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The impact of shared book reading on children's language skills: A meta-analysis

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ABSTRACT

Shared book reading is thought to have a positive impact on young children's language development, with shared reading interventions often run in an attempt to boost children's language skills. However, despite the volume of research in this area, a number of issues remain outstanding. The current meta-analysis explored whether shared reading interventions are equally effective (a) across a range of study designs; (b) across a range of different outcome variables; and (c) for children from different SES groups. It also explored the potentially moderating effects of intervention duration, child age, use of dialogic reading techniques, person delivering the intervention and mode of intervention delivery.

Our results show that, while there is an effect of shared reading on language development, this effect is smaller than reported in previous meta-analyses ($\bar{g}$ = 0.194, p = .002). They also show that this effect is moderated by the type of control group used and is negligible in studies with active control groups ($\bar{g}$ = 0.028, p = .703). Finally, they show no significant effects of differences in outcome variable ($ps \geq .286$), socio-economic status (p = .658), or any of our other potential moderators ($ps \geq .077$), and non-significant effects for studies with follow-ups ($\bar{g}$ = 0.139, p = .200). On the basis of these results, we make a number of recommendations for researchers and educators about the design and implementation of future shared reading interventions.

1. Introduction

Research has shown that poor language skills in the early years can have a far-reaching and long-lasting impact on the child (Hoff, 2013; Pace, Alper, Burchinal, Golinkoff, & Hirsh-Pasek, 2018). Children who enter school with good language skills have better chances at school, better chances of entering higher education and better economic success in adulthood (Blanden, 2006). In contrast, children who have poor language skills at age five are more than twice as likely to be unemployed at age thirty-four than children who have normally developing language at age five (Law, Rush, Schoon, & Parsons, 2009). Research has suggested that rates of language delay are linked to socio-economic status, with higher rates for children from disadvantaged backgrounds. Children as young as 18 months show differences in their vocabulary size and language processing abilities based on their family socio-economic

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status (Fernald, Marchman, & Weisleder, 2013; McGillion, Pine, Herbert, & Matthews, 2017). This social gradient in language development increases over the preschool years. For example, Locke, Ginsborg, and Peers (2002) found that more than half of children starting nursery school in disadvantaged areas of England had delayed language skills despite their general cognitive abilities being in the average range for their age. Similarly, Waldfogel and Washbrook (2010) reported that at school entry, low-income children are 16 months behind their high-income peers in terms of vocabulary size. Relatedly, research has indicated that vocabulary at age five is the best predictor of whether a child from a socially disadvantaged family will manage to "buck the trend" and escape poverty in later adult life (Blanden, 2006).

Given the significant impact that poor language skills can have on a child's life and the link between rates of language delay and socio-economic status, the need for language interventions that are accessible and effective for all socio-economic groups is stark. One such intervention, which has been shown to support children's early language development, is shared book reading.

Shared book reading refers to the practice of sharing or reading a book with a child. This includes sharing books with preschool children before they have begun to read themselves and reading books with older children, whether individually or in groups. However, it excludes other related forms of activity such as storytelling in the absence of a book, engaging in decontextualized talk, reminiscing about past events or talking about the future. There are many forms of shared book reading. For example, interactive (or dialogic) reading involves using a specific set of techniques to scaffold an interaction or conversation between the adult and child about the book. Other forms of shared reading target specific language skills, such as vocabulary or phonological awareness, and use the book as a tool to explicitly or implicitly teach the child these skills.

Research has indicated that shared book reading can support a range of early language skills including vocabulary (e.g. Elley, 1989; Farrant & Zubrick, 2011), narrative and conversation skills (e.g., Morrow, 1988; Reese, 1995), future reading ability (e.g., Bus, van IJzendoorn, & Pellegrini, 1995), print awareness (e.g., Justice & Ezell, 2000, 2004), grammatical development (e.g., Valdez-Menchaca & Whitehurst, 1992; Whitehurst et al., 1988) and phonological awareness (e.g., Chow, McBride-Chang, Cheung, & Chow, 2008; Lefebvre, Trudeau, & Sutton, 2011). On the basis of this research, a great deal of emphasis has been placed on the importance of caregivers and practitioners reading with children in the early years to support language development and school readiness. However, despite the considerable volume of research in this area, a number of issues remain outstanding.

First, it is unclear whether the size of the effects reported for shared book reading interventions is exaggerated as a consequence of weak study design. Studies without a control group and/or without a pre-intervention or baseline assessment are common in the field of shared reading and have been included in previous meta-analyses (e.g., Flack, Field, & Horst, 2018). However, this kind of design does not allow us to isolate the impact of the specific intervention from other variables that could influence the child's performance. For example, maturation, non-specific effects of being included in an intervention group, and pre-existing differences in ability between the control and intervention groups could all account for any improvement seen in the intervention group. The implication is that studies with this kind of design do not definitively show that any improvement in child outcomes is due to the intervention the child received.

Of the studies that have a control group, some studies include a passive, "business-as-usual" control group and other studies include an active, alternative treatment control group. Similarly, some studies randomly allocate participants to groups and other studies use a non-random form of allocation. Previous reviews and meta-analyses in other domains have indicated that the type of control group and the form of allocation can moderate the effect of the intervention. Smaller effect sizes are found when active control groups and random allocation are used compared to passive control groups and non-random allocation (Sala & Gobet, 2017; Simons et al., 2016). Given the findings from other domains, and the varying quality of studies included in previous meta-analyses, it remains unclear whether shared reading will be shown to impact on language outcomes when only studies with high quality research designs are included.

Second, although there is evidence from individual studies for the positive impact of shared reading on a range of language skills, reviews that have synthesised the evidence have suggested that the evidence base for some language skills is stronger than the evidence base for others. For example, the What Works Clearinghouse has produced two reports on shared reading, both of which have questioned the strength of evidence for shared reading interventions on some language skills (U.S. Department of Education, Institute of Education Sciences, What Works Clearinghouse, 2007; 2015). In a 2007 report on dialogic reading, the authors concluded that dialogic reading has a positive impact on oral language skills but "no discernible effects" (p. 1) on phonological processing. Similarly, in a 2015 report on shared reading more generally, the authors concluded that shared reading had "no discernible effects" (p. 2) on alphabetics or reading achievement and mixed results for language comprehension and language development. In summary, these reports, which include only the highest quality relevant research, question the claim that shared reading supports the full range of language skills, and instead indicate that the efficacy of shared reading interventions may vary depending on the specific language outcome.

Third, it is unclear whether shared reading is equally effective for children from different socio-economic backgrounds. Many studies have investigated the role of shared reading as a method of improving the language skills of children from disadvantaged backgrounds (e.g., Korat, Shamir, & Heibal, 2013; Valdez-Menchaca & Whitehurst, 1992; Whitehurst et al., 1988). These studies have been motivated by the higher rate of language delay in disadvantaged children and the desire to close this language gap. These individual studies have generally reported positive outcomes. However, when meta-analytic methods have been used to synthesise the results, research has indicated that socio-economic status moderates the effects, with smaller effect sizes for children from disadvantaged backgrounds (Manz, 2010; Mol, Bus, de Jong, & Smeets, 2008). Given the need for interventions that close the language gap, rather than increase it, these findings have potentially important implications.

Fourth, given the heterogeneity of the intervention studies reported in the literature, a number of questions remain about the factors that influence the effectiveness of shared book reading interventions. For example, what is the relation between the duration

of book reading interventions and their effectiveness? Are such interventions equally effective in promoting the language skills of preschool and school-aged children? Are interventions that involve the use of dialogic reading techniques associated with better outcomes than interventions that do not? And does it make a difference whether the intervention is delivered to the child by a researcher, a teacher or a parent? Or whether it is delivered to the child on a one-to-one basis or as part of a group? The answers to these questions are of both practical and theoretical importance since they not only have implications for the design of future interventions, but also for our understanding of how shared book reading interventions achieve their effects.

As discussed above, there have been a number of previous meta-analyses that have investigated the relationship between shared reading and language development. However, these meta-analyses do not allow us to draw definitive conclusions about the impact of shared reading on language skills for a number of reasons. First, Manz, Hughes, Barnabas, Bracaliello, and Ginsburg-Block (2010) and Mol et al. (2008) are relatively old and require updating due to the significant amount of research that has been carried out since the publication of these meta-analyses. These two meta-analyses include only 14 and 16 studies, respectively. Second, previous meta-analyses have included studies that did not include a control group (e.g., Flack et al., 2018), or studies that did not include a pre-intervention assessment (e.g., Mol et al., 2008). These studies may over-estimate the impact of shared reading on language development and should therefore be interpreted with caution. Third, previous meta-analyses have been limited in scope in terms of the outcome variables (e.g., Flack et al., 2018; Mol et al., 2008) and the types of shared reading on which they have focused (e.g., Mol et al., 2008). These meta-analyses therefore do not tell us whether shared reading in all its forms has a positive impact on a range of different outcome variables. Finally, Fitton, McIlraith and Wood's (2018) meta-analysis provides a robust analysis of the effect of shared reading on the language skills of children learning English as an additional language. However, although it addresses a significant gap in the literature, it focuses only on this specific subgroup of children. It therefore does not allow conclusions to be drawn about the impact of shared reading on the language skills of monolingual children. In addition, this meta-analysis reports a high amount of unexplained between-study true variance in most of its models, which makes the interpretation of the results problematic. Notably, at least part of this variance is probably a statistical artefact due to the fact that no correction for clustered samples (e.g., classes, schools, etc.) is applied in Fitton and colleagues' meta-analysis (or any other meta-analysis in the field).

In summary, in addition to uncertainty about the moderating effects of type of outcome variable, SES and a number of other factors, there are issues with the scope and quality of previous reviews and meta-analyses of shared book reading interventions, and a need to update several of the earlier analyses to take account of more recent research in the area. The current meta-analysis will address these issues by investigating the effects of the following moderators: (a) type of control group; (b) type of group allocation; (c) type of outcome variable; (d) socio-economic status; (e) duration of the intervention; (f) age of the child; (g) whether the intervention involved dialogic reading; (h) whether the intervention was delivered by a researcher, teacher or parent; and (i) whether the intervention was delivered on a one-to-one basis or in a group. It will also include a number of studies that have not been analysed in previous syntheses. The meta-analysis will use more stringent inclusion criteria than previous meta-analyses, only including studies that have a control group and a pre-post intervention design. It will also use up-to-date meta-analytic techniques to control for the effects of statistical dependence of effect sizes, publication bias, influential cases, and artefacts due to the nested structure of the data in the primary studies.

2. Method

2.1. Literature search

In line with the PRISMA statement (Moher, Liberati, Tetzlaff, & Altman, 2009), we performed a systematic search of Education Research Complete, ERIC, PsycINFO, JSTOR Journals, Medline Full Text, and ProQuest Dissertation & Theses databases to identify all potentially relevant studies. We used the following search string, “book reading” OR “storybook reading” OR “shared reading” OR “interactive reading” OR “dialogic reading” OR “vocabulary instruction” OR “vocabulary intervention*” OR “vocabulary growth” OR “teaching vocabulary” OR “vocabulary learning” OR “teaching word” OR “listening to stories” OR “read aloud” OR “recast*.”

We also examined the reference lists from previous studies to identify additional studies and we emailed researchers (n = 100) to request data. We received 39 responses, of which 14 were positive.

2.2. Inclusion criteria

The following criteria were used to assess the eligibility of studies:

1. The study contained a universal and/or targeted shared book reading intervention. This included interventions involving the use of dialogic reading techniques to support language learning in general and interventions that used book reading to target specific language skills; it also included interventions with preschool or school age children, interventions delivered by researchers, teachers or parents, and interventions delivered on a one-to-one basis or interventions delivered to children in groups;
2. The study contained at least one control group. This included studies with passive or “business as usual” control groups and studies with active control groups, including control groups exposed to non-language-oriented interventions such as structured play and visuo-motor skills training; and control groups exposed to alternative language-oriented interventions such as joint writing, phonological awareness training and elaborative reminiscing.
3. The participants in the study were typically developing children aged 7 years or younger;
4. The study did not target multilingual populations and/or the acquisition of an additional language;

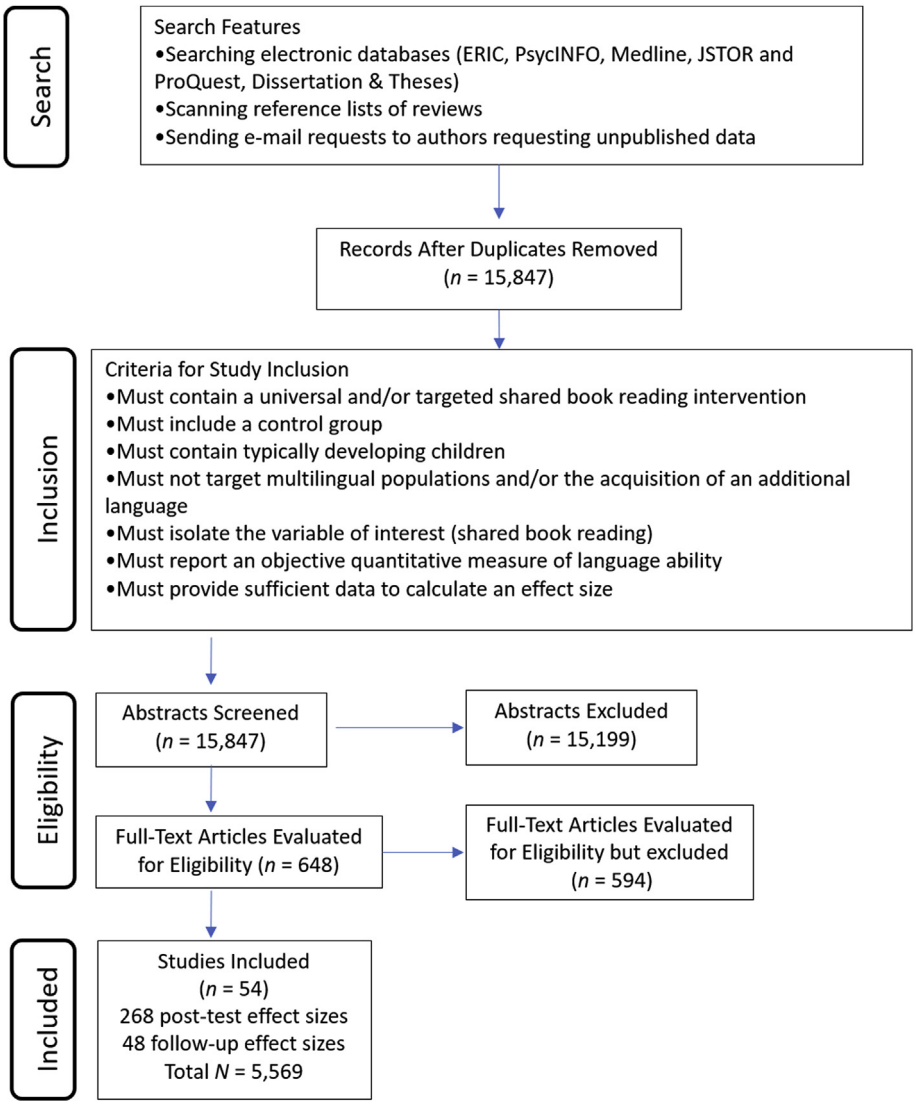

Fig. 1. Flow diagram of the studies included in the meta-analyses.

5. The study design allowed the isolation of the variable of interest, that is, shared book reading. We thus excluded studies in which both the treatment and control group received a shared reading intervention;
6. The study contained at least one language outcome measure that yielded an objective quantitative score. These outcome measures included scores on standardized language tests and scores on author-designed assessments, such as tests of children's knowledge of the vocabulary to which they had been exposed in the course of the intervention.
7. The data within the study, or the data provided by the authors, were sufficient to calculate an effect size.

We searched for relevant published and unpublished articles from as early as 1880 through to September 2017. This search yielded 54 studies that met the inclusion criteria detailed above, all from the period between 1989 and 2017 (the list of the included studies is provided in the supplemental materials). These studies included 316 effect sizes and 5569 participants. Nine of the studies reported follow-up effects. The dataset with the raw data and all the details of the included studies can be found in the Supplemental materials available online. Fig. 1 summarises the process.

2.3. Moderators

We coded 11 potential moderators. Three moderators concerned the design quality of the study:

1. Allocation (dichotomous variable): Whether the participants were randomly allocated to the groups.

2. Type of control group (active or passive; dichotomous variable): Whether the shared reading intervention group was compared to another activity/intervention or to a no-contact (or "business-as-usual") control group.
3. Baseline difference (continuous variable): The standardized mean difference (Hedges' *g*) between the experimental and control groups at baseline. This moderator was added to control for possible statistical artefacts due to regression to the mean.

The other eight moderators examined the characteristics of the training programs and participants:

4. Outcome measure (categorical variable): This moderator included expressive vocabulary (EV), receptive vocabulary (RV), phonological awareness (PA), print concepts (PC), expressive language (EL), and miscellaneous (M), with this last category used to classify composite outcome measures such as the Preschool Language Scale (Zimmerman, Steiner, & Pond, 2011), which collapse across two or more of the above categories. Two authors coded each effect size for outcome measure independently. The Cohen's kappa was $\kappa = 0.83$, 95% CI [0.80; 0.86]. The two authors resolved each discrepancy through discussion.
5. Language skill vs. Precursor to reading skill (categorical variable): whether the outcome variable measured a language skill (expressive vocabulary, receptive vocabulary, or expressive language) or a precursor to reading skill (phonological awareness or print concepts). This moderator was derived from the previous moderator (Outcome measure).
6. Socio-economic status (SES; categorical variable): This moderator included two levels; middle/high and low SES. Studies that made no mention of SES or included a sample that contained participants from a range of socio-economic backgrounds were excluded from the SES moderator. This moderator was tested in a separate analysis because not all the studies reported this information. Two authors coded the studies reporting information about the participants' SES (57% of the total sample) independently. The Cohen's kappa was $\kappa = 0.81$, 95% CI [0.61; 1.00]. The two authors resolved each discrepancy through discussion.
7. Intervention duration (continuous variable): the time in weeks over which the intervention was delivered.
8. Age (preschool vs. school age; dichotomous variable): Whether the target children were under 5 years old or between 5 and 7 years old at the start of the intervention;
9. Dialogic Reading (dialogic vs. non-dialogic; dichotomous variable): Whether the intervention involved the use of dialogic reading techniques to scaffold an interaction or conversation between the adult and child about the book.
10. Person delivering the intervention (categorical variable): Whether the intervention was delivered by a researcher, a teacher, a parent, or a teacher and a parent combined.
11. Mode of delivery (one-to-one vs. group; dichotomous variable): Whether the intervention was delivered to the child on a one-to-one basis or as part of a group.

2.4. Effect size and sampling error variance calculation

The effect size used in all the meta-analyses was the corrected standardized mean difference (i.e., Hedges' *g*; Hedges & Olkin, 1985). The effect size represented the improvement of the experimental groups over the controls immediately after the end of the training or at follow-up assessment. The formula for the effect size was:

$$g = \frac{(M_{e\_post} - M_{e\_pre}) - (M_{c\_post} - M_{c\_pre})}{SD_{pooled\_pre}} \times \left(1 - \frac{3}{(4 \times N) - 9}\right) \quad (1)$$

that is, the post-pre between-group mean difference standardized by the pooled pre-test standard deviations and corrected for upward bias. The formula for the sampling error variance was:

$$Var_g = \frac{N_e - 1}{N_e - 3} \times \left(\beta + \frac{g_e^2}{2} \times \frac{N_e}{N_e - 1}\right) \times \frac{1}{N_e} + \frac{N_c - 1}{N_c - 3} \times \left(\beta + \frac{g_c^2}{2} \times \frac{N_c}{N_c - 1}\right) \times \frac{1}{N_c} \quad (2)$$

with

$$\beta = \frac{2 \times (1 - r)}{r_{xx}} \quad (3)$$

where r_{xx} is the pre-post-test reliability coefficient of the measure, N_e and N_c are the sizes of the experimental group and the control group, g_e and g_c are the within-group standardized mean differences of the experimental group and the control group, and r is the pre-post-test correlation (Hedges & Olkin, 1985; Schmidt & Hunter, 2015, pp. 343–355). Since the pre-post-test correlations and reliability coefficients were rarely provided in the primary studies, we fixed the value of β to 1.[1]

Some studies reported a multi-site intervention. In these cases, we applied a correction for the nested structure of the data. First, we multiplied the effect size by the following factor (Hedges, 2007):

[1] We replicated the analyses using different values of pre-post-test correlations and reliability coefficients (range 0.30–0.80). Only negligible differences were found.

$$\sqrt{1 - \frac{2 \times (n-1) \times \rho}{N-2}} \quad (4)$$

where n is the cluster mean sample size and ρ is the intra-class correlation. The intra-class correlation was rarely provided in the primary studies and was thus fixed at 0.233 (Hedges & Hedberg, 2007). Then, we multiplied the sampling error variance by the following factor (Higgins, Deeks, & Altman, 2008):

$$1 + (n-1) \times \rho \quad (5)$$

2.5. Modeling approach

We calculated the effect sizes for all the relevant dependent variables reported in the primary studies. Some of the effect sizes included were statistically dependent. We used *robust variance estimation* (RVE) with hierarchical weights and small-sample corrections to calculate the overall effect size and run meta-regression analysis (Hedges, Tipton, & Johnson, 2010). RVE models statistically dependent effect sizes and calculates robust standard errors. RVE also provides estimates of the within-cluster true (i.e., not due to sampling error) variance and between-cluster true variance components (ω^2 and τ^2, respectively). We thus grouped all the effect sizes extracted from the same study into one cluster. We performed the analyses with the Robumeta software R package (Fisher, Tipton, & Zhipeng, 2017).

2.6. Sensitivity analysis

To test the robustness of the findings, we ran Viechtbauer and Cheung's (2010) influential case analysis (Metafor R package; Viechtbauer, 2010). This analysis detected those effect sizes that exerted an unusually pronounced influence on the overall effect sizes. The meta-analytic models were thus run both with and without influential effect sizes.

Once the influential effect sizes were removed, we employed Cheung and Chan's (2014) individual-samplewise correction to merge the effect sizes extracted from the same paper. (For details on the procedure, see the Supplemental material available online.) Then, we ran a random-effect model with the Restricted Maximum Likelihood estimator (REML) in all the models with 20 or more observations. When the observations were less than 20, we used the Hunter-Schmidt estimator (HS; Schmidt & Hunter, 2015, pp. 420–423). We then performed several publication bias analyses. First, we ran Henmi and Copas' (2010) method to examine whether true variance inflated the overall effect size. Then, we used the "precision effect test" (PET) and "precision-effect estimate with standard error" (PEESE; Stanley & Doucouliagos, 2014). When the PET detects the presence of a non-zero effect at a 10% significance level ($p < .100$, one-tailed; Stanley, 2017), the PEESE estimate is usually preferred. (It is worth noting that selection methods [e.g., McShane, Böckenholt, & Hansen, 2016; Vevea & Woods, 2005; Simonsohn, Nelson, & Simmons, 2011] are not suitable when statistically dependent effect sizes are merged because the original *p*-value distribution is disrupted.)

2.7. Follow-up effects

Together with data referring to immediate post-test performance, some primary studies reported follow-up effects. The relevant effect sizes were calculated by replacing the pre-post-test gains with the difference between the follow-up mean minus the pre-test mean in formula (1). Due to the small number of effect sizes, we did not run any sensitivity analysis in follow-up models.

3. Results

3.1. Post-test effects

3.1.1. Preparatory analysis

This RVE model included all the effect sizes related to pre-post-test training-induced effects. The overall effect size was $\bar{g} = 0.278$, $SE = 0.064$, 95% CI [0.144; 0.412], $m = 54$, $k = 268$, $df = 20.95$, $p < .001$, $\omega^2 = 0.000$, $\tau^2 = 0.143$. This analysis showed that the overall standard error and the between-cluster component were quite large. This was due to a few effect sizes that were too large to be realistic (e.g., $g = -29.159$ and $g = 8.116$). We thus applied the following limiter, that is, $|g| < 3.000$,[2] to all the models. Seventeen effect sizes were excluded, most of which ($n = 13$) were from the same study (i.e., Elmonayer, 2013).

3.1.2. Main model

After removing the excessively large effects from the dataset, the overall effect size was $\bar{g} = 0.237$, $SE = 0.056$, 95% CI [0.118; 0.357], $p < .001$, $df = 16.82$, $\omega^2 = 0.000$, $\tau^2 = 0.029$, $k = 53$, $m = 251$. As predicted, the measure was more accurate ($SE = 0.056$ instead of $SE = 0.064$), and the total true variance was heavily reduced ($\omega^2 + \tau^2 = 0.029$ instead of 0.143). We ran a meta-regression model including the three moderators concerning the design quality of the studies. Baseline difference and Control group were the

[2] Not applying this limiter yields very similar results after sensitivity analysis. These analyses are reported in the Supplemental materials available online.

only statistically significant moderators ($b = -0.332$, $p = .020$ and $b = 0.234$, $p = .012$, respectively) and explained nearly all the observed true variance ($\omega^2 = 0.000$ and $\tau^2 = 0.010$).

Seven influential cases were detected and excluded. The overall effect size was $\overline{g} = 0.194$, $SE = 0.049$, 95% CI [0.088; 0.299], $p = .002$, $df = 13.79$, $\omega^2 = 0.000$, $\tau^2 = 0.000$, $k = 52$, $m = 244$. Thus, excluding the influential cases did not meaningfully affect the overall effect size ($\overline{g} = 0.194$ instead of $\overline{g} = 0.237$). After removing the influential cases, Baseline difference and Control group remained the only statistically significant moderators ($b = -0.287$, $p = .044$ and $b = 0.271$, $p = .012$, respectively; $\omega^2 = 0.000$, $\tau^2 = 0.000$).

The overall effect size in those studies comparing treated samples with passive control groups was $\overline{g} = 0.263$, $SE = 0.056$, 95% CI [0.142; 0.384], $p < .001$, $df = 13.54$, $\omega^2 = 0.000$, $\tau^2 = 0.000$, $k = 43$, $m = 174$. When active comparisons were used, the overall effect size was $\overline{g} = 0.028$, $SE = 0.067$, 95% CI [-0.157; 0.212], $p = .703$, $df = 4.13$, $\omega^2 = 0.000$, $\tau^2 = 0.000$, $k = 12$, $m = 70$.

The other eight moderators were examined one by one. No moderator was significant, either with or without influential cases. Regarding Outcome measure, none of the 15 pairwise comparisons reached statistical significance after applying the Holm's correction for multiple comparisons (all $ps \geq .286$). No significant difference was found between language skills and precursors of language skills ($p = .413$ and $p = .077$, with and without influential cases, respectively). The same applies to the SES of the participants ($p = .591$ and $p = .658$), duration of the intervention ($p = .257$ and $p = .960$), the age of the participants ($p = .632$ and $p = .848$), whether the study implemented a dialogic reading intervention ($p = .579$ and $p = .370$), who administered the intervention (all $ps \geq .675$), and the mode of delivery ($p = .583$ and $p = .774$).

3.1.2.1. Publication bias analysis. After merging the effect sizes with Cheung and Chan's (2014) method, we ran the random-effect models. The overall effect size of the main model was $\overline{g} = 0.184$, $SE = 0.043$, 95% CI [0.099; 0.269], $p < .001$, $\tau^2 = 0.010$, $k = 52$. The Henmi and Copas (2010) corrected estimate was $\overline{g} = 0.140$, $SE = 0.032$, 95% CI [0.083; 0.197], $p < .001$, $\tau^2 = 0.000$. PET and PEESE estimates were $\overline{g} = 0.046$, $SE = 0.036$, $p = .210$ and $\overline{g} = 0.121$, $SE = 0.028$, $p < .001$.

The overall effect size in those studies comparing treated samples with passive control groups was $\overline{g} = 0.238$, $SE = 0.051$, 95% CI [0.137; 0.338], $p < .001$, $\tau^2 = 0.011$, $k = 43$. The Henmi and Copas (2010) corrected estimate was $\overline{g} = 0.202$, $SE = 0.040$, 95% CI [0.131; 0.273], $p < .001$, $\tau^2 = 0.000$. PET and PEESE estimates were $\overline{g} = 0.106$, $SE = 0.047$, $p = .030$ and $\overline{g} = 0.182$, $SE = 0.034$, $p < .001$, respectively. With active control groups, the overall effect size was $\overline{g} = -0.011$, $SE = 0.058$, 95% CI [-0.124; 0.102], $p = .846$, $\tau^2 = 0.000$, $k = 12$. Given the small number of effect sizes and the near-zero overall effect size, no publication bias analysis was run.

3.2. Follow-up effects

All the follow-up effect sizes fell into the range $-3.000 < g < 3.000$. Thus, no effect size was excluded. The overall effect size was $\overline{g} = 0.132$, $SE = 0.035$, 95% CI [-0.035; 0.299], $p = .075$, $df = 1.83$, $\omega^2 = 0.000$, $\tau^2 = 0.000$, $k = 9$, $m = 48$. When $df < 4$, the results may not be reliable (Fisher et al., 2017). Nonetheless, the random-effect model run with the merged effects provided very similar results, $\overline{g} = 0.139$, $SE = 0.109$, 95% CI [-0.074; 0.352], $p = .200$, $\tau^2 = 0.000$, $k = 9$.

4. Discussion

This meta-analytic review investigated the effects of shared book reading on children's language skills. The overall impact appears modest. Once the few outliers and influential cases are excluded, the overall effect size is small ($\overline{g} = 0.194$, $p = .002$). Furthermore, publication bias analysis suggests that the real effect may be even smaller (corrected $\overline{g}$ s ranging between 0.046 and 0.140).

Meta-regression analysis shows that differences at baseline and, most importantly, differences in type of control group account for all the observed true variance ($\omega^2 = 0.000$, $\tau^2 = 0.000$). Consequently, no other moderator has been found to significantly affect the impact of shared book reading on the children's language skills. While the comparison with passive controls yields a reliable (e.g., Henmi & Copas' corrected estimate $\overline{g} = 0.202$, $p < .001$) impact on the participants' language skills, the effects are nearly null when active controls are employed ($\overline{g} = 0.028$, $p = .703$). This finding suggests that the effect of shared book reading on language skills documented in the literature to date may be best understood as a non-specific effect. This conclusion is also consistent with other outcomes, such as the absence of any difference between types of language skill and the non-significant and highly consistent overall effect at follow-up ($\overline{g} = 0.139$, $p = .200$, $\tau^2 = 0.000$). In short, all the available evidence seems to suggest that the observed effects (where there are any) are non-specific and temporary.

4.1. Previous meta-analyses

The current meta-analysis reports a small overall effect size, which diverges from previous meta-analyses, which have reported medium effect sizes (e.g., Flack et al., 2018; Mol et al., 2008). However, there are fundamental differences between the present and previous meta-analyses, which may account for this difference in effect size. First, previous meta-analyses have included studies without control groups (e.g., Flack et al., 2018) and studies without a pre-intervention assessment (e.g., Flack et al., 2018; Mol et al., 2008). As discussed in the introduction, studies with these designs do not allow isolation of the impact of the specific intervention from other variables that could influence the child's performance. They may therefore over-estimate the impact of shared reading interventions, and including them in meta-analyses may inflate the effect size reported.

Second, the current meta-analysis includes a broader range of outcome measures (e.g., Flack et al., 2018; Mol et al., 2008) and a

wider range of shared reading interventions (e.g., Mol et al., 2008) than previous meta-analyses. The aim of the present meta-analysis was to determine whether shared reading in its many forms has an impact on a broad range of outcome measures, rather than to determine the impact of one specific shared reading intervention (e.g. dialogic reading) on one specific outcome variable (e.g. receptive vocabulary). Taken together, these factors may account for the more modest effect of shared reading reported in the present meta-analysis. This conclusion is consistent with the recent meta-analysis by Fitton, McIlraith, and Wood (2018) on shared book reading and language and literacy skills of children learning English as an additional language. Fitton et al. (2018) included a broad range of shared reading interventions and outcome measures in the meta-analysis, and also considered the methodological rigour of previous studies, and report a similar effect size ($\bar{g}$ = 0.27 for passive control groups) to the present study, despite focusing on a different population.

In addition to a more modest effect size for studies with passive controls, the current meta-analysis also reports a near-zero effect when studies with active control groups are considered. As discussed in the introduction, interventions with "business-as-usual" passive control groups do not allow one to distinguish between effects of the specific content of the intervention and non-specific effects that are simply a consequence of being subject to some form of intervention. In order to rule out such non-specific effects, intervention studies need to include active control groups. However, reading interventions with active control groups appear to result in zero or near-zero effect sizes. Previous meta-analyses in other domains have reported similar results to the present study, with smaller effect sizes when active rather than passive control groups are used (Sala & Gobet, 2017; Simons et al., 2016). To the best of our knowledge, this is the first meta-analysis of shared reading interventions that has considered the role of type of control group. Consequently, it is the first to suggest that the impact of shared book reading on language skills may reflect non-specific effects.

Taken together, the findings of the present meta-analysis suggest that current evidence for the effectiveness of shared reading interventions is much weaker than was previously thought. On the face of it, this conclusion would appear to raise serious doubts about the effectiveness of shared book reading interventions in increasing children's language skills. However, there are at least two reasons why it may be premature to dismiss the potentially language-boosting effects of shared book reading interventions.

First, there are good theoretical reasons for believing that shared book reading interventions may have language-boosting effects, if delivered at a realistic dosage. Research has shown that shared book reading tends to include a number of potentially language boosting behaviours which have been linked to positive language outcomes in the past. For example, shared book reading tends to result in child directed speech with higher levels of lexical and syntactic diversity than play-based interaction (Cameron-Faulkner & Noble, 2013; Noble, Cameron-Faulkner, & Lieven, 2018), and high levels of lexical and syntactic diversity in child-directed speech have been linked to higher levels of lexical and syntactic diversity in children's speech (Huttenlocher, Vasilyeva, Cymerman, & Levine, 2002). Shared book reading is also likely to foster high levels of joint attention, contingent talk and responsiveness, which have also been linked to positive language outcomes (Carpenter, Nagell, Tomasello, Butterworth, & Moore, 1998; Farrant & Zubrick, 2013; McGillion et al., 2017; Tomasello & Farrar, 1986). It also provides ample opportunities to use techniques such as expanding, recasting and asking open-ended questions, all of which have been shown to be positively related to children's oral language development (Baker & Nelson, 1984; Cleave, Becker, Curran, Van Horne, & Fey, 2015; Farrar, 1990; Girolametto & Weitzman, 2002; Huttenlocher et al., 2002; Huttenlocher, Waterfall, Vasilyeva, Vevea, & Hedges, 2010; Nelson, 1977). While much of this evidence is only correlational, it would be surprising if interventions that included so many potentially language-boosting ingredients had no discernible effect on children's language outcomes.

Second, while the current meta-analysis suggests that previous intervention studies may have been less effective than was previously thought, it is important to be aware that many of these studies involved relatively low-dose interventions. In fact, the most common duration of the shared reading interventions in the present meta-analysis was between 6 and 8 weeks (e.g., Lonigan & Whitehurst, 1998; Whitehurst et al., 1994). There is good reason to question whether this level of dosage is sufficient to result in measurable and long-lasting effects. For example, much of the longitudinal research that has indicated a positive relationship between shared reading and language outcomes spans much longer periods of time, typically several years (e.g., DeBaryshe, 1993; Farrant & Zubrick, 2013). It is therefore probably unrealistic to expect long-term, positive effects on language outcomes from book reading interventions that last only a few weeks. Indeed, one might argue that small non-specific effects that do not last to follow-up are about as much as one would expect on the basis of such low-dose interventions.

For these reasons, we caution against dismissing shared book reading as a potentially language-boosting form of intervention and, instead, make a series of recommendations for future research, which will make it possible to determine whether shared reading interventions have a positive impact on language outcomes.

1. We recommend that future interventions use a higher dosage (e.g. 6–12 months as opposed to 6–8 weeks). This will allow a more realistic test of the hypothesis that shared book reading interventions have a positive effect on children's language outcomes.
2. We recommend that future studies use active control groups with carefully chosen non-language promoting alternative treatments (e.g. engaging children in physical exercise, practicing fine motor skills). Using active control groups with non-language promoting alternative treatments will allow researchers to test for specific effects of book reading over non-specific effects of simply being subjected to some form of intervention.
3. We recommend that future studies include follow-up testing to investigate whether the effects of shared book reading interventions extend beyond the period of the intervention and hence have measurable long-term effects.
4. We recommend that future studies include a range of outcome variables to investigate whether shared reading has different effects on different types of language outcome.
5. We recommend that future studies include children from a range of SES backgrounds to investigate whether shared reading interventions are more or less effective in different SES groups.

4.2. Conclusion

In the current meta-analysis, we show that, while there is an effect of shared reading on language development, this effect is smaller than reported in previous meta-analyses ($\bar{g} = 0.194$, $p = .002$) and that the effect is near zero when active control groups are used ($\bar{g} = 0.028$, $p = .703$). The meta-analysis also indicates no differences between type of language outcome, no effect of SES, and no significant effect at follow-up. Taken together, these findings suggest that current evidence for the effectiveness of shared reading interventions is much weaker than was previously thought and may reflect non-specific effects. However, given the low dosage of many of the studies included in the present meta-analysis, we caution against the conclusion that shared reading interventions have no real benefits and make a series of recommendations for the design of future studies. These will allow researchers to determine whether shared book reading interventions have any real impact on early language skills, as well as to determine how any such impact varies as a function of outcome variable and participant SES.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.edurev.2019.100290.

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