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Does attachment security predict children's thinking-about-thinking and thinking-about-feeling? A meta-analytic review

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A B S T R A C T

Previous research presents no clear picture of the association between caregiver–child attachment and the two hallmarks of children's mentalizing abilities: false-belief understanding (FBU) and emotion understanding (EU). The present meta-analytic study investigated four questions: (a) what is the pooled correlation between attachment and children's mentalizing abilities, as indicated by FBU and EU?; (b) are there differences in the magnitude of correlations between attachment and FBU on the one hand, and attachment and EU on the other hand?; (c) does children's verbal ability mediate the relation between attachment and children's FBU and EU?; (d) is the relation between attachment and children's mentalization moderated by the attachment assessment approach (behavioral vs. representational) and/or instrument? A total of 64 effect sizes ($N = 1734$ children) were subjected to multilevel analyses. The results showed that the association between attachment and EU, $r = 0.31$, was significantly larger than the association between attachment and FBU, $r = 19$. Language ability partially mediated the association between attachment and FBU, but not attachment and EU. Studies using behavioral measures of attachment reported lower correlations compared to studies using representational measures. The findings suggest that the association between attachment and FBU is indirect, and that methodological differences between the different attachment measures may partially explain the significant relations between attachment and children's mentalizing abilities.

Introduction

The concept of mentalizing, or theory of mind, encapsulates how we make sense of our own and others' minds. It refers to an imaginative mental activity: perceiving and interpreting human behavior in terms of mental states (e.g., needs, desires, feelings, beliefs, goals, and reasons; Allen, 2003). Mentalizing facilitates social functioning because it makes people's behavior predictable and comprehensible. Even walking through a market requires us to attribute thoughts and intentions to others, or we would constantly bump into each other. Mentalizing makes people's behavior meaningful—the meaning we attribute to our own and others' behavior is crucial to how we see and feel about ourselves, others, and our relationships with others. Disturbances in the capacity to mentalize frequently and/or accurately appear to be a transdiagnostic factor across a wide range of disorders (e.g., autism, anxiety, psychosis, depression, eating disorders, borderline personality disorder; Chung, Barch, & Strube, 2014; Cusi, Nazarov, Holshausen, MacQueen, & McKinnon, 2012; Fonagy & Luyten, 2016; Kuipers & Bekker, 2012; Ladegaard, Larsen, Videbech, & Lysaker, 2014; Luyten, Fonagy, Lemma, & Target, 2012; Skårderud, 2007). Understanding the development of people's mentalizing capacity and tendency has therefore been an important goal of the social and behavioral sciences.

The ability to mentalize is thought to stem from an evolutionary process of language development and social cooperation (Dunbar, 1998). To increase the chance of survival and reproduction in a potentially frightening and increasingly complex social world, conscious reflection and planning of action were required (Cortina & Liotti, 2010). It became an apparent asset to imagine

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one's own and others' thoughts, beliefs, and feelings so that one was able to anticipate people's behavior, make decisions, and manage our own responses (Damasio, 2010; Freeman, 2016). The evolutionary development of language might have been crucial in this process since the unique feature of language is the ability to transmit information about things in the physical and mental realm. By using language and symbols, humans became able to bridge the world of imagination and reality (Slade, 2005; Winnicott, 1965,1971).

Early forms of mentalizing emerge in infancy, such as intentional or referential communication (Colonnesi, Stams, Koster, & Noom, 2010; Liszkowski & Tomasello, 2011) and implicit (nonverbal) understanding of others' intentions, desires, and beliefs (e.g., Brooks & Meltzoff, 2015; Colonnesi, Rieffe, Koops, & Perucchini, 2008; Yott & Poulin-Dubois, 2016). These mentalizing tendencies during infancy have been found to be precursors of mentalizing abilities later in childhood, and develop into a full understanding of one's own and others' mental states (e.g., Colonnesi et al., 2008; Thoermer, Sodian, Vuori, Perst, & Kristen, 2013; Wellman, Lopez-Duran, LaBounty, & Hamilton, 2008). Moreover, meta-analytic data show that individual differences in children's mentalizing abilities predict prosocial behavior and peer relationships (Imuta, Henry, Slaughter, Selcuk, & Ruffman, 2016; Slaughter, Imuta, Peterson, & Henry, 2015). Hence, mentalizing seems to evolve throughout childhood and plays an important role in children's social functioning.

Two of the most examined hallmarks of children's mentalizing development are the emerging abilities to understand false beliefs and emotions in others. *False-belief understanding* (FBU) entails whether children are able to understand that actions or thoughts are driven by different beliefs that people hold. The classic task assessing FBU is a storytelling task developed by Wimmer and Perner (1983), which was later modified to what is known as the Sally-Anne test (Baron-Cohen, Leslie, & Frith, 1985). A puppet named Sally takes a marble and hides it in her basket. She then "leaves" the room and goes for a walk. While she is away, Anne takes the marble out of Sally's basket and puts it in her own box. Sally is then reintroduced and the child is asked: "Where will Sally look for her marble?" Around 4 years of age there is substantial variation in whether or not children correctly understand where Sally will look for the marble, providing information on the advancement of children's developing mentalization abilities (Baron-Cohen et al., 1985). The main focus points of research on this task were to grasp (a) the age at which children typically appreciate the representational nature of mental states, and (b) the deviations in mentalizing among children with autism spectrum disorders (for meta-analyses, see Wellman, Cross, & Watson, 2001; Yirmiya, Erel, Shaked, & Solomonica-Levi, 1998).

Emotion understanding (EU) entails whether a child is able to recognize emotional states from facial expressions and understand the causes of emotions that are typically felt by individuals (Denham, 1986). The classic task assessing this ability is Denham's (1986) puppet task. This task includes affect labeling and affective perspective-taking judgements, and measures the degree to which children are able to match facial expressions to the correct emotion (i.e., sad, happy, angry, scared). The task involves hand puppets enacting multiple stories, after which the child is asked how the puppet would feel and is then asked to affix the appropriate felt face. Performance on EU tasks also varies greatly among preschoolers, and both FBU and EU performance have been shown to predict positive aspects of children's socio-emotional development, such as prosocial behavior and social competence (Barreto, Osório, Baptista, Fearon, & Martins, 2017; Cassidy, Werner, Rourke, Zubernis, & Balaraman, 2003; Imuta et al., 2016; Walker, 2005; Weimer, Dowds, Fabricius, Schwanenflugel, & Suh, 2017), moral reasoning (Lane, Wellman, Olson, LaBounty, & Kerr, 2010), and peer popularity (Slaughter et al., 2015).

A number of meta-analyses have been performed to establish the factors that predict children's mentalizing abilities, and highlight children's executive functioning, language ability, number of siblings, and socioeconomic status as predictors of performance on FBU and EU tasks (Cooke, Stuart-Parrigon, Movahed-Abtahi, Koehn, & Kerns, 2016; Devine & Hughes, 2014, 2016; Milligan, Astington, & Dack, 2007). Furthermore, two meta-analyses showed that parents who tend to be more attuned to the mental states of their child or who tend to talk regularly about mental states to their children have preschoolers who perform better on FBU tasks regardless of their children's language ability (Devine & Hughes, 2016; Tompkins, Benigno, Kiger-Lee, & Wright, 2018). These findings highlight the role of the parent in the development of children's mentalizing abilities.

Predicting children's mentalizing abilities – the role of attachment security

The ability to appreciate oneself as an individual with a mind is not a genetic given and is thought to be acquired in the context of interpersonal experiences with primary caregivers (Carpendale & Lewis, 2004; Fonagy, 2006), and further shaped by relationships with significant others, such as siblings and peers (e.g., Banerjee, Watling, & Caputi, 2011; Ruffman, Perner, Naito, Parkin, & Clements, 1998). Attachment theory provides an important framework for understanding family influences on children's developing mentalizing abilities (Fonagy, Gergely, Jurist, & Target, 2002). Attachment theory proposes that the basic evolutionary purpose of the newborn's attachment instinct is to maintain proximity to an attachment figure (Bowlby, 1969/1982; Cortina & Liotti, 2010; Midgley & Vrouva, 2012). The infant's attachment behaviors, such as crying or smiling, are answered by attachment behaviors of the adult (touching, holding, soothing), and these reactions strengthen the infant's attachment behavior toward that particular adult. At the end of the first year, there are strong differences in the extent to which secure and insecure infants apply different self-regulatory behaviors when physiologically stressed (Beijers, Riksen-Walraven, Sebesta, & de Weerth, 2017). Furthermore, there are strong differences in the extent to which infants seek out and are comforted by a particular attachment figure when they encounter danger and stress (Ainsworth, Blehar, Waters, & Wall, 1978; Bowlby, 1969/1982). Securely attached infants turn to their attachment figure in times of distress, and both behavioral and physiological signs of negative affect disappear relatively quickly through contact with the attachment figure (Ainsworth et al., 1978; Main, 2000). Where the securely attached infant is able to use the caregiver to become regulated in an effective way, the insecurely attached infant is typically unable to do so.

Over the course of the preschool years, the infant's attachment behavior is proposed to develop into an internal working model

(IWM), a cognitive framework comprising mental representations of attachment relationships (Bowlby, 1969/1982). According to Bowlby, the primary caregiver acts as a prototype for future relationships via the IWM. There are three main features of the IWM: the extent to which (1) others are represented as being trustworthy, (2) the self is represented as valuable, and (3) the self is represented as effective when interacting with others. Although IWMs are mental representations of the self and others in close relationships, they are based on and informed by the child's real-life interactions with attachment figures.

It has been proposed that secure attachment aids the child's construction of coherent and organized mental representations of the caregiver–child relationship that can be used effectively to predict the caregiver's behavior (Ontai & Thompson, 2008). This ability, in turn, is argued to provide children with the competence to engage in so-called 'goal-corrected partnerships' (Bowlby, 1969/1982). In these partnerships children apply their awareness of the caregiver's needs to align their own goals with those of the caregiver. Attachment relationships are thus assumed to offer children channels through which they can attend to and use mental representations of others to guide behavior. Considering this view, these processes occurring during the developing attachment relationship closely relate to the processes involved in mentalizing: using information related to beliefs, emotions, and intentions to make sense of behavior. A secure attachment may enhance children's sensitivity to internal states first within caregiver–child interactions, and subsequently within interactions with others (Ontai & Thompson, 2008). Secure attachment may therefore be expected to relate to superior performance on tasks assessing mentalizing abilities.

More recent views on attachment argue that the influence of attachment goes beyond the development of a set of social expectancies and also encompasses physiological stress reactions and brain regulation (Cozolino, 2014; Feldman, 2017; Fonagy, 2006; Sroufe, 1996). There are rodent studies indicating that early adverse attachment experiences lead to lasting changes in dopamine responsivity to stress and the corticotropin-releasing hormone involved in the stress response (e.g., Moriceau, Roth, & Sullivan, 2010; Plotsky et al., 2005; Zhang, Chrétien, Meaney, & Gratton, 2005). Since stress has substantial (negative) impact on our ability to mentalize (Luyten & Fonagy, 2015), one might hypothesize that being insecurely attached leads to suboptimal functioning of the (physiological and neurological) stress-system and thus to poorer mentalizing performance.

Empirical studies on attachment and children's FBU and EU

The theoretical notions about attachment mentioned above have led to a significant amount of research examining the relations between attachment and children's thinking-about-thinking (FBU) and thinking-about-feeling (EU). Findings have been rather consistent on the relation between attachment and EU. A previous meta-analysis on this relation (including 10 studies) showed a significant medium correlation, with the vast majority of studies reporting significant positive associations between attachment and EU ($r = 0.34$; Cooke et al., 2016). With regard to the association between attachment and FBU, the findings have been inconsistent, with studies reporting significant medium to large relations (e.g., Fonagy, Redfern, & Charman, 1997; Marchetti et al., 2014; McElwain & Volling, 2004; Meins, Fernyhough, Russell, & Clark-Carter, 1998; Meins et al., 2002; Meins, Bureau, & Fernyhough, 2018; Villachan-Lyra et al., 2015), and studies reporting null findings (e.g., Greig & Howe, 2001; Laranjo, Bernier, Meins, & Carlson, 2014; Meins et al., 2002, in preparation; Ontai & Thompson, 2008; Reese, 1998). These inconsistent findings have led researchers to speculate on whether attachment relates more strongly to thinking-about-feeling than to thinking-about-thinking (e.g., Greig & Howe, 2001).

A possible reason for this finding might be that FBU and EU tasks tap into different aspects of mentalizing (Luyten & Fonagy, 2015). The extant literature presents mixed findings on the correlation between performance of FBU and EU tasks, varying from moderate or strong correlations (e.g., Greig & Howe, 2001; $r = 0.47$, Meins et al., in preparation; $r = 0.47$ to 0.53, De Rosnay, Pons, Harris, & Morrell, 2004; $r = 0.51$) to non-significant correlations (e.g., Dunn, Brown, Slomkowski, Tesla, & Youngblade, 1991, $r = 0.25$, ns; Tarullo, Bruce, & Gunnar, 2007, ns). On the one hand, FBU requires an appreciation of other people's thoughts and how these thoughts guide people's behavior. This taps into mentalizing about cognitive features, primarily governed by conscious (controlled) processes (Shamay-Tsoory, Aharon-Peretz, & Perry, 2009). Typical EU tasks, on the other hand, require participants to recognize emotions from facial expressions and to understand the causes of emotions (Shamay-Tsoory & Aharon-Peretz, 2007; Shamay-Tsoory et al., 2009; Walter, 2012). Moreover, FBU tasks require an understanding of mental concepts that are not visible to the eye (thinking-about-thinking), whereas EU tasks require children to connect the outer (facial expressions) with the inner (emotions) world (Luyten & Fonagy, 2015). Indeed studies show that different neural circuits underlie performance on FBU and EU tasks (Shamay-Tsoory & Aharon-Peretz, 2007; Shamay-Tsoory et al., 2009; Walter, 2012). For instance, two studies have shown that patients with lesions in the ventromedial prefrontal cortex appear to be impaired in FBU tasks, whereas patients with lesions in the inferior frontal gyrus appear to perform worse on EU tasks (Shamay-Tsoory & Aharon-Peretz, 2007; Shamay-Tsoory et al., 2009). In other words, the neuropsychological structures underlying FBU and EU task performance seem to differ and it has been argued that separate mechanisms for the interpersonal understanding concerning emotions and belief states should be considered (Fonagy et al., 2002; Fonagy, Bateman, & Luyten, 2012).

For the abovementioned reasons, it seems interesting to test whether children's recognition and understanding of emotions and false beliefs associates differently with child-parent attachment security. So far, however, meta-analytic data on attachment and FBU are lacking, as are meta-analytic data on whether attachment explains more of the variance in FBU or EU. The first aim of the present study was to calculate the pooled correlations between attachment and FBU and attachment and EU and explore whether the pooled correlations differed significantly from each other in magnitude.

Attachment and mentalizing abilities: a direct link?

Apart from testing theoretical predictions about the association between attachment and mentalizing, meta-analyses provide an opportunity to test methodological concerns. Given that some studies present null-findings on the association between attachment and measures of mentalizing abilities, it has been suggested that the pathway connecting the two is indirect (e.g., Cooke et al., 2016; Sharp & Fonagy, 2008). That is, there are multiple correlates of attachment that also relate to children's FBU and EU, and which might explain the attachment–mentalizing relation (e.g., children's language ability; parents' use of mental state language; Cooke et al., 2016; Devine & Hughes, 2016; van Ijzendoorn, Dijkstra, & Bus, 1995; Meins et al., 2002; Milligan et al., 2007; Zeegers, Colonnesi, Stams, & Meins, 2017). In addition, the nature of the attachment assessment may influence any association between attachment and mentalizing abilities. The second goal of this review was to get a better understanding of the direct or indirect nature of the attachment–mentalizing relation, by explore the potential mediating effect of language ability as well as the moderating effect of the method of assessing attachment.

Language ability. Mentalizing ability tasks often involve verbal stories and require verbal answers. A large number of studies document a medium to strong correlation between children's verbal ability and performance on FBU or EU tasks (e.g., FBU: Laranjo, Bernier, Meins, & Carlson, 2014; Meins et al., 2002; Oppenheim et al., 2005; EU: Altamura, 2010; Greig & Howe, 2001; Meins et al., in preparation; Repacholi & Trapolini, 2004). A comprehensive meta-analysis on FBU and language ability ($k = 104$) has shown a medium to large effect of language on FBU (Milligan et al., 2007). Studies in which the relation between attachment and children's mentalizing abilities was examined therefore often included language ability as a control variable.

The secure infant–parent attachment system also seems to relate to language ability. Positive associations between attachment and verbal ability have been reported in a number of studies, including an exploratory meta-analysis (e.g., Altamura, 2010; Greig & Howe, 2001; van IJzendoorn, Dijkstra, & Bus, 1995; Meins, 1997; Meins et al., 2018). Multiple hypotheses for the attachment–language relation have been posed. For instance, it is thought that the types of infant–parent interaction that promote attachment security lead to interactions that are optimally suited for 'stretching' the child's linguistic capabilities (Meins, 1997). van IJzendoorn, Dijkstra, & Bus (1995) mention that since secure infants feel free to explore their environment, they are open to derive insights and skills from new environments, rather than being focused on attachment-related environmental cues. The positive associations found between attachment and verbal ability on the one hand, and verbal ability and mentalizing task performance on the other hand, indicate that language may mediate the association between attachment and children's mentalizing abilities. We studied this question by examining whether the pooled correlation between attachment and mentalizing abilities was mediated by language ability.

Measures of attachment. Other than the possible third factors that might explain the association between attachment and mentalizing abilities, the previous inconsistent findings may also stem from the possibility that the type of measure used to assess attachment is a confounding factor. Table 1 provides an overview of the different types of attachment assessment procedures that have been used in past studies on the development of mentalizing abilities. Studies assessing attachment and mentalizing abilities concurrently typically assess attachment using a representational measure rather than behavioral measures like the strange situation and the Attachment Q Sort (Ainsworth et al., 1978; Waters & Deane, 1985). Representational measures involve activating the child's attachment system by presenting emotionally provocative situations as photographs or pictures (Separation Anxiety Test; SAT; Klagsbrun & Bowlby, 1976) or as the beginning of stories (Attachment Story Completion Task; ASCT; Bretherton, Ridgeway, & Cassidy, 1990). These representational measures of attachment define security in terms of the extent to which children (a) feel comfortable with minor separations but react negatively to major separations (SAT), or (b) use attachment figures to assuage the story character's distress (ASCT). However, these representational measures also require an understanding of other minds, given that responses on these tasks require the child to represent the internal states of others (Thompson, 1998, 2008). This lack of independence between representational assessments of attachment and mentalizing abilities may thus inflate their association.

Interpreting the findings of studies investigating links between representational assessments of attachment and children's mentalizing abilities is further complicated by the fact that there is questionable support for the assumption that the different types of attachment assessment tap into the same construct. In the largest study on this topic ($N = 90$), Bar-Haim, Sutton, Fox, and Marvin (2000) reported no associations (p-levels > 0.30) between infant attachment security as assessed using the strange situation and performance on the SAT at 58 months. Similarly, Trapolini, Ungerer, and McMahon (2007) found no longitudinal associations between infant strange situation classification and ASCT performance at age 4. Of the two studies to report impressive longitudinal associations, one used a small and highly selective sample (65% insecure; $r = 0.76$, $p < .001$; Main, Kaplan, & Cassidy, 1985), and the other assessed attachment representations using a newly-devised doll play procedure in a sample of only 27 children (kappa = 0.67, $p < .001$; Gloger-Tippelt, Gomille, Koenig, & Vetter, 2002).

The findings described above are important for the present review, since previous studies on attachment and children's mentalizing abilities used a wide variety of attachment instruments. If representational attachment scores depend on the child's FBU and EU, we would expect to find a significant pooled correlation for studies using representational assessments, whereas we would expect to find a weaker association for studies using behavioral measures of attachment. We studied this question by examining to what extent the use of behavioral or representational attachment assessments, as well as the different attachment instruments, moderated the pooled correlation between attachment and children's mentalizing abilities.

The present study

In sum, the present study adds to existing research by examining the following research questions: (a) what is the overall relation between attachment and children's mentalizing abilities, as indicated by FBU and EU; (b) are there differences in the magnitude of

Table 1
Description of Attachment Instruments.

Instrument	Author(s)	Age range	B or R[1]	Assessment procedure and scoring
Strange Situation Procedure (SSP)	Ainsworth et al., 1978	12 to 18 months	B	20-minute observational lab procedure in which the child has encounters with an unfamiliar woman and experiences two caregiver separation episodes. Attachment behaviors are classified (insecure-avoidant (Type A), secure (Type B) or insecure-resistant (Type C)) on the basis of the child's behaviors during the reunion with the caregiver (proximity seeking, contact maintenance, avoidance, resistance, settling down from distress, continuing regular activities).
Preschool Assessment of Attachment (PAA)	Crittenden, 2004	18 months to 5 years	B	Maturational expansion of the infant SSP including similar episodes and accommodates children's ability to walk, talk, and open doors. Classification of behavior yields three types of attachment: defended (Type A), Secure/balanced (Type B), and coercive (Type C).
Preschool Attachment Classification System (PACS)	(Cassidy and Marvin, 1992)	3 to 6 years	B	Maturational expansion of the infant SSP, including two five-minute separations from the caregiver and reunion. Classification yields the following types of attachment: Secure, Insecure-Avoidant, Insecure-Ambivalent, Controlling-Caregiving, Controlling-Punitive, Controlling-General, and Behaviorally-Disorganized.
Attachment Q Sort (AQS)	Waters & Deane, 1985	1 to 5 years	B	Child and parent are observed in a routine situation at home by a trained observer. The observer sorts 90 cards with statements about the child's secure-base behavior, dependency, affectivity, social interaction, object manipulation, and social perceptiveness into nine piles ranging from "most descriptive of this child" to "least descriptive of this child." An overall security score is calculated by correlating the sort for each child with a criterion sort (prototypical behaviors of a securely attached child). The overall score for each child results in a variable ranging from + 1.0 (i.e., very secure) to −1.0 (i.e., very insecure).
Attachment Story Completion Task (ASCT)	Bretherton et al., 1990	3 to 9 years (requires modification when used with older children)	R	The child participates in four story-stems on attachment themes using five dolls (mother, father, grandmother, child, sibling): physical injury (hurt knee), fear (nightmare), separation (parents are going away for the night), and reunion (parents return the next morning). Children are asked to show and tell the experimenter what happens after the story-stem. Children's stories are coded for the story-telling process (signs of avoidance of negative feelings) and content (resolution and coherence).
Separation Anxiety Test (SAT)	Klagsbrun & Bowlby, 1976	4 to 7 years	R	Children are interviewed after two picture stories about a character's mild and major separation from his parents. In the interview the child is asked three questions about the character's feelings and actions, and the child is asked to imagine what he or she would feel/do in the same situation. Answers are assigned points (21 in total) on self-reliance, attachment and avoidance.
Manchester Child Attachment Story Task (MCAST)	Green, Stanley, Smith, & Goldwyn, 2000	4 to 8.5 years	R	Child is introduced to play materials including a doll house, and is asked to choose a doll that represents him or her and the caregiver of interest. There are 5 attachment-related 'distress' vignettes. In each one the child is placed in a situation of specific distress with the caregiver close by but not proximate. The stories' coding scales fall into four broad dimensions (attachment-related behavior, narrative coherence, disorganized phenomena, mentalization skills) and these dimensions are scored through 21 rating scales (most of them ranging from 1 to 9).

Age range = appropriate to use within this range of the child's age.
[1] B = behavioral measure of attachment, R = Representational measure of attachment.

correlations between attachment and FBU on the one hand, and attachment and EU on the other hand; (c) does children's language ability mediate the relation between attachment and children's FBU and EU; (d) is the relation between attachment and children's mentalization moderated by the attachment assessment approach (behavioral vs. representational) and/or instrument? Questions (a) and (b) attempt to answer whether there is a significant link between attachment and children's mentalizing abilities. Questions (c) and (d) attempt to gain more insight into the direct or indirect nature of any association.

Methods

Selection of studies

The following electronic databases were searched until April 2018 for articles, book chapters, dissertations, and reports on attachment and FBU and/or EU: Web of Science, PsycINFO, Google Scholar, Medline, Embase, Cochrane Library, and ERIC. The most relevant combination of components that we used were consistent with other meta-analyses on attachment or mentalizing capacity (Cooke et al., 2016; Devine & Hughes, 2016; Milligan et al., 2007; Luke & Banerjee, 2013; Wellman et al., 2001; Zeegers et al., 2017): attachment AND "theory of mind" OR mentali*ing OR "social cognition" OR "social understanding" OR "emotion understanding" OR "emotion recognition" OR "perspective taking" OR "false belief" OR "belief-desire reasoning" OR "understanding mental states" OR "mental representations". The search yielded 3585 results. The titles and abstracts of the articles we gathered from this search were screened, and full article texts of possibly relevant studies were checked. The reference sections of the articles identified through these searches were examined for any other relevant studies. The reference list of a meta-analysis on the relation between attachment and EU was also checked (Cooke et al., 2016). Relevant researchers in the field were contacted by email with a request for any unpublished manuscripts or data that met the inclusion criteria described below. A PRISMA flow diagram, including the number of documents identified, included and excluded, is presented in Appendix A of the online supplementary materials.

Eligibility criteria

Studies were eligible for inclusion in the meta-analysis if they examined the relation between attachment and FBU and/or EU. The assessments of FBU and EU had to be made when a child was aged between 36 and 72 months. This time span was chosen because children are not likely to pass false belief tasks before the age of 3 years, while almost all children are likely to pass false belief tasks after age 6. Performances on EU tasks tend to vary more, even after the age of 6 years. However, because we aimed to compare the magnitude of the pooled correlations between attachment and children's EU and FBU respectively, we kept the age during task assessments equal among studies. On a similar note, the average age during the assessments of attachment could not exceed 72 months. We aimed to keep the overall sample in this meta-analysis as homogeneous as possible in order to prevent excessive moderator analyses including different types of sample characteristics (e.g., parents having a mental disorder diagnosis or not, biological vs. adopted or foster children). Studies were therefore only included when participants came from a typically developing population. We excluded samples of children who had been diagnosed with a developmental disorder or who had been exposed to maltreatment. We also excluded adoptive or foster samples. We did include samples of socio-economically disadvantaged children (i.e., Raikes & Thompson, 2006, 2008).

We included studies in which attachment was assessed using one of the following standardized procedures: (a) behavioral measures: strange situation procedure (SSP; Ainsworth et al., 1978), Preschool Assessment of Attachment (PAA; Crittenden, 2004), Preschool Attachment Classification System (PACS; (Cassidy and Marvin, 1992)), Attachment Q Sort (AQS; Waters & Deane, 1985), and (b) representational measures: Attachment Story Completion Task (ASCT; Bretherton et al., 1990), Separation Anxiety Test (SAT; Klagsbrun & Bowlby, 1976), Manchester Child Attachment Story Task (MCAST; Green, Stanley, Smith, & Goldwyn, 2000).

With regard to the assessments of children's mentalizing abilities, studies had to assess either FBU or EU. Studies that employed classic false belief tasks or revised versions of classic false belief tasks were eligible: unexpected location tasks (Baron-Cohen et al., 1985; Wimmer & Perner, 1983), unexpected content tasks (Perner, Leekam, & Wimmer, 1987), appearance–reality tasks, (Flavell, Flavell, & Green, 1983), and belief–desire reasoning tasks (Harris, Johnson, Hutton, Andrews, & Cooke, 1989).

With regard to assessments of EU, we included studies that used Denham's task (1986), which is a standard measure of EU in children aged 3–6 years. This task measures the child's ability to label emotions correctly and to understand which emotion is most likely to be felt in a given situation (e.g., feeling happy when receiving a present). Studies in which an alternative method to assess the EU abilities described above were also included, but only if they assessed the EU abilities that are also targeted in Denham's task (i.e., labelling emotions and understanding the context of emotions; e.g., Kidwell et al., 2010; Repacholi & Trapolini, 2004). For instance, we included a study that used the 'causes of emotion' interview developed by Dunn and Hughes (1998). In this interview children are asked to label emotional expressions and to describe 'what kind of things make you feel X'. Similar to Denham's task, the questions in this interview entail that the child must be able to understand contextual causes of emotions. The other EU measures that we included also involved the child's understanding of emotions given a certain context.

Calculating effect sizes

A total of 17 unique samples reported on the association between attachment and FBU, while a total of 15 samples reported on the association between attachment and EU (9 of the attachment–EU studies were also included in the meta-analysis of Cooke et al., 2016). An overview of all studies that were included, the characteristics of each study, and effect sizes are presented in Tables 2 and

3. Effect sizes were reflected in Pearson's r correlation coefficients. If necessary, statistics were converted into correlational scores using the converter of Wilson and Mason (www.campbellcollaboration.org). This converter is based on the formulae of Lipsey and Wilson (2001). Prior to the main analyses, the correlations were Fisher's z transformed, in order to approximate a normal sampling distribution (Lipsey & Wilson, 2001). If necessary, authors were contacted to provide raw correlations, when correlations were 'controlled' for the effects of other variables. A few studies assessed FBU with multiple tasks (e.g., unexpected location and unexpected content task/appearance–reality task; e.g., Meins et al., 2002; Villachan-Lyra et al., 2015). Authors were also contacted to provide the correlations between attachment and individual false-belief tasks when only the association between attachment and a false-belief or EU sum score was reported, or the raw correlations were not reported in the paper (e.g., Marchetti et al., 2014; Meins et al., 2002; Ontai & Thompson, 2008; Oppenheim et al., 2005).

Where a study employed the SSP to assess attachment security, we calculated Pearson's *r* representing the difference in performance on a social cognition task in secure versus insecure groups (i.e., we made no attempt to investigate differences in social cognition using the four category classification system). Where a study employed the SAT to assess attachment security, we extracted the correlation between mentalizing task performance and overall SAT security scores on the SAT (which are calculated by totaling the self-reliance and attachment scores and subtracting the inverse avoidance scores; Fonagy et al., 1997).

The correlation between FBU and the total SAT score was not available for one study (Repacholi & Trapolini, 2004). We therefore calculated the effect size of this study based on the mean correlation between FBU and the three dimensions (i.e., security, self-reliance, and reversed avoidance). Where a study employed the ASCT to assess attachment security, and classified participants as either securely or insecurely attached (as in Greig & Howe, 2001; Arranz, Artamendi, Olabarrieta, & Martín, 2002), we calculated an effect size representing the difference in mentalizing performance between these groups. One study (Vaughn et al., 2011) formed a composite measure of ASCT performance and used this as their measure of attachment security. For this study, we extracted the correlation between scores on that composite dimension and EU. Two studies (Altamura, 2010; Meins et al., in preparation) reported participants' scores on multiple measures (e.g., resolution, coherence, security) of ASCT performance. In both cases, we calculated a mean correlation coefficient representing the mean association between the ASCT dimension scores and FBU or EU.

Statistical approach

We investigated the research questions using a three-level approach to meta-analysis (Hox, 2002; Raudenbush & Bryk, 1985; van den Noortgate & Onghena, 2003) and meta-analytic structural equation modelling (MA-SEM; Cheung, 2008; 2015; Jak, 2015). The multilevel approach also allowed us to include studies that reported multiple correlations between attachment and children's mentalizing abilities (e.g., when a single study reports correlations between attachment and both emotion and false understanding, or when a single study reports longitudinal and cross-sectional correlations). With this approach we were able to test whether the pooled attachment–FBU correlation was different in magnitude from the pooled attachment–EU correlation. The MA-SEM approach was used to study whether language ability mediates the relation between attachment and EU and/or FBU (Jak, 2015).

In conventional meta-analytic strategies only one effect size per study is considered, either by averaging or eliminating effect sizes that were documented (i.e., the fixed effects model or two-level random effects model; Raudenbush, 2009). This means that information on differences between effect sizes within a single study is lost. As a result, there is lower statistical power (Assink & Wibbelink, 2016; Cheung, 2015). This approach also limits the research questions that can be addressed, as the impact of sampling dissimilarities, methods and designs cannot be examined accurately. In a three-level random effects model, three sources of variance are modeled: (a) variation in effect sizes due to random sampling of effect sizes (Level 1); (b) variation in effect sizes due to differences within a single study (Level 2); and (c) variation in effect sizes between different studies (Level 3; Cheung, 2015). The three-level approach additionally considers the dependency of effect sizes reported in a single study. This approach thus allows for the inclusion of multiple effect sizes per study. More than one relevant effect size was reported in 53% and 33% of the studies on the relation between attachment–FBU and attachment–EU, respectively. Multiple effect sizes per study were coded when: (a) associations between the constructs were assessed at multiple time points (e.g., Meins et al., in preparation; Symons & Clark, 2000); (b) single constructs were assessed with different instruments or tasks (e.g., Meins et al., in preparation; Ontai & Thompson, 2008, 2002; Reese, 1998), and (c) attachment was assessed for child–mother and child–father dyads separately (McElwain & Volling, 2004; Steele, Steele, Croft, & Fonagy, 1999).

In order to test the mediating effect of children's verbal ability, we fitted a meta-analytic structural equation model (MA-SEM; Cheung, 2008, 2015) to the data, using the metaSEM package in R (Cheung, 2014), and following the guidelines of Jak (2015, pp. 39–56). An advantage of MA-SEM is that data from studies investigating parts of the model can be included. By using the effect sizes of all included studies within the three meta-analytic datasets, maximum use of the available data is obtained. For these analyses we did not use the multilevel approach, and converted the multilevel meta-analytic dataset to a conventional dataset (i.e., one effect size per study). This was done by averaging the effect sizes reported in one study, if necessary. When researchers reported on effect sizes for different sample sizes (e.g., Meins et al., in preparation), the sample size was considered in the calculation of the mean effect size. We tested a model in which the direct effects of attachment and verbal ability on FBU or EU were specified as well as the covariance between attachment and verbal ability. In order to test the mediating effect of verbal ability, we modeled the indirect effect of attachment security on FBU/EU through children's verbal ability. The pooled correlation between attachment and FBU/EU therefore controlled for the effects of verbal ability on FBU. Likelihood based confidence intervals were calculated to evaluate the significance of the direct and indirect path coefficient (see Jak, 2015, pp. 51–52).

Heterogeneity in effect sizes

In the case of a multilevel approach to meta-analysis, moderator analyses are beneficial in understanding the extent to which the effect sizes may be inflated or deflated because of differences between and within studies. Yet, moderator analyses are only useful when the variation in the effect sizes can be assigned not only to sampling variance (Level 1), but also to second- and third-level-variance. The suggestions of Hunter and Schmidt (1990) were followed to evaluate these variances: heterogeneity between effect sizes is substantial when < 75% of the total variance is assigned to sampling variance (Level 1). Two separate one-sided log-likelihood-ratio-tests were performed to examine whether heterogeneity in effect sizes on the second and third level was substantial (Assink & Wibbelink, 2016). These tests compare a full multilevel model with a model in which one of the variance parameters is excluded. Cheung (2014) reported formulae which we used to estimate the portion of variances that could be ascribed to the separate variance levels.

Moderators. There were several probable moderators that could explain variation in effect sizes between and within studies: type of attachment assessment (behavioral or representational), cross-sectional or longitudinal design, age of the child during the FBU or EU assessment, type of FBU task, and type of EU task. The main aim of the moderator analyses was to examine the effect of the attachment assessment approach on the relation between secure attachment and children's mentalizing abilities. Behavioral measures of attachment were most often used during infancy, and therefore more often in longitudinal studies, whereas representational measures of attachment were only used during the preschool years, and are thus more common in cross-sectional studies. We therefore performed several moderator analyses to examine the effect of the attachment approach, taking into account the study design.

First, we examined the moderating influence of all behavioral assessment approaches versus representational assessment approaches, without taking into account children's age during the assessment. We thus included studies using the AQS and SSP during infancy and preschool. Second, we examined the moderating influence of the behavioral assessments during infancy only (< 36 months) versus the representational measures. Third, we checked the moderating influence of the behavioral assessment during childhood (> 36 months) only versus the representational measures. In this way, we were able to gain insight into whether the type of assessment affected the pooled correlation, taking into account the influence of the study design (cross-sectional or longitudinal).

Second, age during the FBU or EU assessment could be a moderating factor since older children are more likely to pass or do well on FBU or EU tasks. The resulting reduction in variance in older children's mentalizing abilities could lead to smaller effect sizes. Lastly, we performed moderator analyses on the type of mentalizing tasks for another reason: It has been argued that the different types of FBU tasks appeal to different aspects of mentalizing (Villachan-Lyra et al., 2015), which have relatively distinct underlying neural circuits (Luyten & Fonagy, 2015). The unexpected location task asks children to explain others' (searching) behaviors based on their beliefs (i.e., mentalizing based on external features; medial frontoparietal network), whereas the unexpected identity/content task asks children to understand what someone else might (wrongly) think about an object (mentalizing based on internal features; lateral frontotemporoparietal network; Luyten & Fonagy, 2015; Villachan-Lyra et al., 2015). Hence, it is worthwhile to explore whether attachment (which is hypothesized to influence brain regulation; Cozolino, 2014; Fonagy, 2006) associates differently with the two tasks. We therefore examined whether studies using the unexpected location task (Sally-Ann task or a similar task) had different effect sizes than studies using the unexpected identity/content tasks (understanding false beliefs about an object that is different than it first appears, or has a different content than expected).

Based on similar reasoning, we examined whether the pooled correlation between attachment and FBU was different for studies using the belief–desire reasoning task (e.g., Fonagy et al., 1997) or either one of the other false belief tasks (i.e., unexpected location or unexpected content/identity), since the belief-desire reasoning task does not concern the understanding of mistaken beliefs. Last, we performed moderator analyses to check whether the type of EU task used (i.e., binary variable: Denham's puppet task or not) moderated the pooled correlation on attachment and EU, since our inclusion criteria of studies using EU tasks other than the Denham task could have an effect on the pooled association.

Interrater agreement. All studies were coded by the first author and the second author coded a randomly selected 20% of the studies (k = 10). Agreement was evaluated by assessing absolute agreement (agree or disagree). Cohen's kappas were calculated to evaluate the interrater agreement among the moderator variables (Fleiss & Cohen, 1973). There was full agreement for the number of participants, child age during the assessment, percentage boys, and type of measurement instrument. Kappa was excellent for SES (ICC 0.90) and effect size (0.90).

Statistical analyses

The statistical analyses were performed using the guidelines of Assink and Wibbelink (2016). The function "rma.mv" of the metafor package was used in the software environment R (version 3.2.2; R Core Team, 2015; Viechtbauer, 2010). We calculated restricted maximum likelihood estimates, as full maximum likelihood estimates seem to show a more downward bias, especially when the number of studies in the meta-analysis is relatively small (e.g., Thompson & Sharp, 1999; Turner, Omar, Yang, Goldstein, & Thompson, 2000). Individual regression coefficients and corresponding confidence intervals for the models were calculated using the t-distribution (Knapp & Hartung, 2003). The omnibus tests (for the moderator effects) followed an F-distribution (see Assink & Wibbelink, 2016 for a more elaborate explanation of the test distributions).

Publication bias

We investigated publication bias by examining the funnel plots with all effect sizes regressed against their standard errors. This gives a clue to which studies are missing (e.g., studies with small sample sizes reporting small effect sizes) or overrepresented (e.g., studies with small sample sizes reporting large effect sizes), and therefore possible publication selection bias. We report on both the funnel plot based on the multilevel dataset, as well as the funnel plot based on the conventional dataset (i.e., 1 effect size per study).

Second, we used the PET-PEESE approach to further explore publication bias (Stanley & Coucouliagos, 2014). In the PET-PEESE approach, the effect size is first regressed on the standard error of the effect size in a weighted least squares (WLS) regression with the standard error of the effect size as the weight. Testing if the slope of the regression line is statistically significant serves as a test for statistically significant publication bias. The typical relation when publication bias is present is that higher standard errors are associated with larger effect sizes. The intercept of this regression is interpreted as an estimate of the effect in a hypothetical study of no error ($SE = 0$) and therefore no bias. Testing whether the intercept is statistically significant serves as a test of whether there is a true, bias-corrected effect different from 0. This test is called the precision-effect test (PET). In the simulation study by Stanley and Doucouliagos (2014), PET performed well when the true effect in the meta-analyzed studies was 0, with the intercept being a slight overestimation of the true effect. When there was a true effect, however, using the variance as the predictor in the regression showed better performance. Here the intercept was a slight underestimation of the true effect. This test is called a precision-effect test with standard error (PEESE). The authors thus suggested that PET is followed up by PEESE if PET shows a true, bias-corrected effect. However, it should be noted that the precision-effect test (PET) sometimes has low power in identifying a genuine nonzero effect when there are only 10 or 20 estimates available in an area of research (Stanley, 2017). We therefore examined both the PET and PEESE results to get an idea of the publication bias. The PET-PEESE approach has shown to outperform other methods used to detect publication bias, such as the Fail Safe Number analysis and the Trim and Fill procedure (Stanley & Doucouliagos, 2014). Because there are no studies yet on the use of PET-PEESE with multilevel datasets, we used the conventional dataset (i.e., one effect size per study) that we also used for the MASEM analyses.

Results

Preliminary analyses

Heterogeneity in effect sizes. Before investigating our research questions, we examined whether effect size differences could be assigned to random sampling error (Level 1), within-study variance (Level 2), or between-study variance (Level 3). That is, only if substantial heterogeneity between or within the effect sizes of studies is present, it is useful to further look at the impact of moderators that might explain the heterogeneity (Assink & Wibbelink, 2016). For the association between attachment and FBU, within-study variance was non-significant, $\hat{\sigma}^2 < 0.002$, χ^2 (1) = 0.400, $p = .540$, whereas between study-variance was significant, $\hat{\sigma}^2 < 0.022$, χ^2 (1) = 6.936, $p = .008$. Of the total variance, 6.02% was explained by variation within studies, 56.62% by variation between studies, and 37.36% by random sampling variance. For the association between attachment and EU, within-study variance was significant, $\hat{\sigma}^2 < 0.0012$, χ^2 (1) = 7.646, $p = .006$, as well as between study-variance, $\hat{\sigma}^2 < 0.010$, χ^2 (1) = 3.816, $p = .051$; one-sided; Assink & Wibbelink, 2016). Of the total variance, 35.51% was explained by variation within studies, 32.12% by variation between studies, and 32.37% by random sampling variance. The outcomes described above point to substantial heterogeneity between studies in the attachment–FBU dataset, and substantial heterogeneity within and between studies in the attachment–EU dataset.

Main results

Pooled correlations between attachment and FBU and EU. After the analyses, correlations were transformed to Pearson's r coefficients, to which we refer in the text. With regard to the association between attachment security and FBU, the analyses were based on 36 effect sizes from 1029 unique children (within 17 samples). The sizes of the study samples varied from 20 (Villachan-Lyra et al., 2015) to 163 (Meins et al., in preparation). Attachment showed an overall significant positive association with FBU, $r = 0.19$, 95% CI [0.10, 0.28].

With regard to the association between attachment security and EU, the analyses were based on 28 effect sizes from 912 unique children (within 15 samples). The sizes of the study samples varied from 29 (Ontai et al., 2005) to 160 (Meins et al., in preparation). Attachment showed an overall significant positive association with EU, $r = 0.31$, 95% CI [0.23, 0.41].

Comparing FBU and EU in relation to attachment. In order to compare whether attachment showed a significantly larger association with EU, we created a new multilevel dataset with all effect sizes on the attachment–FBU and the attachment–EU relation, and tested whether the effect sizes differed substantially from each other in moderator analyses. The multilevel moderator analyses allowed us to include studies that reported an effect size on both the attachment–FBU relation and the attachment–EU relation. First, the analyses showed that the pooled correlation for the overall relation between attachment and children's mentalizing abilities was $r = 0.25$, 95% CI [0.19, 0.31]. Second, the pooled correlation for the relation between attachment and FBU was significantly lower than the pooled correlation for attachment and EU, $F(1, 62) = 4.24$, $\beta = -0.10$, $p = .025$, 95% CI [-0.19, -0.01].

The mediating role of language ability. The results of the MA-SEM are displayed in Figs. 1a and 1b, presenting the effect of a predictor, after controlling for the effect of the other predictor in the model. With regard to the prediction of FBU, 10 out of 17 studies reported on correlations between verbal ability and FBU or attachment. The MA-SEM analyses showed a significant direct effect of

Table 2
Characteristics of the Included Studies on Attachment and False Belief Understanding.

Author	Year	*N*	Child age attachment (months)	Attachment	Child age FB task (months)	FB task	*r*
Arranz et al.	2002	90	44.6	ASCT	44.6	Unexpected transfer	0.23
De Rosnay & Harris	2002	51	60.9	SAT	60.9	Desire-belief reasoning	0.37
Fonagy et al.	1997	77	58	SAT	58	Desire-belief reasoning	0.37
Greig & Howe[a]	2001	45	40	ASCT	40	Unexpected identity/content	0.09
Laranjo et al.	2010/2014	59	15.6	AQS - observer	48.9	Unexpected identity	-0.13
Marchetti et al.*	2014	30	76.4	SAT	76.4	Unexpected transfer	0.19
McElwain & Volling	2004	32	12	SSP	51	Unexpected transfer	0.30/0.50
Meins et al.	1998	30	12	SSP	49/61.5	Unexpected transfer; Desire-belief reasoning	0.51/0.23
Meins et al. *	2002	52	12	SSP	45/48	Unexpected transfer; Unexpected identity/ content	0.26; -0.19
Meins et al.	In prep	160	15;44;51	SSP; PAA; ASCT; SAT	51; 61	Unexpected transfer; Unexpected identity/ content	0.01; 0.02; 0.08; 0.06; 0.09; 0.16
Moore & Symons	2005	48	48	AQS	42	Unexpected transfer; Unexpected identity/ content	0.32
Ontai & Thompson *	2008	78	54	AQS	54	Unexpected transfer; Desire-belief reasoning	-0.02; 0.04
Oppenheim et al.*	2005	107	12	SSP	48	Unexpected transfer	0.00
Reese (U)*	1998	48	19	AQS	51	Unexpected transfer; unexpected identity/content	-0.21; 0.00
Repacholi & Trapolini	2004	48	54	SAT	54	Unexpected identity	0.22
Symons & Clark	2000	46	25/70	AQS	70	Unexpected transfer; unexpected identity/content	0.02; 0.06/0.23; 0.30
Villachan-Lyra et al.	2015	20/20	36/48	ASCT	36/48	Unexpected transfer; unexpected identity/content	0.42; 0.43;/0.44; 0.45

Note. ASCT = Attachment Story Completion Task; AQS = Attachment Q-Sort; SAT = Separation Anxiety Test; SSP = Strange Situation Procedure; PAA = Preschool Assessment of Attachment.
*Additional information provided by authors [a] effect size calculated on the basis of insecure/secure group differences.

Table 3
Characteristics of the Included Studies on Attachment and Emotion Understanding.

Author	Year	*N*	Child age attachment (months)	Attachment assessment	Child age EU (months)	EU task	*r*
Altamura (U)	2010	58	49	ASCT	49	Emotion labelling and understanding (Denham, 1986)	0.45
Greig & Howe[a]	2001	45	40	ASCT	40	Emotion labelling and understanding (Denham, 1986)	0.52
Kidwell et al.	2010	54	54	PAA	54	Emotion labelling and understanding (positive and negative; Saarni, 1999)	0.46
Laible & Thompson	1998	40	50	AQS – mother	50	Emotion labelling and understanding (Denham, 1986 + naturalistic interview)	0.39
Laible et al.*	2004	51	49	AQS – mother	49	Emotion labelling and understanding (Denham, 1986)	0.50
Laible et al.*	2011	50	51	AQS – mother	51	Emotion labelling and understanding (Denham, 1986)	0.00
McQuaid et al.	2008	33	57	PAA	57	Emotion understanding; scenario played by actors; child narratives scored on expressions of emotion understanding	0.34
Meins et al.	In prep	160	15; 44; 51	SSP; PAA; ASCT; SAT	51; 61	Emotion labelling and understanding (Denham, 1986) + Test of Emotion Comprehension (Pons, Harris, & de Rosnay, 2004)	0.07; -0.02; 0.24; 0.19; 0.33; 0.23; 0.11; -0.04
Ontai et al	2002	50	41	AQS – mother	41	Emotion labelling and understanding (Denham, 1986)	0.12
Psychogiou et al. *	2018	105; 81	68	Manchester Child Attachment Story task	68	Test of Emotion Comprehension (Pons & Harris, 2000)	0.30; 0.32
Raikes & Thompson	2006; 2008	42	28	AQS – observer	42	Emotion labelling and understanding (Denham, 1986)	0.44
Repacholi & Trapolini	2004	48	54	SAT	54	Causes of Emotion interview (Dunn & Hughes, 1998)	0.30
Steele et al.	1999; 2002; 2008	60;63	12;18	SSP	71	Emotion labelling and understanding (The Affect Task; Croft, 1997)	0.41; -0.03
Vaughn et al.	2011	39	45	ASCT	45	Emotion labelling and understanding (adapted version; Denham, 1986)	0.46
Waters et al.	2010	72	54	AQS - mother	54	Understanding of negative emotions (Denham, 1986)	0.10

Note. ASCT = Attachment Story Completion Task; AQS = Attachment Q-Sort; SAT = Separation Anxiety Test; SSP = Strange Situation Procedure; PAA = Preschool Assessment of Attachment.

[a] Effect size calculated on the basis of insecure/secure group differences.

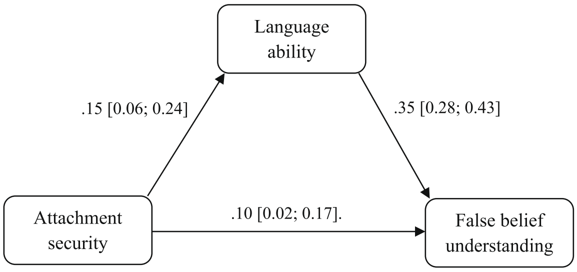

Fig. 1a. Results of the meta-analytic structural equation model with direct and indirect effects of attachment security on false belief understanding. The indirect effect of attachment on false belief understanding through language ability was r = 0.05 [0.02, .09].

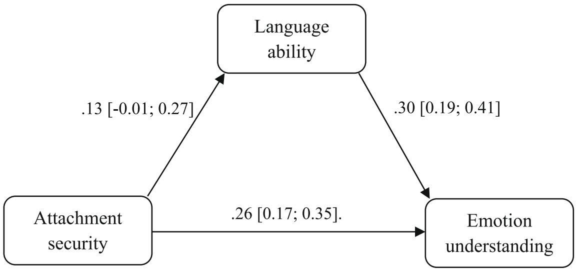

Fig. 1b. Results of the meta-analytic structural equation model with direct and indirect effects of attachment security on emotion understanding. The indirect effect of attachment on emotion understanding through language ability was r = 0.04 [−.00, .09].

attachment on FBU, r = 0.10, 95% CI [0.02, 0.17]. The direct effect of language on FBU was r = 0.35, 95% CI [0.28, 0.43], and the effect of attachment on verbal ability was r = 0.15, 95% CI [0.06; 0.24]. The total amount of variance in FBU explained by attachment and verbal ability was 15%. Further, 2% of the variation in verbal ability was explained by attachment security. The indirect effect of child–parent attachment security on FBU via verbal ability was significant, r = 0.05, 95% CI [0.02, 0.09]. Because the direct effects of attachment and verbal ability on FBU were also significant, the indirect effect indicates that the relation between attachment and FBU is partially mediated by verbal ability (Rucker, Preacher, Tormala, & Petty, 2011).

With regard to the prediction of EU, 8 out of 15 studies reported on correlations between verbal ability and EU or attachment. The MA-SEM analyses showed a significant effect of attachment on EU, r = 0.26, 95% CI [0.17; 0.35]. The direct effect of language on EU was also shown to be significant, r = 0.30, 95% CI [0.19; 0.41], while the effect of attachment on verbal ability in this model was nonsignificant, r = 0.13, 95% CI [−0.01; 0.27]. The total amount of variance in EU explained by attachment and verbal ability was 18%. Further, 2% of the variation in verbal ability was explained by attachment security. The indirect effect of child–parent attachment security on EU via verbal ability was not significant, r = 0.04, 95% CI [−0.00, 0.09]. The relation between attachment security and EU was thus not mediated by children's verbal ability.

The moderating effect of attachment assessment approach

The results of the moderator analyses including the attachment assessment approach are reported in Table 4, including Fisher's Z estimates. For ease of interpretation we report on r correlations in text. When attachment was measured with a behavioral instrument (k = 8), the pooled correlation between attachment and FBU was significantly lower than when attachment was measured with a representational instrument (k = 10), $F(1, 34)$ = 10.19, β = −0.15, p = .003. The estimated correlation between behavioral attachment and FBU was r = 0.13, p = . 006, whereas the estimated correlation between representational attachment and FBU was r = 0.28, $p < .001$. We also found moderating effects when only behavioral assessments during infancy or during childhood were taken into account, suggesting that, overall, the association between behavioral measures of attachment and FBU performance was significantly smaller (although still significant) than the association between representational measures of attachment and FBU performace (see Table 4).

Studies using the SSP and AQS showed the lowest estimated pooled correlation of r = 0.16 and r = 0.06 respectively, whereas studies using the SAT and ASCT showed higher correlations, r = 0.35 and r = 0.26 respectively. The moderating effect of the PAA/PASC on the attachment–FBU relation could not be studied because there was only one study available that used the PASC (Meins et al., in preparation). This study showed a correlation of r = 0.21 between the PASC at 4 months and FBU at 51 months, and a correlation of r = 0.14 between the PASC at 44 months and FBU at 61 months.

With regard to the correlation between attachment and EU, studies with behavioral assessment approaches (k = 10) did not report higher or lower correlations with EU than studies using representational assessments (k = 6), $F(1, 26)$ = 1.79, β = -0.09, p = .192. Also there were no moderating effects of type of assessment approach when only behavioral assessments during infancy or during childhood were taken into account. Lastly, the type of attachment instrument (SSP, AQS, ASCT, PAA/PASC/SAT) did not

Table 4
Moderator Effects of Attachment Assessment: Estimated Results (Fisher's Z, Regression Coefficients, Omnibus-Test) for Continuous and Categorical Moderator Variables (Bivariate Models).

	#studies	#ES	*Zr* (SE)	β_1 (SE)	*F* (df1, df2)$_a$	*p*-value
FBU						
Attachment assessment approach						
All					*F*(1,34) = 10.19	0.003**
Behavioral (SSP/AQS/PAA)	10	22	0.13 (0.05)**			
representational	8	14	0.28 (0.05)***	0.15 (0.05)		
< 36 months					*F*(1,27) = 9.57	0.005**
behavioral (SSP/AQS)	8	15	0.12 (0.05)*			
representational	8	14	0.28 (0.05)***	0.16 (0.06)		
> 36 months					*F*(1,19) = 5.23	0.034*
behavioral (AQS/PAA)	4	7	0.16 (0.06)*			
Representational	8	14	0.28 (0.05)***	0.13 (0.06)		
Attachment instrument[a]					*F*(3,30) = 4.14	0.014*
SSP	5	9	0.17(0.06)*			
AQS	5	11	0.06 (0.08)	−0.10 (0.09)		
SAT	4	8	0.35 (0.07)***	0.18 (0.06)*		
ASCT	5	6	0.27 (0.07)***	0.10 (0.07)		
EU						
Attachment assessment						
All					*F*(1,26) = 1.79	0.194
Behavioral (SSP/AQS/PAA)	10	18	0.28 (0.05)***			
representational	6	10	0.37 (0.05)***	0.09 (0.05)		
< 36 months (SSP/AQS)					*F*(1,13) = 1.83	0.199
behavioral	3	5	0.25 (0.09)*			
representational	6	10	0.37 (0.07)***	0.12 (0.09)		
> 36 months (AQS/PAA)					*F*(1,21) = 0.70	0.411
behavioral	8	13	0.28 (0.06)***			
Representational	6	10	0.35 (0.06)***	0.07 (0.08)		
Attachment instrument					*F*(4,21) = 1.30	0.303
SSP	2	4	0.23 (0.10)*			
AQS	6	10	0.30 (0.08)**	0.06 (0.13)		
PAA	3	4	0.41 (0.10)***	0.18 (0.11)		
SAT	4	5	0.46 (0.10)***	0.23 (0.11)		
ASCT	2	4	0.34 (0.09) ***	0.11 (0.11)		

Note. PAA = Preschool assessment of attachment; SSP = Strange situation procedure; AQS = attachment Q sort; SAT = Separation Anxiety Test.
[a] The moderating effect of the PAA on the attachment-FBU relation could not be studied since there was only 1 study available that used the PAA ([Meins et al., in preparation). This study showed a correlation of $r(128) = 0.21$ between the PAA at 44 months and FBU at 51 months, and a correlation of $r(125) = 0.14$ between the PAA at 44 months and FBU at 61 months.

moderate the relation between attachment and EU.

Moderator analyses

Attachment and FBU. Table 5 shows the results of the moderator analyses for the meta-analysis on the relation between attachment and FBU. The analyses showed that effect sizes based on cross-sectional data ($k = 10$) were larger than effect sizes based on longitudinal data ($k = 9$), $F(1, 34) = 5.84$, $\beta = 0.12$, $p = .021$. The estimated pooled correlation was $r = 0.24$, $p < .001$ for effect sizes based on cross-sectional data, whereas the pooled correlation was $r = 0.14$, $p = .009$ for effect sizes based on longitudinal data. Within the pool of longitudinal studies, effect sizes were not significantly smaller when the number of months between the attachment and false belief assessments was larger, $F(1, 18) = 1.99$, $\beta = -0.00$, $p = .176$.

Attachment and EU. Table 6 shows the results of the moderator analyses for the meta-analysis on the relation between attachment and EU. Effect sizes based on cross-sectional data tended to be larger than effect sizes based on longitudinal data, $F(1, 26) = 4.98$, $\beta = 0.14$, $p = .051$. The estimated pooled correlation was $r = 0.36$, $p < .001$ for effect sizes based on cross-sectional data ($k = 13$), and $r = 0.22$, $p = .003$ for effect sizes based on longitudinal data ($k = 4$).

Publication bias

Figs. S1 and S2 in the online supplementary material (Appendix B) display funnel plots of effect size estimates against their standard errors for both meta-analyses including the data from the multilevel analyses. Figs. 3 and 4 display funnel plots of effect size estimates against their standard errors for both meta-analyses including the data from the conventional analyses. Inspecting the plots, some asymmetry in the distribution of effect sizes was apparent. First, with regard to the relation between attachment and EU, there were relatively few studies with large sample sizes (i.e., small standard errors) reporting on large effect sizes. Second, with regard to

Table 5
Moderator Analyses Attachment and FBU: Estimated Results (Fisher's Z, Regression Coefficients, Omnibus-Test) for Continuous and Categorical Moderator Variables (Bivariate Models).

	#studies	#ES	*Zr* (SE)	β_1 (SE)	*F* (df1, df2)$_a$	*p*-value
Study characteristics						
Design					*F*(1,34) = 4.84	0.035*
longitudinal	9	20				
cross-sectional	10	16	0.14 (0.05)**	0.11 (0.05)		
Time between measurements (c)	9	20	0.25 (0.05)***	-0.01 (0.00)	*F*(1,18) = 1.75	0.202
Sample characteristics						
Age child false belief assessment (c)	17	36		-0.00 (0.00)	*F*(1,34) = 0.10	0.754
False belief assessment $_b$						
Task					*F*(2,24) = 0.09	0.915
Unexpected location	9	12	0.21 (0.07)**			
Unexpected object identity	8	10	0.18 (0.08)*	-0.03 (0.09)		
Desire-belief reasoning	5	5	0.18 (0.10)	-0.03 (0.10)		
FBU Sum score					*F*(1,34) = 0.03	0.863
yes	3	10	0.21 (0.11)			
no	14	24	0.19 (0.05)**	0.02 (0.12)		

Note. # studies = number of studies; # ES = number of effect sizes; Z *r* = Fisher's Z correlation; SE = standard error; β_1 = estimated regression coefficient; (c) = continuous variables; * p < .05; ** p < .01; *** p < .001.
a Omnibus test of all regression coefficients in the model.

Table 6
Moderator Analyses Attachment and EU: Estimated Results (Fisher's Z, Regression Coefficients, Omnibus-Test) for Continuous and Categorical Moderator Variables (Bivariate Models).

	#studies	#ES	*Zr* (SE)	β_1 (SE)	*F* (df1, df2)$_a$	*p*-value
Study characteristics						
Design					*F*(1,26) = 3.92	
longitudinal	4	10	0.21 (0.06)**			0.058
cross-sectional	13	18	0.34 (0.04)***	0.14 (0.07)		
Sample characteristics						
Age child false belief assessment (c)	15	28		−0.00 (0.00)	*F*(1,26) = 1.71	0.203
EU assessment						
EU Task					*F*(1,26) = 2.84	0.101
Denham task	9	11	0.38 (0.06)***			
not Denham task	7	17	0.25 (0.06)***	−0.13 (0.08)		
EU Sum score					*F*(1,26) = 2.76	0.109
yes	2	19	0.19 (0.07)*			
no	13	9	0.34 (0.05)***	0.15 (0.09)		

Note. # studies = number of studies; # ES = number of effect sizes; Z *r* = Fisher's Z correlation; SE = standard error; β_1 = estimated regression coefficient; (c) = continuous variables; * p < .05; ** p < .01; *** p < .001.
a Omnibus test of all regression coefficients in the model.
b the continuous variables were mean centered before conducting the moderator analyses.

the relation between attachment and FBU, there were relatively few studies with small samples (i.e., large standard errors) that reported negative correlation coefficients or correlation coefficients below the mean effect size. This suggests that publication bias could be present in the meta-analysis on attachment and FBU in particular.

To explore this further, we performed PET-PEESE analyses. These analyses showed a similar pattern. With regard to the attachment–FBU relation, the PET intercept was b_0 = –0.03, *p* = .882, 95% CI [–0.380, 0.326]. Since the intercept *b*0 was not significantly different from zero, the PET-PEESE estimate of the true underlying effect is −0.03. Because −0.03 differs from the pooled correlation between attachment and FBU that was found (*r* = 0.19), it is likely that publication bias is present in the attachment–FBU dataset. With regard to the attachment-EU relation, the PET intercept was b_0 = 0.40, *p* = .099, 95% CI [–0.075, 0.875]. This means that the analyses showed that the estimate of the true underlying effect is 0.40, but non-significant. We mentioned above that the precision-effect test (PET) sometimes has low power in identifying a genuine nonzero effect when there are only 10 or 20 estimates available in an area of research (Stanley, 2017). We therefore also examined the PEESE results, which use the variance as the predictor in the regression, and are better able to estimate the intercept when there is a true effect. The PEESE results showed a similar intercept, b_0 = 0.38, *p* = .003, 95% CI [0.131, 0.633]. Thus, both the PET and PEESE estimates of the true underlying effect were higher than the pooled correlation we found in the meta-analysis (which was 0.31), suggesting that the estimated pooled correlation is slightly too low.

Discussion

We examined four questions concerning the direct and indirect relations between parent–child attachment security and children's mentalizing abilities, as indicated by their performance on FBU and EU tasks. The results showed that: (a) the association between attachment and FBU was significant, $r = 0.19$, as was the association between attachment and EU, $r = 0.31$; (b) the pooled correlation between attachment and EU was significantly larger than the pooled correlation between attachment and FBU; (c) children's language ability partially mediated the relation between attachment and performance on FBU tasks, but not the relation between attachment and EU; (d) the link between attachment and FBU was significantly lower for studies using assessments of attachment during infancy compared to studies using preschool assessments of attachment.

With regard to the results of the first research question, the overall correlations between attachment and FBU and attachment and EU were significant, suggesting that there is an association between attachment and children's mentalizing abilities. This adds to the view that mentalizing is acquired in the context of close personal relationships, and is in line with previous meta-analyses that reported small to moderate pooled associations between FBU and family factors, such as parents' mental state talk, mind-mindedness, and number of siblings (Devine & Hughes, 2018; Tompkins et al., 2018). Investigating the role of mind-mindedness (Meins, 1997) or parents' mental state talk in the association between attachment and mentalizing abilities might be a particularly interesting avenue for future research, since a previous meta-analysis showed that parents' mentalizing (operationalized by parents' tendency to use appropriate mind-related speech) is a significant predictor of children's secure attachment during infancy (Zeegers et al., 2017). It would therefore be interesting to test parents' mind-mindedness and attachment as predictors of FBU and EU in a single meta-analytic model to establish whether more variance in children's mentalizing abilities could be explained, and to test whether attachment is a direct and/or indirect predictor of children's EU and FBU. The MA-SEM approach that we used in the present study would be a useful technique to explore this question, since MA-SEM enables researchers to study larger theoretical models in meta-analyses that include multiple predictor and dependent variables.

Turning to the second question, the pooled correlation between attachment and EU could be considered medium to large, and was significantly larger than the attachment–FBU correlation. This outcome suggests that attachment relates more to children's thinking-about-feeling than thinking-about-thinking. It might be that, compared with performance on EU tasks, FBU task performance is less dependent on socialization processes and more related to biological factors. For instance, studies typically show that girls perform slightly better at FBU tasks, but not EU tasks (e.g., Fidalgo, Tenenbaum, & Aznar, 2018; Charman, Ruffman, & Clements, 2002). However, the well-established link between parents' mental-state talk and children's FBU performance (e.g., Devine & Hughes, 2016) counters this proposal.

Another possibility concerns the proposed role of attachment in brain regulation. We explained in the Introduction that there is accumulating evidence that the brain is experience-expectant (Perry, Blair, & Sullivan, 2017), and that attachment-related experiences may affect children's sociocognitive abilities not only through socialization processes, but also via their impact on brain development (Cozolino, 2014). It has been argued that infant attachment security 'resides' in parts of the brain that are particularly important for the processing of emotional experiences (Cozolino, 2014; Feldman, 2017; Shamay-Tsoory & Aharon-Peretz, 2007; Shamay-Tsoory et al., 2009; Walter, 2012). This may explain why our results showed a stronger association between attachment and EU than between attachment and FBU.

The third question of this meta-analytic study concerned the direct or indirect nature of any association between attachment and mentalizing abilities. We studied whether language ability was a significant mediator, because mediation would suggest that the nature of the relation is not direct. With regard to the relation between attachment and FBU, we found that language ability partially mediated the association, as the pooled correlation dropped from $r = 0.18$ to $r = 0.10$ after the mediating effect of language ability was controlled. We did not find a mediating effect of language for the relation between attachment and EU, as the pooled correlation dropped from $r = 0.30$ to $r = 0.26$ after the mediating effect of language ability was controlled. Interestingly, language ability did show a moderate association with EU performance ($r = 0.30$), which was similar to the association between language and FBU performance ($r = 0.35$). Thus, children with good language skills seem to do better on both FBU and EU tasks, but language only affects the attachment–FBU association, and not the attachment–EU association. These results suggest that different (neurological) mechanisms may link attachment to children's understanding of other people's emotions and false beliefs. However, we should be cautious about drawing strong conclusions. A lack of power could underlie this outcome, since only half of the studies reported on correlations for language ability and attachment or FBU/EU.

With regard to the fourth question, meta-analysis allows one to test possible methodological concerns underlying a hypothesized association. We studied whether the pooled correlation between attachment and children's mentalizing was moderated by the attachment assessment approach (behavioral or representational). Studies using representational attachment measures showed a significantly larger correlation with FBU performance than studies using a behavioral attachment measure, regardless of a longitudinal or cross-sectional study design. On a similar note, studies using the SSP during infancy or AQS (during infancy or preschool) reported only non-significant to small correlations between attachment and FBU performance. In contrast, we did not find a moderating effect of attachment assessment approach for the relation between attachment and EU. However, it would be an error to conclude from this finding that assessment plays no role in this association. First, only three studies examined the relation between infant attachment (behavioral measure) and later EU (compared to 8 longitudinal studies on the relation between infant attachment and FBU). The moderator analyses may thus simply not have sufficient power to detect a moderating effect of behavioral versus representational measures. Second, the three studies presented very mixed findings. Steele, Steele, Croft, and Fonagy (1999) reported that mother–infant attachment security at 12 months (SSP) predicted later EU ($r = 0.40$), whereas father–infant attachment security at 18 months did not predict EU at age 6 ($r = 0.08$). Meins et al. (in preparation) found no correlations between attachment security

(SSP) at 15 months and later EU. Raikes and Thompson (2008; $n = 42$) reported a $r = 0.28$ correlation between the attachment security score of the AQS at 28 months and EU at 42 months.

While our results show that behavioral attachment is at best weakly related to FBU, there was a significant relation between representational attachment scores and children's FBU performance. However, this association should be interpreted with caution, and does not necessarily imply a link between secure attachment and superior FBU. As mentioned in the Introduction, representational measures of attachment explicitly require perspective-taking abilities. Given the moderating effect of the attachment assessment approach and the lack of association between behavioral measures of attachment and FBU, the observed association may thus be due to representational attachment measures and FBU tasks drawing on the same ability to understand others' internal states, rather than indicating a genuine link between attachment security and FBU.

Caution is also warranted in concluding that our results provide evidence for early infant secure attachment directly facilitating children's understanding of others' belief states. In fact, these findings could be interpreted as being in line with the opposite direction of cause and effect—preschoolers who are good at reading their own and other people's minds are better able to form secure parent–child attachments in the preschool years. Support for this interpretation comes from a recent study investigating predictors of behavioral preschool attachment and stability in behavioral attachment from infancy to age 4. Meins, Bureau, and Fernyhough (2018) reported that better age-2 perspective-taking as assessed in social symbolic play predicted secure attachment at age 4 and maintaining a secure attachment relationship from infancy through to the preschool years. Interestingly, there was some evidence that poorer symbolic play abilities may play a role in a transition from secure to insecure attachment over time, with marginally lower perspectival play abilities in children whose attachment changed from secure to insecure compared with those who maintained a secure attachment. These findings highlight the potential role of mentalizing abilities in shaping children's attachment security.

In the adult attachment literature, a body of published studies using fMRI to investigate humans' various social attachments provides support for the idea that mentalizing processes such as understanding people's motivations and appreciating different perspectives underpin adult attachment relationships (Feldman, 2017). Moreover, mentalization-based treatment is widely acknowledged as a way of helping adults with a dismissing or anxious attachment style to start building trust in themselves and others (Bateman & Fonagy, 2013). This type of intervention is based on the assumption that improving mentalization helps patients to go on to learn socially from new experiences, and achieve change in their understanding of their social relationships and their own behavior and actions, thereby building secure attachments (Fonagy & Allison, 2014). Thus, for both children and adults, there is evidence for mentalizing abilities playing a causal role in establishing the security of attachment relationships. This perspective is in line with Bowlby's recognition that "the child's capacity both to conceive of his mother as having her own goals and interests separate from his own and to take them into account" (1969, p. 368) as a relevant step towards the establishment of attachment as a goal-corrected partnership.

The potential role of mentalizing abilities in shaping attachment relationships requires further attention in future research. In order to disentangle possible bi-directionality of the attachment–mentalizing association, we need studies that observe parent–child attachment at several time-points and during longer stretches of time to test cross-lagged models (Kenny, 1975). Such studies would ultimately provide valuable information for the development or modulation of interventions aimed at facilitating child attachment security. The Basic Trust method is one example of an attachment intervention method that is focused on explicitly improving children's mentalizing abilities through teaching parents to frequently and consistently make the child aware of their own and others' mental states (i.e., by continuously using mental state language when interacting with their child; Polderman, 1998; 2017). Although the effectiveness of this intervention method requires further study, two initial studies have shown that children showed less insecure attachment behaviors after the interventions (Colonnesi et al., 2013; Zeegers, Colonnesi, Noom, Polderman, & Stams, 2019).

Limitations

A first limitation of this study is that, although the meta-analysis is based on a total of 64 effect sizes, the number of individual samples was relatively small in both meta-analyses ($k = 15$; attachment–EU; $k = 17$ for attachment–FBU). We examined the sensitivity of the analyses to understand more about the robustness of the results. When influential studies were left out (i.e., studies with a large effect size or sample size), the pooled correlations did not change > 0.01. The results were thus not highly dependent on the contributions of one study. The small number of studies may have been more influential when it came to the moderator analyses. As mentioned previously, the moderator analyses were underpowered, and nonsignificant results should be interpreted with caution. We had strict inclusion criteria in terms of sample characteristics (only samples without parents having a mental health diagnosis and non-biological children aged between 3 and 6 at the time of the mentalizing assessment). However, studies used a variety of different validated attachment instruments. Although the moderating impact of the attachment instruments was clear in the analyses, we would have been able to draw more accurate conclusions about the methodological issues that underlie the relation between attachment and children's mentalizing capacity if we could have included more studies.

Another limitation is that most of the studies included in this meta-analysis ($n = 12$) used single task scores to assess FBU with binary outcomes (i.e., pass-or-fail outcomes). FBU task scores will therefore typically show less variation than EU tasks, and this could (partly) underlie the observed finding that the pooled attachment–FBU association was smaller than the attachment–EU association. In addition, we focused on FBU and EU, rather than capturing the entire scope of mental state understanding. There are other (earlier) aspects of mentalizing that were not considered, such as the understanding of discrepant desires, visual perspective taking and the understanding of pointing gestures (Carpendale & Lewis, 2004). Very few studies examined early forms of mentalizing in relation to attachment security (e.g., Laranjo, Bernier, Meins, & Carlson, 2010; Meins et al., 2018). An expansion of research on this topic could help to get a better understanding of the direction of the attachment–mentalizing association.

Mentalizing is currently viewed as a “dynamic capacity that is influenced by stress and arousal, particularly in the context of specific attachment relationships” (Fonagy et al., 2012; p. 19). A child might be able to mentalize about presumed beliefs and emotions of self and others within an impersonal context (i.e., the Sally-Anne task), but, at the same time, be less inclined to mentalize accurately within a relational context (e.g., with attachment figures; Meins, Fernyhough, & Harris-Waller, 2014). Hence, it might be important to assess children’s mentalizing abilities using measurements that activate the attachment system (e.g., see Ensink et al., 2015 for an example of a new interview assessment of mentalizing in children). Future research may also benefit from directing attention to the relation between attachment and mentalizing in more naturalistic settings. In the end, whether and how we use our mentalization capacity during interactions with other people is what counts during daily social experiences. Behavioral measures of children’s mindreading capacity (e.g., use of mind-related speech during interactions with peers) may provide a more ecologically valid index of mentalizing abilities, with stronger implications for clinical intervention.

As for all meta-analytic studies, caution is warranted because some unpublished studies may not have been present in the dataset, leading to possible inflations of the estimated pooled correlations. The inspection of the funnel plots as well as the PET-PEESE analyses indicated that publication bias could have been present, particularly in the meta-analysis on attachment and FBU. We attempted to track as many unpublished studies as possible by searching for unpublished doctoral theses and conference presentations, and by asking expert researchers in the field if they knew about any unpublished studies. We tracked two unpublished studies on the relation between attachment and FBU, which both showed zero correlation between attachment and FBU ($n = 107$, Oppenheim et al., 2005; $n = 48$, Reese, 1998). Interestingly, both studies assessed attachment security with a behavioral measure during infancy (SSP and AQS). This suggests that if there are unpublished studies that we did not track, the pooled correlation might become lower, and possibly the moderating effect of the attachment assessment approach might become even stronger.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.dr.2019.100885.

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