (1995)	23	.0010[b]	.61		1995
George & Solomon (1996)	32	.0000[b]	.64		1996
Gloger-Tippelt et al. (2002)	28	.0009[b]	.56		1996
Head (1996)	42	1.207[a]	.17		1996
Murray et al. (2006)	87	0.775[a]	.09		1996
Schuengel et al. (1999)	85	4.047[a]	- .22		1997
Aux (2000)	53	.16[c]	.16		2000
Brisch et al. (2005)	65	0.860[a]	.12		2000
Gaffney (2000)	20	.1355[b]	- .26		2000
Kazui et al. (2000)	50	1.590[d]	.60		2000
Ward et al. (2000)	49	1.081a	.15		2000
Bailey et al. (2007)	98	0.404[a]	.06		2001
Raval et al. (2001)	96	7.806[a]	.29		2001
McFarland-Piazza et al. (2012; fathers)	94	5.018[a]	.23		2002
Saunders et al. (2011)	104	8.684[a]	.29		2002
Van Londen-Barentsen (2002)	55	0.742[a]	.12		2002
Cassibba et al. (2004; asthma)	30	0.335[d]	.16		2004
Cassibba et al. (2004; control)	30	0.460[d]	.21		2004
Riva Crugnola et al. (2004)	27	.0012[b]	.56		2004
Leigh et al. (2004)	30	.2964[b]	.10		2004
Ammaniti et al. (2005; clinical)	19	.5000[b]	.00		2005
Ammaniti et al. (2005; control)	23	.2846[b]	.13		2005
Behrens et al. (2007)	41	6.508[a]	.40		2005
Chin (2013)	104	.00[c]	.00		2005
Tarabulsy et al. (2005)	64	0.36[c]	.36		2005
Arnott & Meins (2007; fathers)	15	.0167[b]	.55		2006
Arnott & Meins (2007; mothers)	18	.0039[b]	.61		2006
Costantini (2006; control)	20	.2401[b]	.17		2006
Costantini (2006; preterm)	20	.0027[b]	.60		2006
Finger (2006; control)	86	1.139[a]	.12		2006
Finger (2006; methadone)	62	5.415[a]	.30		2006
Jongenelen et al. (2006)	40	6.628[a]	.41		2006
McMahon et al. (2006)	111	15.541[a]	.37		2006
Costantino (2007; control)	19	.0227[b]	.46		2007
Costantino (2007; social disadvantage)	25	.0162[b]	.43		2007
Howes et al. (2011)	60	.40[c]	.40		2007
Coppola et al. (2010)	22	.4060[b]	.05		2008
Dickstein et al. (2009)	81	0.365[a]	.07		2009
Priddis & Howieson (2009)	29	.0003[b]	.60		2009
Sherman (2009)	81	0.114[a]	- .04		2009
Hautamaki et al. (2010)	33	.000[b]	.72		2010
Nowacki et al. (2010)	55	0.174[d]	.09		2010
Sette (2013; fathers)	15	.0088[b]	.60		2010
Sette (2013; mothers)	15	.0440[b]	.46		2010
Cassibba et al. (2011;clinical)	20	.4087[b]	.06		2011
Cassibba et al. (2011; control)	20	.2947[b]	.13		2011
Haltigan et al. (2013)	137[w]	0.300[a]	.05		2011
Pace & Zavattini (2011; adopted)	28	.1539[b]	.20		2011
Pace & Zavattini (2011; control)	12	.4242[b]	.06		2011
Gojman et al. (2012; rural)	31	.1269[b]	.21		2012
Gojman et al. (2012; urban)	35	.0013[b]	.49		2012
Behrens et al. (in prep)	66	12.614[a]	.44		2013
Coppola et al. (2014)	40	1.005[b]	.43		2013
Lionetti & Barone (2014)	30	.0975[b]	.24		2013
Verhage (2013)	137	6.246[a]	.21		2013
Bernier et al. (2014)	130	0.296[d]	.14		2014
Berthelot et al. (2015)	57	0.732[a]	.11		2014
Raby et al. (2015)	54	0.078[a]	- .04		2014
Combined	4,102	.31[c]	.31		



Note. Dashes indicate non-applicability due to empty categories, [w] indicates a winsorized value.
[a] Chi-square value. [b] One-tailed Fisher exact probability value. [c] Pearson correlation coefficient. [d] Cohen's *d*.

Table 6

Moderators for the intergenerational transmission of autonomous-secure attachment (forced classifications)

Substantive moderators (categorical)	*k*	*N*	*r*	95% CI	Homogeneity *Q*	Contrast *Q*	Contrast *p*
Risk status						15.74**	< .001
No risk	54	2,520	.38**	.32, .44	147.01**		
Risk	29	1,582	.18**	.10, .26	67.44**		
Biological vs non-biological caregiver						5.00*	.03
Biological caregiver	79	3,934	.32**	.27, .37	250.19**		
Foster/adoptive caregiver	4	168	.14	-.01, .29	0.6		
Caregiver gender						0.07	.79
Female	75	3,742	.31**	.25, .37	246.76**		
Male	7	318	.33**	.21, .43	6.69		

Substantive moderator (continuous)	*k*	*N*	Slope	*SE*	Model test *Q*	*z*	*p*
Child age C-C	82	2,267	0.002	0.002	1.25	1.12	.26

Methodological moderator (categorical)	*k*	*N*	*r*	95% CI	Homogeneity *Q*	Contrast *Q*	Contrast *p*
Study design						0.75	.39
Longitudinal	50	2,899	.30**	.23, .36	175.61**		
Cross-sectional	32	1,173	.35**	.26, .42	71.98**		
Coder training						1.83	.18
Official training	48	2,431	.30**	.22, .37	160.61**		
No official training	12	599	.37**	.29, 45	13.43		

Note. * p < .05, ** p < .01.

Table 7

Cross tabulation of four-way classifications of caregiver state of mind and child-caregiver attachment

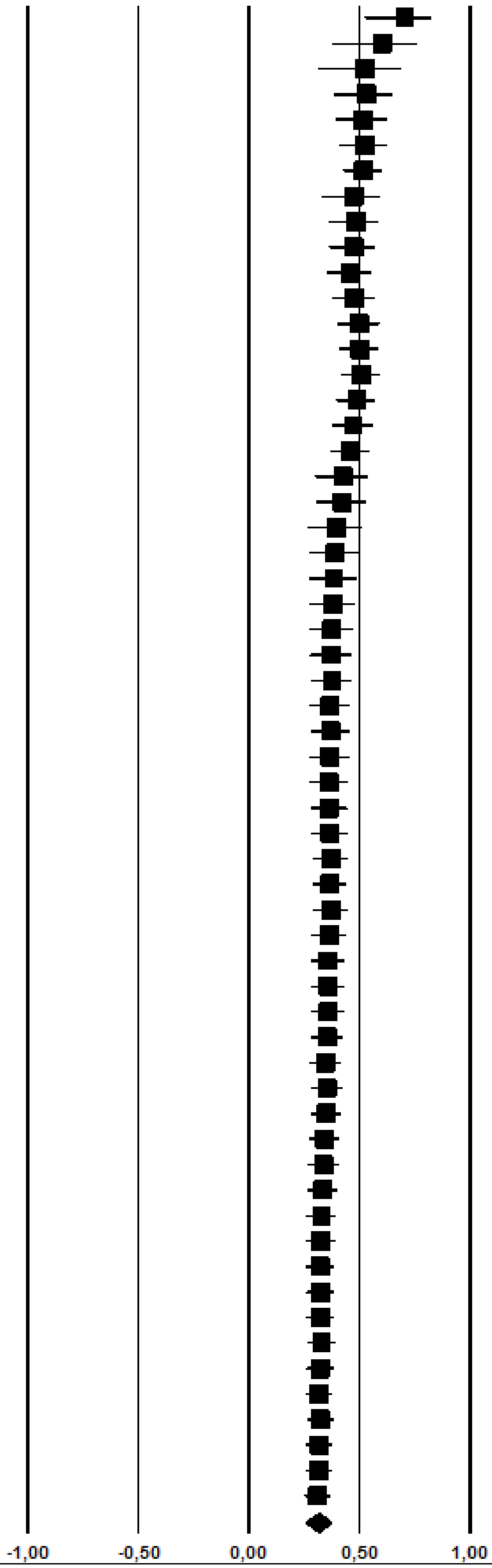
Child attachment	Secure	Avoidant	Resistant	Disorganized	Total	%
Adult attachment						
Autonomous	**912**	122	88	196	1,318	43.8
	(17.1)	(-7.6)	(-5.5)	(-9.9)		
Dismissing	235	**192**	68	146	641	21.4
	(-9.0)	**(12.5)**	(0.6)	(-0.3)		
Preoccupied	77	39	**66**	58	240	8.0
	(-6.5)	(0.7)	**(9.5)**	(0.4)		
Unresolved	223	53	55	**244**	575	19.2
	(-7.2)	(-4.1)	(-0.4)	**(12.3)**		
Total	1,447	406	277	644	2,774	
%	48.3	13.6	9.2	21.5		

Note. Adjusted standardized residuals within brackets.

Table 8

Meta-analytic data of the association between autonomous caregiver attachment representations and secure child-caregiver attachment (four-way classifications)

		Autonomous		Cumulative meta-analysis	
Study	*N*	Statistic	Correlation	Cumulative correlation (95% CI	Publication year[3]
Ainsworth & Eichberg (1991)	45	22.367[a]	.71		1987
Fonagy et al. (1991)*	96	24.873[a]	.51		1991
Steele et al. (1996; fathers)	90	11.075[a]	.35		1991
Ward & Carlson (1995)	74	23.718[a]	.57		1991
DeKlyen (1996; clinical)	25	.42[c]	.42		1992
DeKlyen (1996; control)	25	.60[c]	.60		1992
Radojevic (2005)	44	11.396[a]	.51		1992
Kolar (1993)	66	0.282[a]	.07		1993
Benoit et al. (1994)	85	25.673[a]	.54		1994
Madigan et al. (2011)	31	.0139[b]	.40		1994
Ammaniti & Speranza (1995)	32	.0914[b]	.24		1995
Pederson et al. (1998)	60	24.031[a]	.63		1995
Solomon & George (2011)	59	29.50[aw]	.71		1995
George & Solomon (1996)	32	.0008[b]	.54		1996
Gloger-Tippelt et al. (2002)	28	.0002[b]	.62		1996
Murray et al. (2006; clinical)	51	1.274[a]	.16		1996
Murray et al. (2006; control)	38	0.869[a]	.15		1996
Slade (2005)	40	.24[c]	.24		1996
Schuengel et al. (1999)	85	6.157[a]	- .27		1997
Bernier & Dozier (2003)	64	.37[c]	.37		2000
Brisch et al. (2005)	65	2.042[a]	- .18		2000
Ward et al. (2000)	60	4.848[a]	.28		2000
Bailey et al. (2007)	98	6.957[a]	.27		2001
Raval et al. (2001)	96	10.985[a]	.34		2001
McFarland-Piazza et al. (2012)	97	4.871[a]	.22		2002
Saunders et al. (2011)	104	14.141[a]	.37		2002
Simonelli (2002; clinical)	16	3.626[a]	.48		2002
Van Londen-Barentsen (2002)	55	0.454[a]	.09		2002
Riva Crugnola et al. (2004)	27	.0035[b]	.51		2004
Ammaniti et al. (2005; clinical)	19	.1630[b]	.24		2005
Ammaniti et al. (2005; control)	23	.4099[b]	.05		2005
Behrens et al. (2007)	41	9.918[a]	.48		2005
Arnott & Meins (2007; fathers)	15	.0167[b]	.55		2006
Arnott & Meins (2007; mothers)	18	.0124[b]	.53		2006
Costantini (2006; control)	20	.2401[b]	.17		2006
Costantini (2006; preterm)	20	.0027[b]	.60		2006
Finger (2006; control)	86	0.522[a]	.08		2006
Finger (2006; methadone)	62	1.342[a]	.15		2006
McMahon et al. (2006)	111	16.178[a]	.38		2006
Von der Lippe (2010)	40	.40[c]	.40		2006
Dickstein (2009)	96	3.301[a]	.19		2009
Sherman (2009)	81	0.149[a]	.04		2009
Wong et al. (2009; fathers)	59	19.963[a]	.58		2009
Wong et al. (2009; mothers)	68	4.164[a]	.25		2009
Shah et al. (2010)	49	0.089[a]	- .04		2010
Cassibba et al. (2011; clinical)	20	.4087[b]	.06		2011
Cassibba et al. (2011; control)	20	.2947[b]	.13		2011
Haltigan et al (2014)	137[w]	0.584[a]	.07		2011
Pace & Zavattini (2011; adopted)	28	.3054[b]	.10		2011
Pace & Zavattini (2011; control)	12	.4242[b]	.06		2011
Gojman et al. (2012; rural)	31	.0072[b]	.44		2012
Gojman et al. (2012; urban)	35	.0099[b]	.39		2012
Behrens et al. (in prep)	66	14.030[a]	.46		2013
Jacobsen et al. (2014; control)	42	0.003[a]	.01		2013
Jacobsen et al. (2014; foster)	60	0.145[a]	.05		2013
Lionetti & Barone (2014)	30	.0001[b]	.63		2013
Verhage (2013)	137	2.111[a]	.12		2013
Berthelot et al. (2015)	57	4.534[a]	.28		2014
Raby et al. (2015)	55	0.035[a]	- .03		2014
Combined	3,226	.31[c]	.31		



Note. Striked-through studies were outliers in the analysis, dashes indicate non-applicability due to empty categories.
[a] Chi-square value. [b] One-tailed Fisher exact probability value. [c] Pearson correlation coefficient. [d] Cohen's *d*.
* Data extracted from Steele et al., 1996 on same sample as Fonagy, et al., 1991.

Table 9

Moderators for the intergenerational transmission of autonomous-secure attachment (four-way classifications)

Substantive moderators (categorical)	*k*	*N*	*r*	95% CI	Homogeneity *Q*	Contrast *Q*	Contrast *p*
Risk status						5.96*	.02
No risk	37	2,024	.37**	.30, .44	113.38**		
Risk	22	1,202	.21**	.10, .32	72.55**		
Biological vs non-biological caregiver						0.29	.59
Biological caregiver	54	2,989	.32**	.25, .38	193.06**		
Foster/adoptive caregiver	5	237	.26*	.03, .46	11.93*		
Caregiver gender						1.94	.16
Female	51	2,788	.31**	.24, .38	186.14**		
Male	6	336	.41**	.28, .53	8.37		

Substantive moderator (continuous)	*k*	*N*	Slope	*SE*	Model test *Q*	*z*	*p*
Child age C-C[a]	58	3,214	0.004	0.002	3.35	1.83	.07

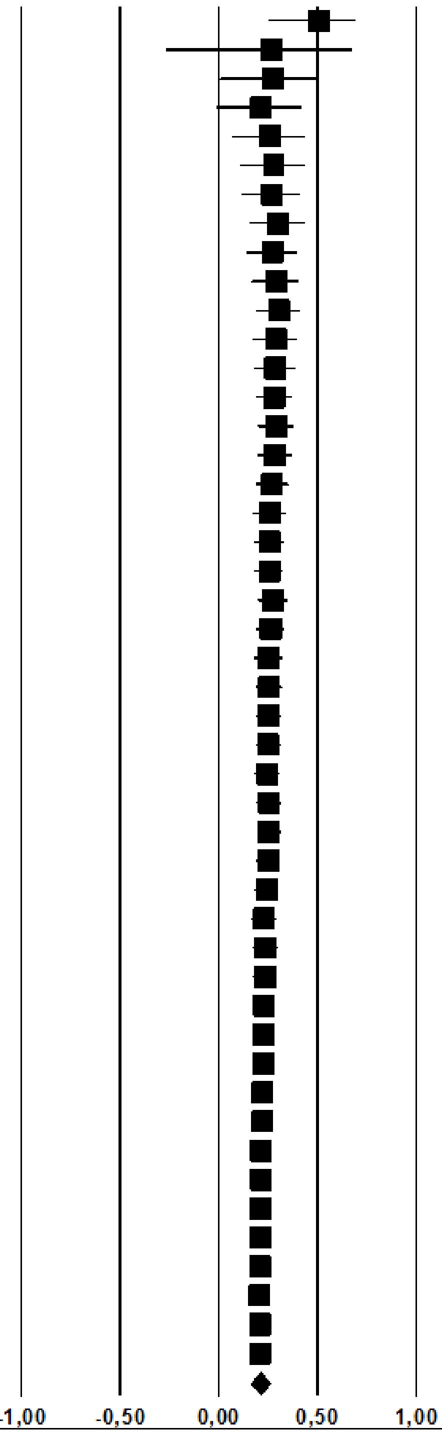
Methodological moderator (categorical)	*k*	*N*	*r*	95% CI	Homogeneity *Q*	Contrast *Q*	Contrast *p*
Study design						< 0.001	.98
Longitudinal	37	2,403	.31**	.23, .39	154.30**		
Cross-sectional	22	823	.31**	.21, .41	51.34**		
Coder training						0.801	.37
Official training	38	2,191	.32**	.24, .40	144.58**		
No official training	9	523	.38**	.28, 47	12.23		

Note. * $p < .05$, ** $p < .01$.
[a] The study by George and Solomon (1996) was winsorized, because it had an outlying value on the moderator variable.

Table 10

Meta-analytic data of the association between caregiver unresolved attachment representations and the disorganized child-caregiver attachment relationship

Study	*N*	Unresolved Statistic	Unresolved Correlation	Cumulative meta-analysis: Cumulative correlation (95% CI)	Publication year[3]
Ainsworth & Eichberg (1991)	45	25.741[a]	.51		1987
Lyons-Ruth et al. (2005)	41	.01[c]	- .01		1990
Fonagy et al. (1991)*	96	8.125[a]	.29		1991
Steele et al. (1996; fathers)	90	0.144[a]	.04		1991
Ward & Carlson (1995)	74	12.949[a]	.42		1991
DeKlyen (1996; clinical)	25	4.812[a]	.44		1992
DeKlyen (1996; control)	25	.2108[b]	.17		1992
Radojevic (2005)	44	10.923[a]	.50		1992
Kolar (1993)	66	0.474[a]	.08		1993
Benoit et al. (1994)	88	14.529[a]	.41		1994
Madigan et al. (2011)	31	.0017[b]	.51		1994
Ammaniti & Speranza (1995)	32	.4173[b]	.04		1995
Pederson et al. (1998)	60	3.665[a]	.25		1995
Solomon & George (2011)	59	3.445[a]	.24		1995
George & Solomon (1996)	32	.0039[b]	.46		1996
Gloger-Tippelt et al. (2002)	28	.1786[b]	.18		1996
Schuengel et al. (1999)	85	0.492[a]	.08		1997
Brisch et al. (2005)	65	0.739[a]	.11		2000
Ward et al. (2000)	60	3.022[a]	.22		2000
Bailey et al. (2007)	99	6.282[a]	.25		2001
Hughes et al. (2001)	106	.50[c]	.50		2001
Raval et al. (2001)	96	0.480[a]	.07		2001
McFarland-Piazza et al. (2012)	97	0.624[a]	.08		2002
Saunders et al. (2011)	104	7.591[a]	.27		2002
Van Londen-Barentsen (2002)	55	2.497[a]	.21		2002
Crugnola et al. (2004)	27	.2621[b]	.13		2004
Ammaniti et al. (2005; clinical)	19	.5000[b]	.00		2005
Behrens et al. (2007)	43	10.962[a]	.50		2005
Arnott & Meins (2007; fathers)	15	.2000[b]	.23		2006
Arnott & Meins (2007; mothers)	18	.5000[b]	.00		2006
Finger (2006; control)	86	0.422[a]	.07		2006
Finger (2006; methadone)	62	0.827[a]	- .12		2006
McMahon et al. (2006)	111	14.745[a]	.36		2006
Evans (2008)	66	1.940[a]	.17		2008
Dickstein (2009)	96	1.038[a]	.10		2009
Sherman (2009)	81	1.203[a]	.12		2009
Wong et al. (2009; fathers)	59	5.651[a]	.31		2009
Wong et al. (2009; mothers)	68	0.371[a]	.07		2009
Shah et al. (2010)	49	1.041[a]	.15		2010
Haltigan et al (2014)	137[w]	0.203[a]	.04		2011
Gojman et al. (2012; rural)	31	.2582[b]	.12		2012
Gojman et al. (2012; urban)	35	.1109[b]	.21		2012
Behrens et al. (in prep)	66	0.315[a]	.07		2013
Lionetti & Barone (2014)	30	.2759[b]	.11		2013
Verhage (2013)	137	1.627[a]	.11		2013
Berthelot et al. (2015)	57	8.757[a]	.39		2014
Raby et al. (2015)	55	1.742[a]	.18		2014
Combined	2,945	.21[c]	.21		



Note. Striked-through studies were outliers in the analysis, dashes indicate non-applicability due to empty categories.
[a] Chi-square value. [b] One-tailed Fisher exact probability value. [c] Pearson correlation coefficient. [d] Cohen's *d*.
* Data extracted from Steele et al., 1996 on same sample as Fonagy, et al., 1991.

Table 11

Moderators for the intergenerational transmission of unresolved-disorganized attachment (four-way classifications)

Substantive moderators (categorical)	*k*	*N*	*r*	95% CI	Homogeneity *Q*	Contrast *Q*	Contrast *p*
Risk status						0.06	.81
No risk	30	1,855	.20**	.14, .26	44.71*		
Risk	17	1,090	.22**	.13, .30	35.16**		
Caregiver gender						0.41	.52
Female	41	2,609	.21**	.15, .26	68.10**		
Male	6	336	.27**	.08, .43	12.85*		

Substantive moderator (continuous)	*k*	*N*	Slope	*SE*	Model test *Q*	*z*	*p*
Child age C-C[a]	47	1,656	0.004	0.002	3.96*	1.99	.047

Methodological moderator (categorical)	*k*	*N*	*r*	95% CI	Homogeneity *Q*	Contrast *Q*	Contrast *p*
Study design						0.34	.56
Longitudinal	35	2,432	.22**	.16, .27	61.11**		
Cross-sectional	12	513	.18**	.06, .29	18.80		
Coder training						0.01	.92
Official training	34	2,204	.21**	.15, .27	61.78**		
No official training	6	443	.21**	.07, .32	9.15		

Note. * $p < .05$, ** $p < .01$.
Moderator analyses could not be carried out with caregiver type (non-biological caregivers *k* = 2).
[a] The study by George and Solomon (1996) was winsorized, because it had an outlying value on the moderator variable.

Table 12

Meta-analytic results used for path model on the mediating role of caregiver sensitivity in the association between caregiver attachment representation and child-caregiver attachment security[7]

Study name	AAI – CC attachment		AAI – Sensitivity		Sensitivity – CC attachment	
	N	Pearson's *r*	N	Pearson's *r*	N	Pearson's *r*
Aviezer et al. (1999; communal sleeping)	20	-.24	20	.42	20	-.11
Aviezer et al. (1999; home sleeping)	25	.36	23	.23	23	.45
Bailey et al. (2007; adolescent mothers)	99	.06	99	.14	99	.28
Bernier et al. (2014)	130	.23	130	.29	130	.39
Cassibba et al. (2011; clinical)	20	.36	20	.30	20	.04
Cassibba et al. (2011; comparison)	20	.37	20	.36	20	.53
Chin (2013; preterm children)	104	.00	104	.05	104	.70
Coppola et al. (2010)	22	.04	22	.20	22	-.16
Costantini (2012; preterm children)	40	.30	40	.17	40	.38
Gojman et al (2012; rural)	31	.45	31	.41	31	.35
Gojman et al (2012; urban)	35	.47	32	.31	32	.62
McFarland-Piazza et al. (2012; fathers)	97	.22	97	.21	97	.58
Nowacki et al (2012; foster children)	55	-.17	55	-.06	55	.30
Pederson et al. (1998)	60	.60	60	.28	60	.51
Raval et al. (2001)	96	.25	96	.18	96	.35
Tarabulsy et al. (2005; adolescent mothers)	64	.36	64	.37	64	.40
Van Londen-Barentsen (2002; adoptive children)	55	.24	55	-.14	55	.05
Von der Lippe et al. (2010)	40	.40	40	.37	40	.69
Ward & Carlson (1995; adolescent mothers)	74	.57	74	.28	74	.06
Wong et al. (2009; fathers)	59	.10	91	.15	63	.15
Wong et al. (2009; mothers)	68	.05	88	.14	69	-.02
Combined	1,214	.25	1,261	.20	1,214	.35

Note. * $p < .05$, ** $p < .01$. CC = child-caregiver.

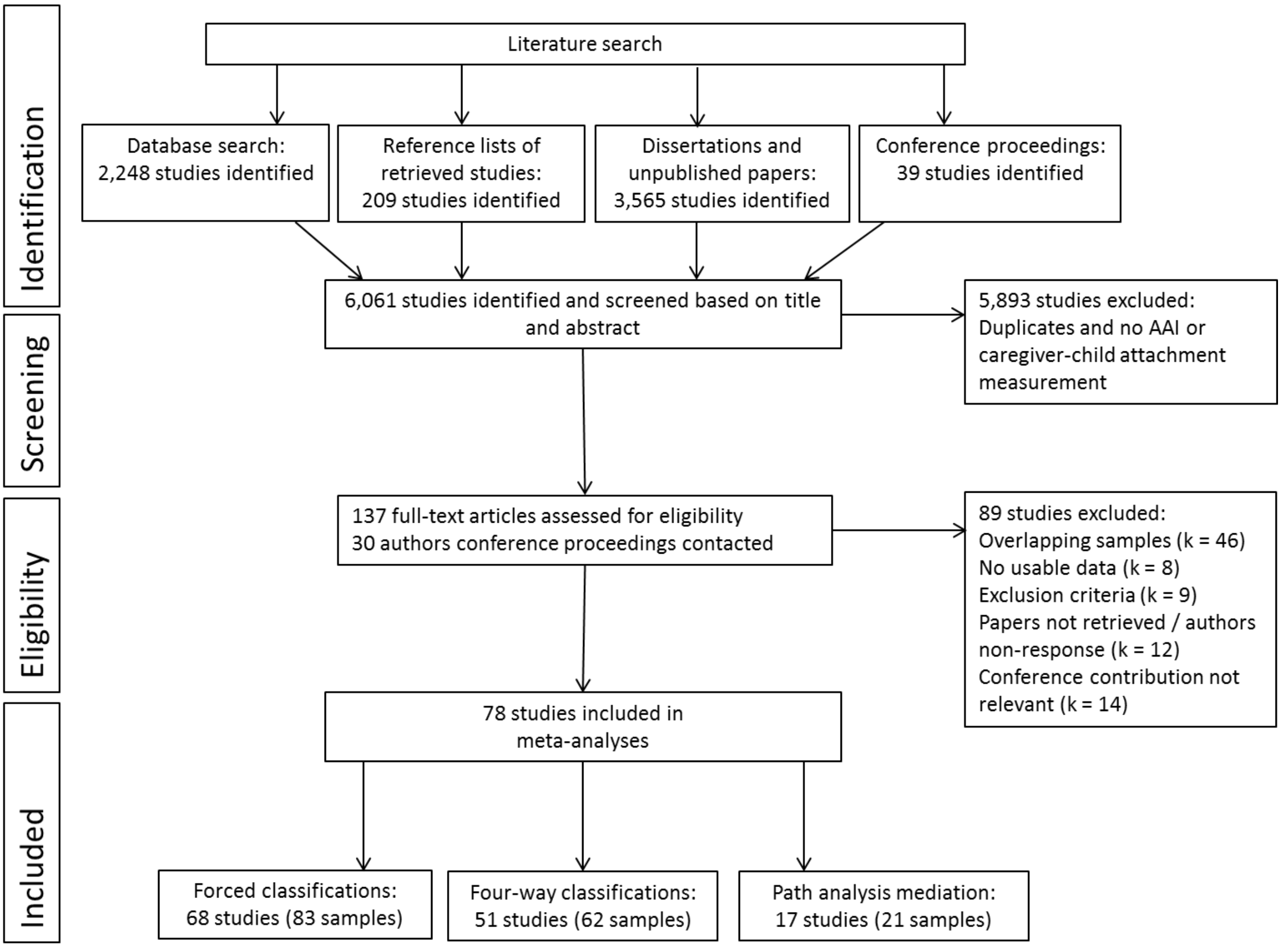


Figure 1. PRISMA flow chart used to identify studies for the current meta-analyses.

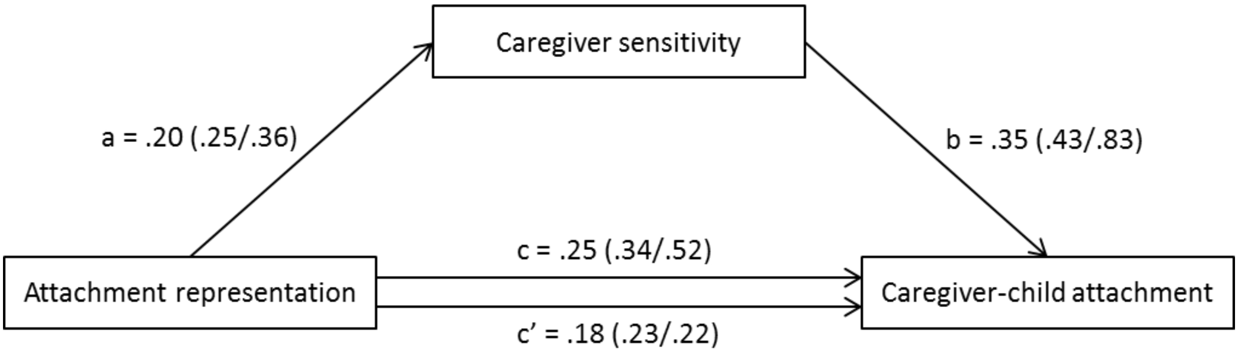


Figure 2. Path model of mediation effect of caregiver sensitivity and the transmission gap.

Note. The path coefficients are in standardized metric. Values in parentheses are values after correction for attenuation for interrater reliability and values after correction for attenuation for test-restest reliability.

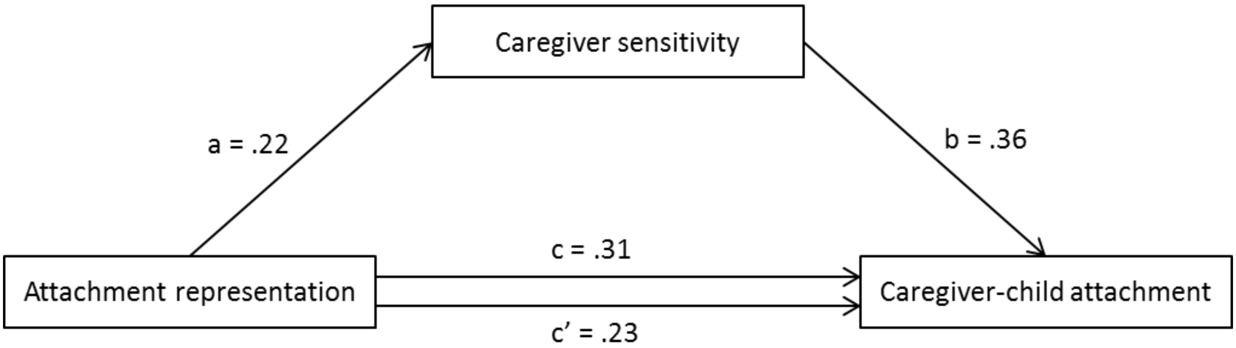


Figure 3a. Path model including studies performed before 2006.

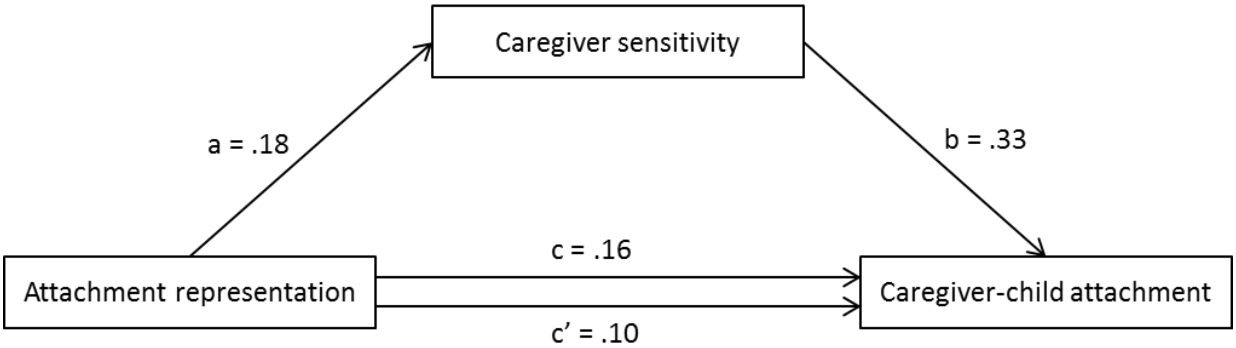


Figure 3b. Path model including studies performed after 2006.

Figure 3. Path models of mediation effect of caregiver sensitivity and the transmission gap of studies performed before and after 2006.

Note. The path coefficients are in standardized metric.

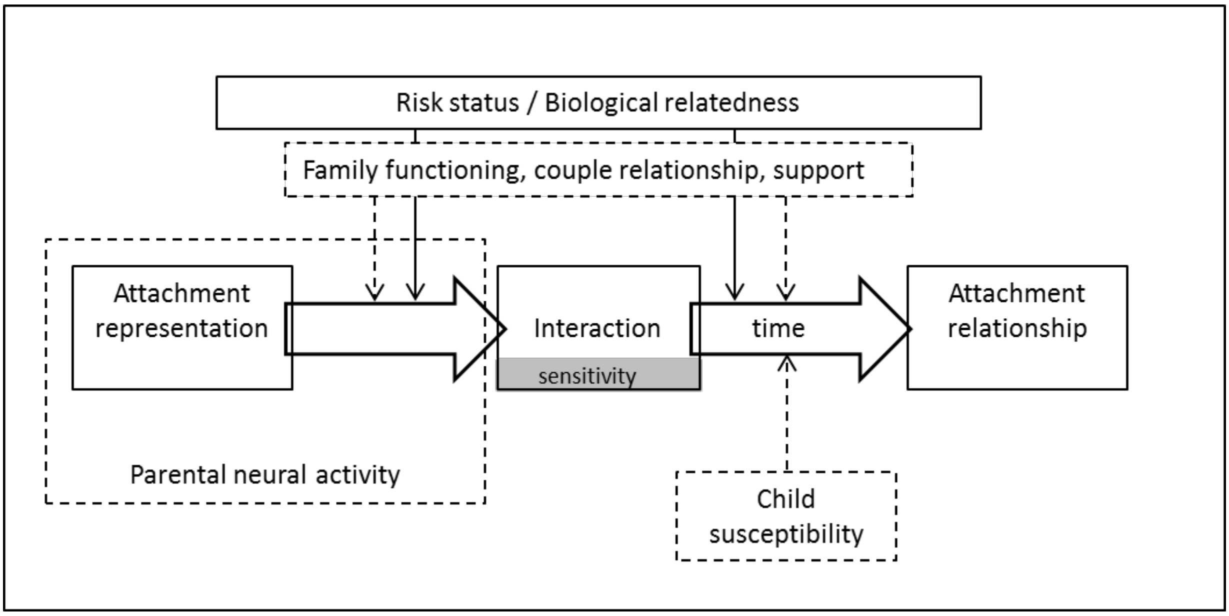


Figure 4. Proposed theoretical model of the intergenerational transmission of attachment.

Constructs in dashes remain to be investigated.