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Research article

The intergenerational transmission of child maltreatment: A three-level meta-analysis

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ABSTRACT

A parental history of experiencing child maltreatment is an important risk factor in several etiological theories of child maltreatment. In the past, two reviews have been conducted on the available evidence for intergenerational continuity in child maltreatment, but were only qualitative in nature. Therefore, the present review aimed to provide a quantitative summary of the current knowledge on intergenerational transmission of child maltreatment. In our 3-level random-effects meta-analysis, we included 84 studies reporting on 285 effect sizes and found a medium summary effect of $r = 0.289$; 95% CI [0.257, 0.337], with significant variation in effect sizes within (level 2) and between (level 3) studies. This implies that in families of parents who experienced maltreatment in their own childhood, the odds of child maltreatment are almost three times the odds of child maltreatment in families of parents without a history of experiencing child maltreatment (OR = 2.990). However, as indications for bias were found, caution is warranted in interpreting this effect. Moderator analyses revealed that the effect of intergenerational transmission was the smallest in children who experienced physical abuse. Further, study quality was negatively associated with effect size magnitude. We highlight the need for an improvement in quality of primary research, and discuss implications of our findings for clinical practice.

1. Introduction

Child maltreatment is a worldwide problem that severely impacts both victims and society. Stoltenborgh, Bakermans-Kranenburg, Alink, and Van IJzendoorn (2015) demonstrated in their meta-analytic review that estimated prevalence rates based on self-report studies were 12.7% for sexual abuse, 22.6% for physical abuse, 36.3% for emotional abuse, 16.3% for physical neglect, and 18.4% for emotional neglect. Other researchers have provided evidence for the association between child maltreatment victimization and problems in multiple domains of functioning, such as academic achievement, social and emotional development, psychopathology, and neurobiological deficits (see Widom, 2014 for a review of child maltreatment consequences). Therefore, it is important to understand the risks associated with child maltreatment, so that effective preventive strategies can be developed targeting these risks.

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In several etiological theories of child maltreatment (e.g., Bandura & Ribes-Inesta, 1976; Belsky, 1980, 1993), a parental history of experiencing child maltreatment is considered to be an important risk factor for child maltreatment. In this meta-analytic review, we focus on the effect of this specific parent-related risk factor.

Differences between studies in prevalence and consequences of (forms of) child maltreatment can be partly explained by differences in definitions of child maltreatment used by scholars and practitioners. The World Health Organization states that "child abuse or maltreatment constitutes all forms of physical and/or emotional ill-treatment, sexual abuse, neglect or negligent treatment or commercial or other exploitation, resulting in actual or potential harm to the child's health, survival, development or dignity in the context of a relationship of responsibility, trust or power" (Krug, Dahlberg, Mercy, Zwi, & Lozano, 2002). In The United States, child abuse and neglect is defined by the Federal Child Abuse and Prevention and Treatment Act as (1) any recent act or failure to act on the part of a parent or caretaker resulting in death, serious physical or emotional harm, sexual abuse, or exploitation; or (2) an act, or failure to act, that presents an imminent risk of serious harm (CAPTA Reauthorization Act of, 2010). In this review, we focused on four forms of child maltreatment: physical, sexual, and emotional abuse, and neglect.

Child maltreatment is generally regarded as a multifactorial determined phenomenon, meaning that many different risk factors contribute to the occurrence of child maltreatment. Belsky (1980, 1993) noted that risk factors can be found at four different "levels of analysis" varying in proximity to the child. The first level represents parental- and child-related factors, the second level represents family-related factors, the third level represents risk factors present in the community of the family as well as risk factors in the social system surrounding the family, and the fourth level represents risk factors regarding a society's attitude towards children and abuse. Brown, Cohen, Johnson, and Salzinger (1998) demonstrated that in particular an accumulation of risk factors at different levels contributes to child maltreatment. As parents (or caretakers) are most proximal to children, it can be theorized that particularly parent-related factors are most determinative in the occurrence of child maltreatment. Mulder, Kuiper, Van der Put, Stams, and Assink (2018) and Stith et al. (2009) confirmed in their meta-analytic reviews on risk factors for child maltreatment that the largest effects were found for a variety of parent-related risk factors. Examples of these factors with relatively large effects are mental health problems, experiencing stress, (ab)using alcohol and/or drugs, a low self-esteem, and poor parenting skills (Mulder et al., 2018; Stith et al., 2009).

Parenting behavior is greatly influenced by parents reacting to experiences in their childhood. In raising children, parents tend to apply parenting behavior that is based on child-rearing practices of their own parents, meaning that parents raise their children in a way that resembles their own upbringing (see, for instance, the reviews of Puttallaz, Constanzo, Grimes, & Sherman, 1998 and Van IJzendoorn, 1992). In scientific literature, this intergenerational continuity in parenting behavior is often referred to as intergenerational transmission. Not only positive parenting behavior can be passed on by intergenerational transmission, also abusive parenting practices can be passed on from generation to generation. For instance, Finzi-Dottan and Harel (2014) found that the risk for maltreating children was six times greater when parents had experienced child maltreatment themselves. This is in line with evidence from Sroufe, Egeland, Carlson, and Collins (2005) who found that 70% of maltreated parents abuse or neglect their children. Based on these studies, a parental history of experiencing child maltreatment may be one of the parent-related risk factors that, in general, are important in predicting child maltreatment. However, there is also evidence contrary to these findings. Kaufman and Zigler (1987) found that most of the parents who experienced child maltreatment did not maltreat their own offspring, which is in accordance with Browne and Herbert (1997), who found that 'only' 7.6% of the parents who experienced maltreatment abused their offspring.

Several pathways may explain the intergenerational cycle of child abuse. First, behaviorists assume that by experiencing maltreatment, children learn that hurting and harming others is 'normal' (Bandura, 1973, 1977). The maltreating behavior is modeled and internalized and consequently, children are more likely to repeat such behaviors as adults (Bandura & Ribes-Inesta, 1976). Second, Kerig and Becker (2010, Chapter 2) suggest that maltreated children have impaired emotion regulation capacities due to the chronic and pervasive maltreatment. As a result, these children experience increased irritability and impulsivity, resulting in aggressive behaviors later on in life (Kerig & Becker, 2010, Chapter 2). In addition, Kerig and Becker note that cognitive processes, such as hostile attribution, could account for the intergenerational transmission of child abuse. Third, child abuse has been consistently linked to disorganized (or unresolved) attachment (Cyr, Euser, Bakermans-Kranenburg, & Van IJzendoorn, 2010). In parents with unresolved (or disorganized) attachment representations, interactions with their child could trigger their traumatic childhood experiences, leading to atypical, abusive parenting behavior (Goldberg, Benoit, Blokland, & Madigan, 2003; Main & Hesse, 1990). This atypical, frightening behavior of parents can cause disorganized attachment in children, resulting in a cycle of disorganized attachment across generations (Benoit & Parker, 1994; Madigan et al., 2006).

A fourth pathway is the intergenerational transmission of psychopathology. Research has shown that children of parents with mental illnesses are more likely to suffer from psychopathology than children from healthy parents (Deault, 2010; Frick et al., 1992; Goodman et al., 2011), which is, among other things, explained by the genetic vulnerability of psychopathology (Goodman & Gotlib, 1999). Because mental illness in parents has shown to be related to elevated risks of abusive behavior (e.g., Stith et al., 2009), the intergenerational transmission of psychopathology offers an explanation for the intergenerational cycle of child abuse. Finally, the intergenerational transmission of attachment can be explained by the transfer of a general risk environment from parents to children, including a low socio-economic status, and a low quality of the supportive system around the family (Stith et al., 2009). These risks emphasize that a parental history of child maltreatment is only a single (parent-related) risk factor that may be present next to other risk factors. Further, those other risk factors may be equally or even more important than a parental history of child maltreatment, and can be unique predictors independent of a parental history of child maltreatment.

Given the contrary findings of previous studies on intergenerational transmission of child maltreatment (Browne & Herbert, 1997; Kaufman & Zigler, 1987; Sroufe et al., 2005), it is important to synthesize all primary study findings in a systematic manner, so that

knowledge about the true role of intergenerational transmission in the occurrence of child maltreatment is increased. With this in mind, several review studies have been performed in the past, but no clear picture emerges from these studies. For instance, in the qualitative review of Ertem, Leventhal, and Dobbs (2000), in which ten studies were included, the reported risk ratios for intergenerational transmission of physical child abuse ranged between 1.05 (i.e., hardly any evidence for intergenerational transmission) and 37.8 (i.e., very strong evidence for intergenerational transmission). The authors assumed that the magnitude of effect sizes is heavily influenced by methodological aspects of primary studies, and that the quality of future studies needs to be improved to better understand the intergenerational continuity of physical child abuse.

This conclusion is supported by Thornberry, Knight, and Lovegrove (2012), who conducted a qualitative review of 47 primary studies investigating the continuity across generations of physical abuse, sexual abuse, emotional abuse, neglect, and/or co-occurrences of these maltreatment forms. Because primary studies differ greatly in methodological quality, Thornberry et al. (2012) evaluated each included study against 11 methodological criteria, such as using representative samples, administering valid and reliable instruments, collecting prospective data, clearly defining child maltreatment, and using control groups in which non-maltreatment is verified. In general, most primary studies supported intergenerational transmission of child maltreatment, but only few studies met these methodological criteria. Furthermore, the studies of higher methodological quality showed mixed support for intergenerational transmission of maltreatment. Thornberry et al. (2012) concluded that the available primary studies are small in number as well as methodologically weak, and do not provide a determinative conclusion on the role of intergenerational transmission in the occurrence of child maltreatment.

In the two narrative review studies of Ertem et al. (2000) and Thornberry et al. (2012), the intergenerational cycle of child maltreatment was not quantitatively tested, because these studies were only qualitative. However, quantitatively synthesizing results of studies on the cycle of child maltreatment is required for a better understanding of the role of intergenerational transmission of maltreating behavior of parents. Specifically, by performing a meta-analysis, intergenerational transmission of maltreatment can be expressed in a summary effect size of which the magnitude is indicative for the overall strength of the association between a parental history of child maltreatment and parental maltreating behavior. Moreover, by examining variables as potential moderators of this summary effect, we may better understand why differences in evidence for the cycle of maltreatment are found in primary studies. For instance, it is possible that intergenerational transmission is not equally occurring for all forms of maltreatment that parents experienced in their own childhood. It is also possible that the effect of intergenerational transmission is not the same for each form of child maltreatment (i.e., physical, sexual, and emotional abuse, and neglect). These issues can be tested in moderator analyses.

A better understanding of the true effect of intergenerational transmission may not only strengthen our knowledge of the etiology of child maltreatment, it is also important from a clinical perspective. If a parental history of child maltreatment significantly increases the risk for maltreating own offspring, the parental abuse should be taken into account in risk assessment practices required for identifying children and families who are in need of (preventive) care. Additionally, a parental history of abuse may be indicative for unresolved trauma or attachment-related problems of parents requiring attention in treatment. Proper treatment may not only alleviate these problems, but also reduce the risk for future maltreating behavior of parents.

In sum, this study aimed to perform a meta-analysis of primary studies on the intergenerational transmission of (different forms of) child maltreatment. Besides estimating a summary effect of intergenerational transmission as a risk factor for child maltreatment, we examined whether and how this effect is influenced by the form of maltreatment experienced by parents, the form of maltreatment experienced by offspring of these parents, and several study descriptors, including an index for the methodological quality of primary studies.

2. Method

2.1. Eligibility criteria

A number of criteria were specified to determine whether primary studies could be included in the present review. First, as the methodological quality greatly differs between primary studies (Ertem et al., 2000; Thornberry et al., 2012), we decided to only include studies published in peer-reviewed scientific journals and published dissertations. In this way, we aimed for preserving a minimum of study quality. Second, studies had to report at least one association between parental *experiences* of child maltreatment in their own childhood (i.e., parental maltreatment victimization in the first generation) and maltreating *practices* of these parents directed to their offspring (i.e., maltreatment victimization of parental offspring in the second generation). As we were interested in the continuity of maltreatment across generations, child maltreatment had to be assessed in at least two adjacent generations. As for the form of child maltreatment – either experienced by parents or inflicted by parents on their offspring – we included studies that reported on physical abuse, sexual abuse, emotional abuse, and/or neglect, as well as studies reporting on maltreatment in general (i.e., not reporting on specific abuse types). Third, for each association in all primary studies, an effect size or sufficient statistical information to calculate an effect size had to be reported. Fourth, we only included studies performed in Western countries (i.e., USA, European countries, Australia, New Zealand, and Canada), as child maltreatment definitions, the nature and impact of risk factors for child maltreatment, and intergenerational continuity of child maltreatment, can differ greatly across Western and non-Western countries and cultures.

2.2. Literature search

In searching relevant studies for inclusion, we performed several complementary search strategies. First, we searched for studies

in the electronic databases PsycINFO, Web of Science, ScienceDirect, and Google Scholar. Our search comprised permutations of keywords related to "child maltreatment" and "intergenerational transmission". The keywords were: "abuse", "maltreatment", "neglect", "violen*", "harsh parent*", "transmission", "intergenerational", "transgenerational", "continu*", "cycle", "familial", and "history". In each search query, we used different combinations of these keywords. Second, we screened the reference sections of the reviews of Ertem et al. (2000) and Thornberry et al. (2012) to find additional studies that did not came up in our electronic search. We also screened the meta-analytic review of Stith et al. (2009) on risk factors for child maltreatment as parental experiences of child maltreatment may have been examined as a "risk factor" for child maltreatment. In our last search strategy, we screened the reference sections of all included primary studies for other relevant studies. To determine whether primary studies were eligible for inclusion, we read titles, abstracts, and if necessary, full article texts. The final list of included studies was agreed upon by all authors of this study.

2.3. Coding of studies and data extraction

Prior to coding studies, we developed a coding form for extracting relevant information from primary studies. Although it is common practice in meta-analyses to retrieve a large amount of information on a variety of potential moderating variables (e.g., Cooper, 2010; Lipsey & Wilson, 2001), we focused on a rather small and specific set of variables of interest, as the problem of multiple testing in primary research (Tabachnik & Fidell, 2013) is equally present in secondary research. The variables that were coded are described below.

2.3.1. Type of child maltreatment

As child maltreatment can be perpetrated on victims in different forms, we coded the type of child maltreatment using the categories physical abuse, sexual abuse, emotional abuse, neglect, and (general) maltreatment not further specified. This last category refers to abuse or neglect of which the type was unknown, for instance because researchers did not have specific consent for examining social service records for details of the abuse (e.g., Sidebotham, Golding, The ALSPAC Study Team, 2001). The maltreatment type was coded for both the maltreatment experienced by parents and the maltreatment experienced by offspring of these parents.

2.3.2. Type of child maltreatment assessment

Different methods can be used for assessing occurrences of child maltreatment and in each method the problem of underreporting is more or less present. In general, it is believed that official records only capture a small fraction of the actual episodes of child maltreatment (e.g., Fergusson, Horwood, & Woodward, 2000; Finkelhor, 2008; MacMillan, Jamieson, & Walsh, 2003). Therefore, we coded whether child maltreatment was assessed by analyzing official records or by using self-report methods, such as interviewing participants or administering questionnaires. We coded this variable for both parental maltreatment victimization and the offspring's maltreatment victimization.

2.3.3. Study quality index

As Ertem et al. (2000) and Thornberry et al. (2012) noted a large variety in methodological quality of studies, we deemed it necessary to examine whether and how the quality of primary studies would influence the overall effect of intergenerational transmission of child maltreatment. Therefore, we created a study quality index that was based on the following 10 (of 11) criteria proposed by Thornberry and colleagues: (1) the sample is representative of a general population; (2) the participation rate is satisfactory and there are low levels of attrition; (3) the sample comprised both maltreated and non-maltreated individuals; (4) a non-maltreatment status of participants has at least to some degree been verified; (5) prospective (rather than retrospective) measures were collected; (6) different reporters (or more than one method) was used to assess maltreatment victimization and maltreatment perpetration in subsequent generations; (7) child maltreatment was assessed over the same age ranges in maltreated and non-maltreated individuals; (8) follow-up assessments of child maltreatment were performed in a period of at least 5 years; (9) across generations, a child maltreatment assessment method was used that has been demonstrated to properly assess the occurrence of maltreatment; (10) a clear definition of child maltreatment was described in the study (see the review of Thornberry and colleagues for a detailed description of these criteria). We evaluated all included studies against each of these study quality criteria and coded whether a criterion was met (value 1) or not (value 0). Subsequently, we calculated a quality index for each study by adding all coded criterion values.

In calculating our study quality index, we deliberately left out one of the 11 quality criteria that Thornberry et al. (2012) considered to be relevant in evaluating study quality. These authors stated that zero-order associations (i.e., unadjusted associations) are not an adequate test of the cycle of maltreatment hypothesis as making valid inferences requires that associations are controlled for potential "confounding" factors, which in fact are risk factors (e.g., low SES or poverty, young maternal age) that may be associated with the intergenerational continuity of child maltreatment. In primary research, it is indeed of value to examine the unique contribution of intergenerational transmission in explaining variance in child maltreatment by including different covariates in the analyses (such as multiple sample, study, and participant characteristics). However, scholars rarely use the exact same set of covariates across primary studies, which poses a problem in meta-analytic research. Combining and comparing differentially adjusted effect sizes limits the ability to robustly estimate the true overall effect of intergenerational transmission of child maltreatment. Besides, for a primary researcher, it is not possible to control for *all* possible confounding factors one can think of in assessing the effect of intergenerational transmission. Yet, in many cases only adjusted effect sizes could be extracted, mostly derived from studies using a matched (no maltreatment) comparison group or studies that only report on multivariate statistics. Consequently, we

included both adjusted and unadjusted effect sizes in our dataset and we coded for each effect size whether or not it was adjusted for any other variable. In this way, it was possible to test whether the magnitude of unadjusted effect sizes significantly deviated from the magnitude of adjusted effect sizes.

2.3.4. Study design

We coded whether primary studies were longitudinal or cross-sectional and whether studies were prospective or retrospective.

2.3.5. Gender composition of the sample of parents

For each primary study we coded whether the sample of parents comprised only mothers, only fathers, or mothers and fathers (i.e., a mixed category).

2.3.6. Ethnicity

As participants in primary studies have different cultural backgrounds, we were interested in whether and how this affected intergenerational transmission of child maltreatment. However, as most studies did not report on the sample composition in great detail, we only coded the percentage of Caucasians in a sample.

The full coding procedure was conducted by two researchers after having obtained sufficient reliability (Kappa > 0.80, or ICC > 0.70). Any discrepancies in coding entries were discussed and resolved until full consensus on all coding entries was achieved.

2.4. Calculating effect sizes

To quantify the effect of intergenerational transmission of child maltreatment, we calculated a correlation coefficient for each reported association between parental experiences of child maltreatment in their own childhood and maltreatment perpetrated on these parent's child or children. As associations were not only expressed in correlations, it was often necessary to transform study-specific data, such as proportions, odds-ratio's, and means and standard deviations, into correlation coefficients. For these transformations, we used the methods and formulas of Ferguson (1966), Lipsey and Wilson (2001), and Rosenthal (1994). As for the direction of correlations, a positive sign was assigned if higher levels of parental experiences of child maltreatment (first generation maltreatment) were associated with higher levels of maltreatment of the child or children of these parents (second generation maltreatment). A negative sign was assigned to correlations if first generation maltreatment was inversely related to second generation maltreatment.

To prevent that outlying effect sizes would disproportionally influence parameters estimated in the meta-analytic models, we assessed outliers by searching for effect sizes having standardized z scores larger than 3.29 or smaller than -3.29 (see also Tabachnik & Fidell, 2013). In total, 4 effect sizes were identified as an outlier, since the corresponding z values exceeded 3.29. To reduce the influence of these outliers, the raw correlations were substituted by a new value that would fall just within the normal range of the distribution of effect sizes. In this way, a disproportional influence of the outliers on the parameters to be estimated was reduced without discarding any data.

As recommended by several scholars (e.g., Cooper, 2010; Lipsey & Wilson, 2001), correlations should be transformed to corresponding Fisher's z-scores, as correlations are not normally distributed. Therefore, in the final step of calculating effect sizes, we transformed all correlations into Fisher's z-scores. After the statistical analyses were performed, the Fisher's z-scores were converted back into correlations to facilitate interpretability.

2.5. Statistical analyses

We applied a random-effect approach, as the included primary studies were regarded as a random sample from a larger population of studies (e.g., Raudenbusch, 2009; Van den Noortgate & Onghena, 2003). Most studies reported on more than one association between child maltreatment in the first generation and child maltreatment in subsequent generations, for instance, because different abuse types were examined. Consequently, multiple effect sizes could be extracted from one primary study (or calculated using statistics reported in studies), as these effect sizes met our eligibility criteria (see Appendix A for the number of extracted effect sizes per study). In general, it is assumed that effect sizes extracted from the same primary study are more similar than effect sizes extracted from different studies, since the former are based on the same participants, instruments, and/or conditions in which the study was performed (e.g., Houben, Van den Noortgate, & Kuppens, 2015). Because independence of effect sizes is an important prerequisite in meta-analytic research, we had to employ an approach in which effect size dependency is modeled. Therefore, we chose to combine effect sizes in a 3-level meta-analytic model (see also Assink & Wibbelink, 2016).

In 3-level meta-analytic models, three different sources of variance are modeled: variance between studies (level 3), variance between effect sizes extracted from the same study (level 2), and sample variance of the effect sizes (level 1) (Cheung, 2014; Hox, 2002; Van den Noortgate, López-López, Marin-Martinez, & Sánchez-Meca, 2013; Van den Noortgate, López-López, Marin-Martinez, & Sánchez-Meca, 2014). The multilevel meta-analytic models allow calculating a summary (or average/overall) effect size and, if significant variance on level 2 and/or level 3 is present, to examine whether this effect is influenced by study, sample, and maltreatment characteristics by including these variables in the model as covariates. In our model, the sample variance of effect sizes (level 1) was not estimated, but treated as known. We calculated the level-1 variance using the formula of Cheung (2014, p. 2015).

The statistical environment *R* (version 3.3.0, R Core Team, 2015) was used to build the multilevel models, and we used the syntax as described by Assink and Wibbelink (2016) in applying the multilevel approach to meta-analysis as described by Cheung (2014)

and Van den Noortgate et al., 2013, 2014). All model coefficients were tested two-sided using the Knapp-Hartung-correction (Knapp & Hartung, 2003), implying that – rather than a *Z*-distribution - a *t*-distribution was used for testing individual coefficients, and an *F*-distribution was used for the omnibus-test of all coefficients in the model (excluding the intercept). In determining the significance of the level-2 and level-3 variance, two independent one-sided log-likelihood-ratio-tests were performed, in which the deviance of the full model was compared with the deviance of the model without one of the two variance parameters. The restricted maximum likelihood method was used in estimating all model coefficients. Prior to testing variables as potential moderators of the summary effect of intergenerational transmission of child maltreatment, we created dummy variables for each category of all discrete variables, and we centered all continuous variables around their mean. All meta-analyses were performed in *R* using the function "rma.mv" of the metafor package of Viechtbauer (2010). In all analyses, a 5% significant level was used.

2.6. Missing data and sensitivity analysis

Although we performed several complementary search procedures to find and retrieve as much relevant primary studies as possible, it is possible that studies were missed due to shortcomings in our search strategy or other forms of bias, such as publication bias or subjective reporting bias. To determine whether (a form of) bias was present in the effect sizes we examined, several steps were taken. First, we visually inspected a funnel plot in which standardized effect sizes are charted against the standard error around an estimated summary effect. Rather than studies using large samples, small studies are more likely to produce effect sizes of different magnitude due to increased variability in their sampling errors. Therefore, effect sizes from smaller studies are expected to scatter widely at the bottom of the funnel plot, whereas effect sizes from larger studies are expected to be more concentrated at the top of the plot. In the absence of bias, this plot would be a symmetrical inverted funnel with effect sizes equally distributed to the left and right of a summary effect. However, if the plot is asymmetrical, bias may be present. For instance, there is a tendency in scientific literature to publish significant and (large) positive effects rather than small, non-significant, and/or negative effects, which is referred to as publication bias. If publication bias is present, asymmetry in the plot is expected, as the small, non-significant, and/or negative effects are underrepresented to the (bottom) left of the estimated summary effect in the funnel plot. This could imply that a summary effect is an overestimation of a true effect.

Second, objective measures were calculated to determine whether a funnel plot is asymmetrical. Specifically, we estimated Begg and Mazumdar's rank order correlation (Begg & Mazumdar, 1994) and we performed Egger's test (Egger, Davey-Smith, Schneider, & Minder, 1997). The first examines the rank association between the standardized effect sizes and the weighting factors of each effect size, which are determined by the sample size on which the effect is based. A significant association is an indication for asymmetry in the funnel plot and implies the presence of bias. Specifically a significant positive association means that smaller studies produced larger effect sizes than larger studies did, which is indicative for the presence of publication bias. The second examines whether the standardized effect sizes can predict study precision - defined as the inverse of the standard error – in a simple linear regression analysis. In this model, an intercept significantly deviating from zero implies the presence of bias. Specifically a significant positive intercept means that smaller studies with less precision are associated with larger effects, suggesting that publication bias is present (Egger et al., 1997). Sterne, Gavaghan, and Egger (2000) showed that the Egger's test is identical to regressing effect sizes on standard errors, where the weights are inversely proportional to the variance of effect sizes. In this model, the slope, and not the intercept, is the bias indicator.

Third, we conducted the trim-and-fill method of Duval and Tweedie (2000a, 2000b) to determine whether (and which) effect sizes need to be imputed to restore the symmetry of the funnel plot. We examined not only whether small effects (to the left of the summary effect) were underrepresented, for instance because of publication bias, but also whether large effects (to the right of the summary effect) were missing due to other forms of bias. A higher number of "missing" effect sizes indicates bias to a greater extent. After performing the trim-and-fill method, we imputed the "missing" effect sizes in our dataset, so that a re-estimation of the summary effect was possible. In doing so, we could obtain an effect that was adjusted for bias and as such, this re-estimation can be regarded as a form of sensitivity analysis. The trim-and-fill method was performed by using the function "trimfill" of the metafor package (Viechtbauer, 2010) in the R environment (Version 3.2.0; R Core Team, 2015).

Finally, we tested whether a decline in published effect sizes can be observed over time by testing publication year of primary studies as a moderator of the summary effect. In this way, we tested whether the "decline effect" (Schooler, 2011; also referred to as the "law of initial results" by Ioannidis, 2005) was present in our results. This effect refers to the tendency for positive results to get smaller over time, for instance because of regression to the mean.

3. Results

3.1. Study characteristics

The present meta-analysis included $k = 84$ primary studies from which $u = 285$ effect sizes were extracted. On average, 3.393 effect sizes were extracted from each included study (SD = 9.298; range = 1–83). The studies were published between 1975 and 2017, and the median publication year was 2004. Most studies were conducted in the USA/Canada ($k = 71$), several in Europe ($k = 12$), and one in Australia ($k = 1$). Appendix A lists all included studies with several study characteristics. The references of all included studies can be found in Appendix B.

Table 1
Summary Effect Before and After Trim-and-Fill Analysis.

	Mean z (SE)	95% CI	Sig. mean z (p)	% Var. at level 1	Level 2 variance	% Var. at level 2	Level 3 variance	% Var. at level 3	r
Summary effect before trim-and-fill	0.297 (0.020)	0.257; 0.337	< .001	2.08	0.018***	47.06	0.019***	50.86	0.289
Summary effect after trim-and-fill	0.345 (0.020)	0.306; 0.384	< .001	1.94	0.016***	37.62	0.026***	60.45	0.332

Note. Mean z = Mean effect size (Fisher's z); SE = Standard error; CI = Confidence interval; Sig = Significance; % Var = Percentage of variance explained; Level 2 variance = Variance in effect sizes within studies; Level 3 variance = Variance in effect sizes between studies; r = Mean effect size (correlation).
*** $p < .001$.

3.2. Overall effect size

The estimated summary effect of intergenerational transmission of child maltreatment across all studies ($k = 84$) and effect sizes ($u = 285$) was $r = 0.289$, $p < .001$, 95% CI [0.251, 0.325] (see Table 1), indicating that a parental history of child maltreatment (i.e., first generation maltreatment) is associated with child maltreatment (i.e., second generation maltreatment). According to the criteria of Rice and Harris (2005), this is a medium effect. This summary effect should be interpreted with caution, as we found significant level 2 as well as level 3 variance, implying substantial variability in effect sizes extracted from the same study (level 2) and from different studies (level 3) (see Table 1). Around 47% of the total variance could be attributed to within-study differences in effect sizes (level 2) and around 51% could be attributed to between-study differences in effect sizes (level 3). Given the variability in effect sizes within and between studies, also referred to as heterogeneity, testing variables as potential moderators of the summary effect was justified.

3.3. Analysis of bias

Table 2 presents the results of the six techniques that we used to assess whether bias was present in the effect sizes being synthesized. A visual inspection of the funnel plot (see Fig. 1) revealed asymmetry, particularly at the (top) right side of the plot (i.e., above the summary effect) where effect sizes seem to be missing. Asymmetry in the funnel plot, which is indicative of bias, was underlined by significant results of the Rank Correlation Test (Begg & Mazumdar, 1994) and the "classical" Egger's test (Egger et al., 1997). However, when was accounted for dependency of effect sizes, the Egger's test did not yield a significant result implying that bias is absent. Further, the results of the trim-and-fill analysis (Duval & Tweedie, 2000a, 2000b) showed that 46 effect sizes extracted from 18 studies had to be imputed to restore symmetry of the funnel plot. As "missing" effect sizes were imputed to the right of the summary effect, the initial estimated summary effect may be an underestimation of the true effect. A re-estimation of the summary effect after imputing these "missing" effect sizes in the data did reveal a somewhat larger summary effect of $r = 0.332$, $p < .001$, 95% CI [0.297, 0.366]. The level 2 and level 3 variance were still significant after imputing effect sizes, implying substantial variability in effect sizes extracted from the same study (level 2) and from different studies (level 3) (see Table 1). As a further indication of bias, we found a significant moderating effect of publication year, indicating that the magnitude of effect sizes declines over time. This last result supports the "decline effect" (Schooler, 2011) and the "law of initial results" (Ioannidis, 2005), and can be regarded as an indication of bias. In general, the results reflect a moderate to strong indication of bias in our data (see also the outcomes and implications in Table 2).

3.4. Moderator analyses

The results of all moderator analyses are presented in Table 3 in which moderators are classified into maltreatment, study design, and sample descriptors. Below, the results of the moderator analyses are discussed using this same categorization.

3.4.1. Maltreatment descriptors

A significant moderating effect was found for the type of maltreatment experienced by children of parents who were abused in their own childhood. Specifically, we found a lower effect of intergenerational transmission of child maltreatment for children who experienced physical abuse (mean $r = 0.246$) and children who experienced neglect (mean $r = 0.292$) than for children who experienced an unspecified maltreatment type (mean $r = 0.333$). We found no significant effect for the maltreatment type experienced by parents.

3.4.2. Study design descriptors

We found a higher effect of intergenerational transmission in studies using official records for assessing maltreatment of offspring/children (mean $r = 0.357$) than in studies using other assessment methods (mean $r = 0.230$). We also found a significant and substantial negative moderating effect of study quality that was tested using a continuous index (see Method section). As study quality increased, the effect of intergenerational transmission decreased (standardizing the regression coefficient resulted in a β_1 of

Table 2
Results of Six Methods for Assessing Publication Bias.

Method for assessing bias	Outcomes	Implications
Visual inspection of the funnel plot	Asymmetric funnel plot, as effect sizes in the (top) right side of the funnel and, to a less extent, in the (bottom) left side of the funnel seem underrepresented (see Fig. 1).	The asymmetry in the (top) right side of the funnel plot is an indication of bias towards below average findings from relatively precise and large studies (i.e., above average findings from relatively precise and large studies may be underrepresented). On the other hand, (publication) bias might be present, as below average findings from relatively imprecise and small studies seem somewhat underrepresented.
Rank Correlation Test (Begg & Mazumdar, 1994)	Kendall's tau = 0.288, $p < .001$	Kendall's tau represents the correlation between the ranks of effect sizes and the ranks of the corresponding standard errors. The current significant and positive value means that the magnitude of effect sizes increase, as corresponding standard errors increase (i.e., as studies are more imprecise and smaller). This may be indicative of (publication) bias.
"Classical" regression test (Egger et al., 1997)[a]	$\beta_0 = -2.466$, $p < .001$; $\beta_1 = 0.352$, $p < .001$	The slope (β_1) provides a measure of the asymmetry of the funnel plot and indicates the direction of potential bias. The current significant and positive slope means that the magnitude of effect sizes increase, as corresponding standard errors increase (i.e., as studies are more imprecise and smaller). This may be indicative of (publication) bias.
Regression test (Egger et al., 1997) accounting for effect size dependency[b]	$\beta_0 = 0.254$, $p < .001$; $\beta_1 = 0.583$, $p = .189$	In contrast to the results of the "classical" regression test, the slope (β_1) was not significant, implying that the regression test did not yield evidence for bias when accounting for effect size dependency.
Trim-and-fill analysis (Duval & Tweedie, 2000a, 2000b)	The trim-and-fill algorithm restored symmetry in the funnel plot by imputing 46 effect sizes from 18 studies in the (top) right side of the funnel plot (see Fig. 1).	The "missing" effect sizes were imputed to the right of the summary effect, meaning that bias towards below average findings from relatively precise and large studies may be present (i.e., above average findings from relatively precise and large studies were imputed).
Moderator test of publication year[c]	$\beta_1 = -0.004$, $p = .022$	The significant negative slope indicates a decrease in effect size of r = -0.004 per (later) year of publication, meaning that reported effects decline over time. This supports the presence of a "decline effect", that can be regarded as an indication of bias.
Overall indication for bias	Moderate to strong (5 out of 6 applied methods indicated bias)	The estimated effect may differ from the true effect of intergenerational transmission. Firm conclusions on the presence of bias cannot be drawn given the limitations of the available techniques for assessing bias (see Discussion).

Note.
[a] This test was performed in a weighted regression model with multiplicative dispersion in which the standard error was tested as a predictor of effect sizes (i.e., "a classical" Egger's test; Sterne et al., 2000).
[b] This test was performed in a 3-level meta-analytic model (see Methods section) in which the standard error was tested as a predictor of effect sizes. It is similar to the classical Egger's test, but in this case, effect size dependency was accounted for.
[c] Publication year was tested as a moderator of the summary effect to examine whether (published) effect sizes decline over the years. In other words, we examined the presence of the "decline effect" (Schooler, 2011) and whether the "law of initial results" (Ioannidis, 2005) was applicable.

0.36). Still, almost half of the effect sizes (48.8%; $u = 139$) were from high quality studies ($k = 21$), meeting at least 9 of the 10 criteria for study quality as described in the Method section. Therefore, we decided to estimate the summary effect using only these high quality studies in a post-hoc analysis. The results revealed a significant and moderate effect of $r = 0.240$, $p < .001$, 95% CI [0.166, 0.312], which is equivalent to an odds ratio of 2.452. The difference between this effect and the initial estimated summary effects before and after trim-and-fill were small ($\Delta r = 0.049$) and somewhat larger ($\Delta r = 0.092$), respectively. We found no significant moderating effect for assessment type of parental maltreatment, statistical adjustment for effect sizes, and study design (either longitudinal versus cross-sectional, nor retrospective versus prospective).

3.4.3. Sample descriptors

We found no moderating effect of the gender composition of the sample of parents or the percentage of Caucasians in samples.

4. Discussion

Intergenerational transmission plays an important role in theoretical perspectives on the etiology of child maltreatment, but primary studies on the effect of intergenerational transmission as well as qualitative reviews of these studies (Ertem et al., 2000; Thornberry et al., 2012) have not provided a clear picture. Therefore, our aim was to quantitatively summarize findings of primary studies on the maltreatment cycle to improve our understanding of the true effect of intergenerational transmission of child

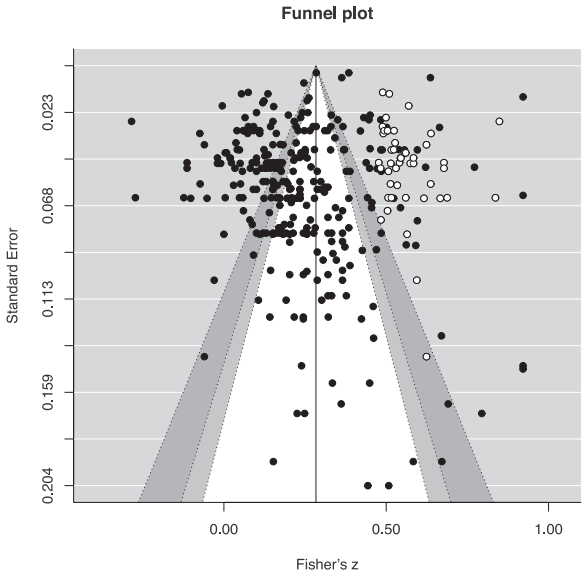

Fig. 1. Funnel Plot.
Note. A contour enhanced funnel plot with Fisher's *z* on the X axis and standard error on the Y axis. The black dots denote the observed effect sizes and the white dots the filled effect sizes. The solid vertical line represents the summary effect. From inside to outside, the dashed lines limit the 90%, 95%, and 99% pseudo confidence interval regions. The imputation of 46 white dots to the right of the summary effect indicates that bias may be present in our results and that above average effect sizes from relatively large and precise studies were possibly underrepresented in the effect sizes that were synthesized.

maltreatment. In pursuing this aim, we conducted a 3-level meta-analysis in which 84 primary studies were included from which 285 effect sizes were extracted.

4.1. Summary effect and bias assessment

Our meta-analysis yielded a significant summary effect of $r = 0.289$, which is medium according to Rice and Harris' (2005) criteria for interpreting effect sizes. Further, this effect falls well within the range representative of medium effect sizes ($0.20 < r < 0.30$) based on a review of empirical studies by Hemphill (2003). The equivalent odds ratio is 2.990 based on the conversion formulas of Borenstein, Hedges, Higgins, and Rothstein (2009) and Rosenthal (1994), implying that in families of parents who experienced maltreatment in their own childhood, the odds of child maltreatment are almost three times the odds of child maltreatment in families of parents without a history of experiencing child maltreatment. In light of the work of Stith et al. (2009), who examined effects of many different risk factors for physical child abuse and neglect, this summary effect is quite substantial and may even be interpreted as relatively large. Our result is in line with the work of Thornberry et al. (2012), who concluded that most of the 47 primary studies included in their review reported findings consistent with the cycle of maltreatment hypothesis. Our effect is also in line with results of Ertem et al. (2000), who found in their review that most of the 10 included studies provided substantial evidence for intergenerational continuity of child physical abuse.

Notwithstanding this substantial summary effect, we acknowledge that not all parents with a history of child maltreatment abuse their own children. Additionally, child maltreatment occurs in families in which the parents have not been maltreated in their own childhood. This implies that protective factors may buffer against the effect of intergenerational transmission, and that a parental history of abuse does not act as a single causal factor for child maltreatment. Other scholars have already pointed out that it is the additive effect of multiple risk factors in different domains, rather than the effect of single risk factors, that underlies child maltreatment (Belsky, 1980, 1993; Cicchetti, Toth, & Maughan, 2000; MacKenzie, Kotch, & Lee, 2011). Based on this notion, Stith et al. (2009) provided an overview of a large range of risk factors for different types of child maltreatment along with estimates of the effects of these factors. Further, Belsky (1980, 1993) noted that the balance between the number of risk and protective factors present in families determines child maltreatment (Belsky, 1980, 1993). In conclusion, child maltreatment is multi-causally determined, and our results suggest that a parental history of child abuse is an important risk factor for child maltreatment.

As there were indications of bias in our results, it should be noted that our estimated effect may differ from the true effect of intergenerational transmission. Considering the asymmetry in the funnel plot, it seemed that in particular above average effect sizes may have been missing in our data, and that our summary effect may be a slight underestimation of the true effect given that the difference between the estimated summary effects before and after the trim-and-fill analysis was only small ($\Delta r = 0.043$). At this point, it must be stressed that no techniques have been developed and tested yet for detecting bias in 3-level meta-analyses. Two major problems of existing techniques are the assumptions of effect size independency and homogeneity of effect sizes (see, for instance, Nakagawa & Santos, 2012; Terrin, Schmid, Lau, & Olkin, 2003), which are often violated in 3-level meta-analytic models.

Table 3
Results of Moderator Analyses (Bivariate Models).

Moderator variables	# Studies	# ES	Intercept (95% CI) / Mean z (95% CI)	Mean r	β_1 (95% CI)	F (df1, df2)[a]	p[b]	Level 2 variance	Level 3 variance
Maltreatment descriptors									
Type of maltreatment experienced by parents						1.324 (4, 280)	.261	.018***	.019***
Maltreatment (type not specified) (RC)	27	65	0.316 (0.261; 0.370)***	0.306					
Physical abuse	48	104	0.307 (0.259; 0.355)***	0.298	−0.008 (−0.066; 0.049)				
Sexual abuse	27	58	0.263 (0.208; 0.319)***	0.257	−0.052 (−0.113; 0.008)+				
Emotional abuse	10	15	0.297 (0.196; 0.398)***	0.289	−0.019 (−0.127; 0.090)				
Neglect	14	43	0.263 (0.202; 0.324)***	0.257	−0.052 (−0.115; 0.010)+				
Type of maltreatment experienced by offspring						3.082 (4, 280)	.012*	.017***	.022***
Maltreatment (type not specified) (RC)	27	60	0.346 (0.285; 0.408)***	0.333					
Physical abuse	42	117	0.251 (0.201; 0.301)***	0.246	−0.095 (−0.165; −0.026)**				
Sexual abuse	15	41	0.341 (0.276; 0.406)***	0.328	−0.006 (−0.085; 0.074)				
Emotional abuse	4	7	0.305 (0.156; 0.454)***	0.296	−0.041 (−0.199; 0.116)				
Neglect	13	60	0.301 (0.241; 0.361)***	0.292	−0.046 (−0.120; 0.029)**				
Study design descriptors									
Assessment type of child maltreatment						24.131 (1, 283)	< .001***	.016***	.022***
No official records (RC)	39	173	0.234 (0.189; 0.283)***	0.230					
Official records	47	112	0.357 (0.310); 0.404)***	0.343	0.123 (0.074; 0.172)***				
Assessment type of parental maltreatment						0.003 (1, 283)	.959	.018***	.020***
No official records (RC)	73	184	0.297 (0.254; 0.339)***	0.289					
Official records	11	101	0.300 (0.196; 0.403)***	0.291	0.003 (−0.107; 0.113)				
Study design						0.675 (1, 283)	.412	.018***	.020***
Cross-sectional (RC)	22	64	0.270 (0.194; 0.346)***	0.264					
Longitudinal	62	221	0.307 (0.260; 0.354)***	0.298	0.037 (−0.052; 0.127)				
Study design						1.924 (1, 283)	.167	.018***	.020***
Prospective (RC)	25	122	0.342 (0.267; 0.418)***	0.329					
Retrospective	60	163	0.280 (0.233; 0.327)***	0.273	−0.062 (−0.151; 0.026)				
Study quality (continuous index)	84	285	0.277 (0.234; 0.320)***	–	−0.027 (−0.045; −0.008)**	8.053 (1, 283)	.005**	.017***	.021***
Statistical adjustment for effect sizes						0.825 (1, 283)	.365	.018***	.020***
No (RC)	29	58	0.324 (0.253; 0.395)***	0.313					
Yes	55	227	0.284 (0.236; 0.333)***	0.277	−0.040 (−0.126; 0.046)				
Sample descriptors									
Gender composition of the sample of parents						0.086 (2, 282)	.918	.018***	.020***
Only mothers (RC)	54	107	0.298 (0.246; 0.349)***	0.289					
Only fathers	11	16	0.278 (0.176; 0.380)***	0.271	−0.020 (−0.122; 0.082)				
Mixed (mothers and fathers)	27	162	0.302 (0.232; 0.372)***	0.293	0.004 (−0.083; 0.091)				
Percentage of Caucasians in samples	16	130	0.249 (0.160; 0.337)***	–	−0.001 (−0.003; 0.002)	0.184	.669	.020***	.016*

Note. # Studies = Number of studies; # ES = Number of effect sizes; Mean z = Mean effect size (Fisher's z); CI = Confidence interval; Mean r = Mean correlation for categories of discrete variables; Level 2 variance = Variance in effect sizes within studies; Level 3 variance = Variance in effect sizes between studies.

[a] Omnibus test of all regression coefficients in the model.

[b] p value of the omnibus test.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

Therefore, the results of the bias analyses must be interpreted with the limitations of these analyses in mind, and the summary effect obtained after trim-and-fill should not be interpreted as the true effect. As a further indication of bias, we found that publication year negatively moderated the summary effect. As such, we found evidence for the "decline effect" (Schooler, 2011) and the "law of initial results" (Ioannidis, 2005), which has also been found in many other research areas. In contrast to above results, this suggests an overestimation rather than an underestimation of our summary effect, which may be caused by the increase in study quality over time, that proved to be associated with smaller effect sizes. In any case, caution is warranted in interpreting the summary effect, as it may not be an unbiased estimate of the effect of intergenerational transmission of child maltreatment.

4.2. Moderator effects

Several moderators of the summary effect were detected. First, we found that the maltreatment type experienced by children moderated the effect. The largest effect was found for maltreatment in general that was not otherwise specified (i.e., any form of child maltreatment), whereas somewhat smaller effects were found for specifically physical abuse and neglect. It is plausible to suggest that the chance of identifying maltreatment increases when child abuse in its broadest sense (not restricted to specific types) is assessed, and decreases when merely one or a few specific types of abuse are assessed. This may explain the larger effect size for maltreatment in general. Although this may also hold for assessing child maltreatment experienced by parents, no moderating effect was found for the abuse type experienced by parents. This implies that a parental history of child maltreatment in general as well as a parental history of specific abuse forms are risk factors of equal strength for the occurrence of child maltreatment. One aspect of which the potential moderating effect could not be examined due to insufficient data was the severity of the maltreatment that parents experienced in their own childhood. It may be hypothesized that the risk of intergenerational continuity may be particularly large when parents were exposed to severe or multiple episodes/types of maltreatment, whereas this risk may be smaller when parents experienced less severe maltreatment. This should be addressed in future research.

Our results do not answer the question whether effects of type-to-type transmissions are different from effects of transmissions to other forms of maltreatment. We decided not to examine this, as the number of effect sizes available for testing the type-to-type transmission of sexual abuse, emotional abuse, and neglect, was only 17, 3, and 14, respectively, which impedes a robust estimate of a type-to-type transmission effect. Moreover, different forms of child maltreatment are interrelated and therefore often occur simultaneously (Higgins & McCabe, 2001). Focusing merely on type-to-type transmissions means ignoring this interrelatedness, and would not provide a clear picture of the role of intergenerational transmission in child maltreatment victimization.

As for study design variables, we found a stronger effect of intergenerational transmission when child maltreatment in adjacent generations was assessed using official records. This may be explained by two difficulties that arise when using self-report methods, such as interviews or questionnaires. First, only children willing to disclose their history of child maltreatment can be identified. Children may have different reasons for not disclosing their maltreatment experiences, for instance, because they have feelings of shame and/or guilt, or they fear an out-of-home placement imposed by youth welfare. Second, it may be difficult for children to recall maltreatment episodes because of the traumatic nature of the experiences, or simply because children were too young at the time of occurrence to remember them later on. On the other hand, researchers found that many occurrences of child maltreatment do not appear in official records (e.g., Fergusson et al., 2000; Finkelhor, 2008; MacMillan et al., 2003). Notwithstanding, above issues may have resulted in an underestimation of the true number of maltreatment occurrences, which in turn influences the magnitude of effect sizes.

Surprisingly, we did not find a moderating effect for the method used in assessing parental experiences of child maltreatment. After all, the issues mentioned above also hold for parents, and we can only speculate about this unexpected finding. It is possible that retrieving official records of maltreatment experienced by parents was more difficult than retrieving official records of maltreatment experienced by children of these parents, because of a less accurate administration of child welfare services in earlier days. Also, definitions of child maltreatment in general and its specific forms are subject to change over time. Parenting practices now regarded as (a type of) child maltreatment may have been acceptable parenting behavior in earlier years. On the other hand, child welfare professionals may have become more sensitive to (different types of) child maltreatment, as child welfare has increasingly professionalized over time (Busschers, Van Vugt, & Stams, 2016). These aspects may limit the accuracy in assessing child maltreatment in parents using official records, and may explain why we found an (almost) equal effect size for official records and self-report methods.

We found that studies of higher quality yielded smaller effect sizes than studies of lower quality, which was a substantial moderator effect. The fact that study quality negatively influences the summary effect has been reported in many other research areas, and implies that – contrary to the results of the trim-and-fill analysis – our summary effect may be an overestimation of the true effect of intergenerational transmission of child maltreatment. This result makes it important to realize that methodological and study design aspects (that are most often study-level aspects) affect our knowledge and understanding of important phenomena, such as child maltreatment transmission. As we also found a negative moderating effect of publication year, it may be that especially in recent years, studies of higher quality with smaller effect sizes were published, producing better estimates of the true effect than earlier studies of lower quality. Ertem et al. (2000) and Thornberry et al. (2012) already expressed their serious concerns about the rather low quality of many primary studies on the generational continuity of child maltreatment, and obviously, this also negatively affects the validity of our results. Consequently, we must conclude that the field is in need of more prospective longitudinal studies of high quality to further clear up the clouded picture of the intergenerational cycle of child maltreatment.

Because primary studies did not comprehensively report on the ethnic composition of samples, we could only test the percentage of Whites/Caucasians in samples as a potential moderator, which obviously is not a thorough test of the influence of ethnic/cultural background on intergenerational transmission of child maltreatment. Moreover, we could extract this information from only 16 of the

84 included primary studies. A similar problem occurred in testing the gender composition of the sample of parents as a potential moderator, as only 11 studies (16 effect sizes) reported results for father samples. For both variables, we did not find a moderating effect, but an absence of a moderating effect must not be interpreted as evidence for no effect (Suchotzki, Verschuere, Van Bockstaele, & Ben-Shakhar, 2017). For a thorough test of these variables as potential moderators, more primary research is needed in which intergenerational transmission of child maltreatment is tested in cross-cultural samples of both fathers and mothers.

4.3. Implications for clinical practice

The current study offers important implications for clinical practice. First, our results show that a parental history of experiencing child maltreatment has substantial predictive value for the occurrence of child maltreatment, and is therefore an important risk factor that needs to be assessed in risk assessment procedures. Accurate risk assessment is essential in identifying children who are at significant risk for maltreatment victimization, so that (preventive) interventions can be offered to the children who are truly in need of care. Risk assessment also informs clinical professionals about the proper level of treatment that is necessary to effectively reduce the risk for future child maltreatment. Valid and reliable risk assessment instruments are required for these purposes, and both existing and future instruments may be improved by including parental experiences of child maltreatment as a risk factor that needs to be assessed. On the other hand, it is important to keep in mind that child maltreatment is a complex phenomenon with multiple causes and that many other risk factors should be assessed as well (see for instance, the risk factors that are described by Stith et al., 2009 and Mulder et al., 2018). Only knowing that a parent has a history of child maltreatment should not be sufficient information to be concerned about a child and start treatment.

Second, the current study shows that the odds of the maltreatment history repeating itself are substantial, but also that there are opportunities to break this cycle, because child maltreatment victimization is not always passed on to the next generation. While there is empirical evidence showing that attachment insecurity (Zuravin, McMillen, DePanfilis, & Risley-Curtiss, 1996), social isolation of parents (Berlin, Appleyard, & Dodge, 2011), young parental age (Valentino, Nuttal, Comas, Borkowski, & Akai, 2012), stress, poverty, parental psychopathology (Dixon, Browne, & Hamilton-Giachritsis, 2009; Egeland, Jacobvitz, & Sroufe, 1988; Sidebotham, Heron, & ALSPAC Study Team., 2006), maternal substance abuse (Appleyard, Berlin, Rosanbalm, & Dodge, 2011), and parents' current violence victimization (Dixon et al., 2009; Renner & Slack, 2006) are associated with intergenerational transmission of child maltreatment, escape from poverty and social support (Dixon et al., 2009), supportive relationships with non-abusive adults both in childhood and adulthood (Conger, Schofield, Neppl, & Merrick, 2013; Egeland et al., 1988; Herrenkohl, Klika, Brown, Herrenkohl, & Leeb, 2013; Jaffee et al., 2013; Thornberry et al., 2013, see Schofield, Lee, & Merrick, 2013, for a meta-analysis) may buffer against intergenerational transmission of child maltreatment. So, in preventing child maltreatment, a parent's history of experiencing child maltreatment should be taken into account next to other risk and protective factors.

It is important for parents with a history of child maltreatment victimization to understand that interacting with their children may trigger unresolved childhood trauma that could cause abusive practices (Goldberg et al., 2003; Main & Hesse, 1990). Even when parents do not want their children to become victims of maltreatment like themselves, child maltreatment victimization may occur by this mechanism. These parents may benefit from EMDR, trauma-focused cognitive behavioral therapy, schema therapy (Ehring et al., 2014; Lobbestael, Arntz, & Sieswerda, 2005), or prolonged exposure therapy and variations of it, such as cognitive processing therapy (e.g., Foa et al., 1999; Resick, Nishith, & Griffin, 2003) in treating consequences of their traumatic experiences.

Third, many parents with a history of child maltreatment victimization did not have a positive parenting example, and they may be at increased risk for insecure internal working models of attachment (Bear & Martinez, 2006), that have been shown to predict later child maltreatment (Huxtable-Jester, 1995; Lo Chan, & Ip, in press). They may know what parental practices they do *not* want to apply in raising their children, but may not know how to apply adequate and effective parenting techniques to establish a secure attachment relationship with their child. Preventive interventions such as the Nurse-Family Partnership (Olds, 2006) or interventions focusing on improving parent-child interactions, such as Parent-Child Interaction Therapy (Abrahamse, Junger, Van Wouwe, Boer, & Lindauer, 2015; Hembree-Kigin & McNeil, 1995), Incredible Years (Webster-Stratton & Reid, 2012), and Video-feedback Intervention to promote Positive Parenting and Sensitive Discipline (Juffer, Bakermans-Kranenburg, & Van IJzendoorn, 2007) have been shown to be effective in improving parenting practices and psychosocial outcomes in children (Hurlbert, Nguyen, Reid, Webster-Stratton, & Zhang, 2013; Juffer, Bakermans-Kranenburg, & Van IJzendoorn, 2016; Kitzman et al., 2010; Thomas & Zimmer-Gembeck, 2011). By applying such interventions, positive parenting strategies of caregivers may be strengthened.

Finally, to prevent generational continuity of child maltreatment, mental health practitioners should always assess whether (new) clients have a history of trauma (for instance because of experiences of maltreatment in their childhood) and whether clients have children, as parental mental health problems are important risk factors for both child maltreatment in general (Stith et al., 2009) and intergenerational transmission of child maltreatment in particular (Dixon et al., 2009; Egeland et al., 1988; Sidebotham et al., 2006; Thompson, 2006). By treating these clients, mental health issues of parents may be relieved and the risk for maltreatment victimization of these clients' children may be reduced. In this way, chances increase that the cycle of maltreatment will be broken.

5. Conclusion

The present meta-analysis revealed a medium and significant summary effect of intergenerational transmission of child maltreatment, that can be regarded as a substantial effect in light of the magnitude of effects of other risk factors for child maltreatment victimization. Therefore, it is important that clinical professionals be aware of the process of intergenerational continuity in child maltreatment. As such, they should pay attention to a parental history of experiencing child maltreatment in both assessment and

treatment, so that prevention of future child maltreatment can be strengthened and chances for breaking the maltreatment cycle increase. Yet, for a better understanding of the maltreatment cycle, primary research of high quality is needed. As already noted by previous researchers (Ertem et al., 2000; Thornberry et al., 2012), studies of high quality examining intergenerational continuity in child maltreatment are generally scarce. Moreover, our review showed that study quality is negatively related to effect size magnitude. Hence, there is an urge for examining the maltreatment cycle in properly conducted studies, and we hope future maltreatment researchers will take this challenge.

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Contributors

Mark Assink participated in designing the study, conducted all statistical analyses, and drafted the manuscript. Anouk Spruit helped drafting the manuscript and critically reviewed the manuscript. Mendel Schuts searched for primary studies and coded all studies. Ramón Lindauer and Claudia E. van der Put critically reviewed the manuscript. Geert-Jan J. M. Stams participated in designing the study, searched for primary studies, coded all studies, critically reviewed the manuscript, and supervised the research project. All authors contributed to and approved the final version of the manuscript.

Conflict of interest

All authors declare that they have no conflicts of interest.

Appendix A. Supplementary data

Supplementary material related to this article can be found, in the online version, at doi:https://doi.org/10.1016/j.chiabu.2018.07.037.

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