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Review

Features of digital media which influence social interactions between adults and children aged 2–7 years during joint media engagement: A multi-level meta-analysis

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A B S T R A C T

This study reviewed research on the features of digital media (e.g. apps, e-books) which influence interactions between adults and children aged 2–7 years when using these media together. We focused on interactions which support child learning, particularly oral language development. We used robust variance estimation to conduct multilevel meta-analyses of 15 experimental studies ($n = 627$ parent–child pairs; $k = 190$ effects). Findings suggest that digital design can shape adult-child interactions ($g = 0.56$, $k = 170$), particularly the quality of parental language input ($g = 1.1$, $k = 86$). Embedding conversation prompts into e-books showed particular promise ($g = 0.84$–0.99, $k = 58$–74). Though small in scope, this study offers direction for media design and research and indicates promise for low-cost intervention via digital design. However, it also indicates a need for more robust and well-powered research to inform design, practice and policy. In particular, better evidence is needed to establish whether the benefits identified for adult-child interaction translate into benefits for wider child and adult outcomes.

1. Introduction

Digital media are now a ubiquitous feature of children's everyday lives. In the UK, for example, 87% of 3–4-year-olds used online digital activities in 2022 (Ofcom, 2023). There is evidence that such activities can benefit young children's learning. Interactive apps[1] can support numeracy and literacy development (Griffith et al., 2020; Kim et al., 2021), and e-books with content-relevant enhancements can elicit better reading comprehension and word learning than paper books for children aged 1 to 8 (Furenes et al., 2021).

However, important caveats exist. Firstly, e-books are less effective than paper books in supporting reading comprehension for young children from disadvantaged families (Furenes et al., 2021). Secondly, app effects are seen primarily for narrow skills such as phonological awareness and counting (Kim et al., 2021) rather than broad domains such as reading comprehension or problem-solving. Thirdly, while the apps demonstrating positive research outcomes tend to be based on principles of child learning, most apps actually accessible to children are of limited educational value (Meyer et al., 2021). As a result, many of the digital activities children use in daily life are unlikely to promote broad skills such as oral language, particularly for children from low-income families. While moderate levels of media use are not detrimental, high levels are associated with poorer early language growth (Dore et al., 2020; Dynia et al., 2021), potentially because screen-time displaces more language-enhancing interactions such as shared reading and adult–child conversation. These combined findings are concerning because disadvantaged preschool children have both the highest levels of media use and the greatest risk of language delay (Huskinson & Tench, 2020; Waldfogel & Washbrook, 2010). Since early

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[1] Software applications, referred to as 'apps' throughout.

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language skills strongly predict children's later literacy, wider academic and life outcomes (Duncan et al., 2007; Morgan et al., 2015; Roulstone et al., 2011) this may have serious long-term consequences.

Reducing children's media use is not a long-term solution in an increasingly digital world. A promising alternative is to encourage adults to join and support their children's media use, on the basis that young children learn more from digital activities when they engage with an adult (Dore et al., 2018; Korat & Shneor, 2019; Taylor et al., 2024; Walter-Laager et al., 2017). A wide range of professional bodies highlight the importance of parent-supported media use for young children (e.g. American Academy of Pediatrics, 2016; Australian Government & Department of Health and Aged Care, 2021). However, almost a third (29%) of preschool children in England typically use apps alone (Huskinson & Tench, 2020) and few apps actively support social interactions between parents and children (Meyer et al., 2021). Research also shows that parents use fewer high-quality interaction strategies when sharing digital media with their children than when sharing traditional media such as books or games (e.g. fewer new words, fewer questions) (Ewin et al., 2021a, 2021b). This suggests that parents may need help in understanding *how* to support their children through technology.

Despite the recognised benefits of adult-supported media use, we know little about which digital features might facilitate adult–child social interactions, since most prior research focuses on children's solo use (Griffith et al., 2020). Recent studies indicate positive effects for e-book features such as conversation prompts (Troseth et al., 2020) or an embedded dictionary (Korat & Shneor, 2019). However, other features show null or negative effects (e.g. Kucirkova et al., 2021; Munzer et al., 2019a), making it hard to draw conclusions to inform digital design.

An important next step is to synthesise current evidence on potential interaction-supporting features to offer guidance for parents, media developers and policy-makers, and to inform a longer-term research agenda. The current study aimed to address this gap.

1.1. *Adult-child interactions with digital media*

Different types of parental media interaction or 'mediation' have been identified. *Restrictive or supervisory interactions* (e.g. monitoring or limiting media time) are aimed at protecting children from potential harms (Ewin et al., 2021b; Nikken & Schols, 2015). During *co-use*, adults share the digital experience with children but do not offer active guidance or discussion (Dore & Zimmermann, 2020; Scott, 2022; Nikken & Schols, 2015). *Active mediation* describes positive interactions aimed at fostering beneficial child outcomes or mitigating harms. These might include **1)** ***cognitive-instructive*** interactions supporting children's engagement with or understanding of content (e.g. asking question, discussing potential risks); **2)** ***affective interactions*** providing emotional support or affection (e.g. encouraging the child); or **3)** ***technical interactions*** aimed at supporting the child to use the device or media features (e.g. menus) or to overcome difficulties (Ewin et al., 2021b; Nikken & Schols, 2015; Scott, 2022). Our interest is in *co-use* and *active mediation* behaviours. Together, we consider these to represent 'joint media engagement' (Stevens & Penuel, 2010; Takeuchi & Stevens, 2011). This occurs "when any two or more people—parent and child, siblings, or peers—are looking at the same media at the same time, are involved in the content together, and are prompted by what they are seeing to interact with each other and bring more meaning to what they are watching or doing" (Guernsey & Levine, 2015, p. 171).

1.2. *The benefits of joint media engagement*

There is good evidence that young children learn more from media when joined by an adult. Korat & Shneor (2019) studied preschool children reading e-books and varied the presence of an embedded dictionary and a supporting parent. The greatest vocabulary growth was seen for children who read the dictionary version with their mother, followed by solo reading with a dictionary, then joint reading with no dictionary. Children learned least when reading alone with no dictionary. Thus, children learned more through co-than solo use, and the positive impact of the dictionary was magnified by parental support. Mothers also benefitted from the dictionary feature: those who read the enhanced e-book used higher-quality vocabulary-supporting strategies than those who read the standard e-book.

Similarly, there is evidence that preschool children engaging with a parent (compared to solo use) learn more from an early coding skills app (Griffith et al., 2022) and a word learning app (Walter-Laager et al., 2017), experience greater physiological arousal, positive emotions and story comprehension when reading e-books (Avelar et al., 2022; Dore et al., 2018), and learn to play new songs more efficiently from the video game Rock Band 3 (Perone et al., 2021).

These studies – and ours – are grounded in sociocultural theory, which understands interactions with more skilled partners to be a key driver of children's development and learning (Rogoff, 2003; Vygotsky, 1978). Strouse and colleagues (2013) propose three mechanisms through which adults might enhance children's learning through digital media. First, they can direct attention to digital content and highlight information to be learned. Second, they can scaffold children's cognitive processing and encoding of information (e.g. provide opportunities to practice retrieving information from memory). Thirdly, adults can provide social feedback, for example, helping children to interpret content.

1.3. *Features of digital media which influence adult-child social interactions*

Bindman et al. (2021) draw on theory, observational studies and evidence from the broader field of child learning to propose 16 interaction-supporting media design principles. We can group these broadly into attributes which **1)** provide *favourable conditions for social interaction* (e.g. interactivity, being child-interest driven), **2)** provide *direct support for social interaction* (e.g. promoting joint attention, collaboration or dialogue), or **3)** act as *potential distractors or inhibitors* (e.g. distracting pop-ups). We consider evidence from experimental research in each of these categories.

In relation to features theorised to provide *favourable conditions for social interaction*, studies have examined touchscreen interactivity (Antrilli & Wang, 2022; Okumura & Kobayashi, 2021), interactive audio-visual or haptic/tactile features (Cingel & Piper, 2017; Korat & Or, 2010; Korat & Shneor, 2019) and personalised e-book content (Kucirkova et al., 2021). Finding have been highly mixed.

In relation to *direct support for social interaction,* conversation prompts (e.g. story-relevant questions) embedded into e-books have shown promise in enhancing the quantity and quality of parent-child talk (Strouse et al., 2023; Troseth et al., 2020) and the positivity of parent–child interaction (Stuckelman et al., 2022). However, parent interaction 'nudges' embedded into a creative co-play app produced null results (Stuckelman et al., 2023).

In relation to *potential distractors or inhibitors,* Strouse et al. (2023) examined whether auto-narration in e-books inhibits parent–child conversation, despite being theoretically supportive for pre-literate children and non-native language speaking parents. Several studies have also examined whether interactive audio-visual features (e.g. 'hotspot' buttons, embedded games) could be distracting for adult–child interactions (Hrobon, 2014; Munzer et al., 2019a, 2019b). Thus, interactive audio-visual and haptic features have been theorised to have both positive *and* negative effects. Closer examination reveals that the expected direction of effects generally depends on the degree to which features align with (and therefore support) media content. For example, Korat and Schneor's (2019) e-book dictionary was closely content-relevant, in that it defined difficult words from the story using audio and visuals. In contrast, Hrobon (2014) tested potentially distracting audio-visual e-book features, such as embedded games with little relevance to story content, with the anticipation of negative effects. Between these extremes sits a hazier picture, with similar features theorised as being both positive and negative in influence (e.g. Korat & Or, 2010; Munzer et al., 2019a, 2019b) and no clear pattern in findings.

The mixed findings and complexity of potential effects mean that a research synthesis is needed to determine whether and how media design features influence adult-child interactions; and whether any behavioural impacts translate – as might be theorised – into impacts on wider outcomes such as child learning or parent–child relationships. Our review aimed to meet this need and – to our knowledge – is the first to do so. This is important for three reasons. First, findings can guide the design of digital media to better support adult-child interaction so that screentime *contributes* to children's 'language nutrition' (Zauche et al., 2016) rather than *displacing* language-enhancing interactions. Second, findings will expand research knowledge of how to shape adult–child interactions with digital media in an increasingly digital world. Finally, our study will expand methodological understanding of how to synthesise findings across emerging areas of research, in which studies differ in many ways from each other.

1.4. Current study – and potential moderators of effects

This meta-analysis aimed to synthesise evidence from experimental studies assessing the impact of digital design features on adult-child interactions with digital media such as apps, e-books and digital games. We focused on children aged 2–7 years, and on adult-child interactions likely to foster beneficial child outcomes (i.e. co-use and active mediation). We were interested particularly in interactions likely to support children's oral language development, such as the number of conversational 'turns' (e.g. Romeo et al., 2018); or parents' use of questions, cognitively challenging language or diverse and sophisticated vocabulary (e.g. Leech et al., 2018; Rowe, 2012; Rowe et al., 2017). Since our goal was to guide *future* media design, we included studies of media not yet available to the general public, for example apps developed by researchers.

Due to the diversity in design features developed and tested by researchers, and in methodologies used by studies across the field, we anticipated high between-study heterogeneity. Heterogeneity is commonly considered problematic for meta-analysis and can reduce statistical power (Henmi & Copas, 2010; Schmidt & Hunter, 2015). However, it can be advantageous when a review aims to understand differential effects (Higgins et al., 2019; Pustejovsky and Tipton, 2022). We sought to understand, not only whether adult-child interactions can be shaped through digital design, but also whether certain types of interaction (e.g. cognitive-instructive, technical) are more mouldable; and whether effects vary by sample or intervention characteristics. For example, we know that adult-child interactions (including media interactions) are influenced by factors such as the age of the child (e.g. Archer et al., 2021), the socio-economic status of the parent (Rowe, 2012) and family culture (Yen et al., 2018); so it is feasible that effects may differ for different populations. Adult-child social interactions are also strongly shaped by activity context (e.g. Holme et al., 2022; Kim & Anderson, 2008); and media activities may differ in their natural affordances for child learning. For example, Jing et al. (2023) found that children learn more from e-books than from television and apps. To achieve maximal impact via digital design we therefore need to understand whether effects on adult-child interaction depend on the media device or digital activity used; whether certain design features are more successful in shaping interactions than others; and whether commercial media can be as successful as those developed by researchers. On this basis, we followed Cochrane guidance in deciding to combine these different subpopulations and interventions in the same synthesis, and actively examine sources of heterogeneity via moderator and subgroup analyses (Higgins et al., 2019). This approach also enabled us to examine the impact of differences in methodological approaches, to understand how best to conduct research in this diverse field. For example, a key question when testing the impact of design features is whether researchers use the same activity (e.g. e-book) for both experimental and control conditions, manipulating only the presence of the design feature being tested; or whether they use activities which are different but matched on characteristics other than the feature being tested (Mathers et al., 2024).

We posed three research questions:

1. What are the effects of digital design features on social interactions between parents and children aged 2–7 years when using digital media together?

   a. Are larger effects seen for some types of interaction than others *(e.g. co-use, cognitive-instructive, technical, affective relational; language quality, language quantity)*?
2. What are the effects of these same design features on wider child and parent outcomes (e.g. *child learning, parent–child affect/ relationships*)?
   a. Are larger effects seen for some types of outcome than others?
3. How do effects vary by:
   a. Sample characteristics;
   b. Digital media and intervention characteristics; and
   c. Study design and publication characteristics?

2. Method

We submitted a protocol to the Open Science Framework platform. We followed the guidelines of the Cochrane Collaboration (Higgins et al., 2019) and PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) guidelines (Page et al., 2021).

2.1. Search and screening

We employed a broad search strategy to ensure identification of all relevant work in this emerging field and reduce the impact of publication bias (Marks-Anglin & Chen, 2020). We conducted multiple database searches; examined the reference sections and conducted forward citation searches of relevant articles; contacted key authors to identify further studies; and examined publication

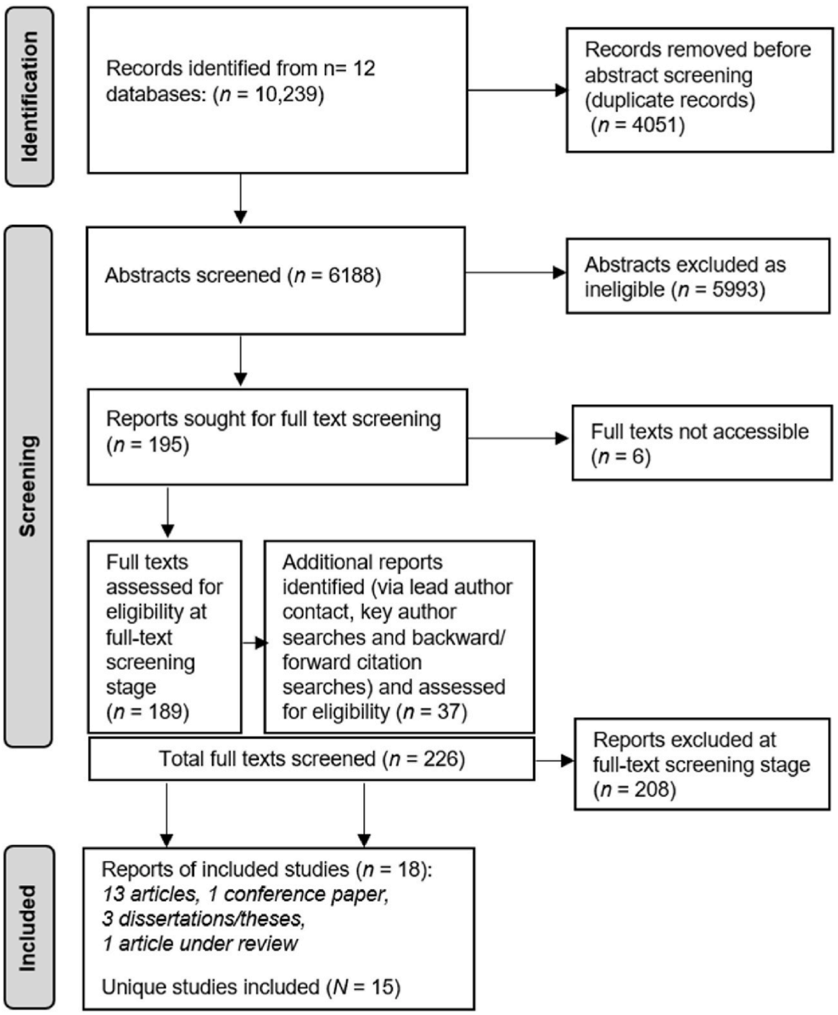

Fig. 1. Prisma flow diagram for the systematic literature search.

records of key authors. We included publications written in any language, although abstracts needed to be in English. The search process is described below and shown in Fig. 1.

An initial scoping search identified several relevant empirical papers and reviews which aided the development of our search terms [Supplementary Materials (SupMats) S.1]. The resulting search syntax sought articles with at least one keyword reflecting: **1)** *a digital context* (e.g. touchscreen, e-book), **2)** *an adult partner* (e.g. parent, adult), **3)** *a child partner* (e.g. child, toddler) and **4)** *interaction between partners* (e.g. joint, interact*, mediat*). The full syntax is shown in Appendix A. Search terms were deliberately broad to reflect the varied language used to describe joint media engagement across the field.

The database search was completed in September 2021 and a top-up search completed in December 2022, covering the intervening period. We used the full syntax in 11 databases reflecting research from the psychology, education, medical and digital fields: Scopus (n = 2501); Web of Science Core Collection (n = 2148); Proquest *(Linguistics Collection, Education Collection)* (n = 2330)*,* Proquest *Australian Education Index* (n = 270)*,* Proquest *Dissertations/Theses Global* (n = 623)*;* EBSCO *(British Education Index, Education Abstracts)* (n = 562)*;* PsycInfo via Ovid (n = 1098); PubMed (n = 47); ACM Digital Library (n = 168). A reduced syntax was used in the IEEE Explore Digital Library database (n = 492) (SupMats S.2). Searches were restricted to publications dating from January 2000, i.e.,

Table 1
Summary of eligibility criteria (see SupMats S.3 for further detail on rationale).

Aspect	Inclusion	Rationale and notes on exclusions
Publication type	Journal articles, conferences papers, book chapters, theses, dissertations, reports (e.g. research, government)	We included non–peer-reviewed literature to ensure all relevant studies were identified and to reduce impact of publication bias (Marks-Anglin & Chen, 2020).
Adult-child interaction focus	Studies of social interactions between adult-child pairs using media together, with the digital device content as a common focal point	Our definition of media-related interactions drew on Ewin et al.'s (2021b) systematic review on joint media engagement. We excluded interactions between 3 or more individuals (e.g. family groups) on the basis that group interactions are different in nature to dyadic (pair) interactions. We also excluded use of media to encourage adult-child interaction in non-digital contexts (e.g. parenting apps) or interactions which were not based around the device content (e.g. Zoom).
Child participants	Children aged 2–7 years inclusive	Most children are verbal and able to interact with technology by age 2. Media effects for this age group are stronger when engaging with an adult (e.g. Taylor et al., 2024)
	Typically developing children	We excluded studies targeting children with non-typical development because children with disabilities or developmental disorders that affect language learning (e.g. specific language difficulties, autism, cochlear implants) have their own distinct learning needs.
Adult participant	All (e.g. parent, teacher, other family member, researcher)	We defined 'adult' as meaning individuals over the age of 18 years.
Study context	All (e.g. home, formal or informal educational settings, laboratory)	
Digital activity	Studies of one or more specified digital media which could feasibly support learning in children aged 2–7 when supported by an adult	We included all activity types (e.g. apps, e-books, digital games) but, recognising that adult-child interaction may differ across digital activities (e.g. e-books vs games), we conducted sub-group analysis to examine the effects of activity type. We excluded studies of general media use (e.g. viewing television, online browsing etc). Digital activities did not need to be designed specifically for children but did need to be developmentally appropriate for children aged 2–7 years.
Digital device	All (e.g. touchscreen tablets, smartphones, computers, games consoles, e-readers)	Recent relevant reviews have focused on mobile touchscreen devices (e.g. Ewin et al., 2021b; Herodotou, 2018; Xie et al., 2018) or e-books (Furenes et al., 2021). We considered that valuable learning relating to adult-child interactions with media could be gleaned from studies of all devices. However, recognising that adult-child interaction may differ across devices, we conducted sub-group analysis to examine the effects of device type.
Study design	Experimental studies which manipulate one or more digital features anticipated to influence adult-child joint media engagement	We limited our focus to experimental studies because random allocation of participants into groups minimises allocation bias and controls for differences between participants (Sullivan, 2011). Inclusion of quasi-experimental or descriptive studies could introduce potential design bias (Coolican, 2014).
Interaction-supporting feature	At least one condition includes an in-app enhancement anticipated to influence adult-child joint media engagement (positively or negatively)	We excluded studies in which the interaction-supporting feature (e.g. parent guidance) was provided externally to the digital media (e.g. Rowe et al., 2021; Zippert et al., 2019) on the basis that these would require external intervention to achieve impact. Our interest is in design features which can easily be made accessible to parents when they download an app, e-book or game. We also excluded studies providing general interaction guidance for parents rather than in relation to specific digital media (e.g. apps, games, e-books).
Outcomes	Studies which assess the impact of the media enhancement on adult- child interaction	Although interested particularly in interactions which might support children's development and learning, we included studies assessing verbal or non-verbal adult-child interactions of any nature/type. We excluded studies which only assessed the impact of enhancements on wider child outcomes (e.g. child learning).

two years before the earliest study of digital media effects we could find. Full search parameters are shown in SupMats S.2. We believe, based on ongoing database alerts, that no additional studies have been published since our search which would meet the eligibility criteria.

The database search yielded 10,239 records. Using EPPI-Reviewer (Thomas et al., 2010), we removed 4051 duplicate records, leaving 6188 records for screening. The large number can be attributed to the deliberate breadth of our search. We first screened titles and abstracts, retaining only those (n = 195) which met all inclusion criteria (Table 1, SupMats S.3). Five reviewers conducted the screening. Three rounds of reliability assessment were conducted to ensure consistency, including 22% of abstracts in all ($N = 1359$). Disagreements were resolved by discussion. Since agreement between reviewer pairs was high by Round 3 (92–98%), remaining abstracts ($n = 4829$) were divided between reviewers.

Following full-text screening (n = 189) we conducted backwards and forwards citation searches on eligible reports identified to date, and on a further 18 excluded-but-relevant reports (SupMats S.4). We contacted key authors to identify further studies (including unpublished work) and examined publication records of key lead authors (SupMats S.4). This identified a further 37 reports which were screened in the same manner.

In all, 18 eligible reports were identified (Table 2a, marked * in references). We identified two cases of multiple publications reporting data from the same sample, with different outcomes presented in each. Data were extracted from each report separately but, since the unit of analysis in a systematic review is the study not the report (Higgins et al., 2019), we included each 'report cluster' as a single study. This resulted in 15 unique studies *(N)* presented in 18 reports *(n)* (Table 2a). Where multiple reports presented the *same* data, a primary manuscript (generally the most recent and/or detailed peer-reviewed publication) was selected but additional reports were used to supplement coding. Supplementary reports are marked *SR in the reference section and detailed in SupMats S.5.

Five studies included multiple intervention groups. Our approach to these was guided by our research questions. For two studies, two comparisons were included separately because these assessed the impact of different design features. For three studies, we selected one of two intervention groups assessing the same design feature and excluded the other (Appendix A, Table A2). In all, effect sizes were calculated for 17 comparisons *(C)* within the 15 studies *(N)* (Table 2a).

2.2. Calculation of effect sizes

Effect sizes were calculated for 330 unique study outcomes extracted from the 17 comparisons. We applied Hedges' *g* formula (Appendix B), addressing standard deviation differences between experimental and control groups (Borenstein et al., 2009; Lipsey & Wilson, 2001). Hedges' g provides a slightly better estimate than Cohen's d for smaller sample sizes (Higgins et al., 2019). As per Cohen's (1988) guidelines, 0.20 reflects a small effect, 0.50 a moderate effect, and 0.80 a large effect. Six effect sizes were excluded because they were not relevant outcomes or because it was unclear what they represented. A further 106 were excluded to account for interdependence between related effect sizes derived from the same study (SupMats S.6). Finally, 8 outcomes which had been combined in the relevant study were split into two with the same mean effect size (SupMats S.6). This resulted in 226 outcomes (effect sizes) for coding.

2.3. Coding

Two raters extracted data from the included reports and supplementary publications. As well as describing studies, we aimed to support analysis of potential moderators, informed by prior literature. The extraction categories, coding framework and rationale are shown Table 2a–c and SupMats S.7.

First, sample characteristics (e.g. *child age*, *region*) were extracted (Table 2a). Second, characteristics of the digital media and intervention were extracted (Table 2b), including details of the design features tested. We categorised these in two ways. The '*design feature category*' grouped features into those theorised to: **1)** provide *favourable conditions for social interaction;* **2)** *directly* support *social interaction*; **3)** act as potential *distractors;* and **4)** act as potential *inhibitors*. Since studies in categories 3 and 4 were theorised to have a negative impact on interaction, we reversed the study conditions to test for the *absence* of distractors or inhibitors. This ensured the theorised direction of effects was positive for all studies. The '*design feature type'* code enabled examination of specific design features. Where several studies assessed the same feature (e.g. conversation prompts) we grouped these to support sub-group analysis. We also coded for the *developer* of the media or design feature (e.g. commercial, researchers) and for the *intervention exposure* (time period, number of intervention sessions).

Third, study design and publication factors were extracted (Table 2c), largely to capture markers of methodological quality. The '*activity match'* code captured whether the same digital activity (e.g. e-book) was used for both experimental and control conditions, manipulating only the presence of the design feature being studied, or whether different activities were used – and, if the latter, whether researchers attempted to match these on criteria other than the design feature being manipulated.

Finally, the 226 outcome effect sizes (*k*) extracted were coded using a framework based on Ewin et al. (2021b) (SupMats S.7). They were first coded by participant (parent, child, dyad), then divided into *adult–child interactions (k=206)* and *wider parent-child outcomes (k=20)*. *Adult–child interactions* included interactions assessed during media use and those assessed using a pre–post design. They were further coded into one of three 'active mediation' interaction types (*cognitive-instructive, technical*, *affective-relational)* (Ewin et al., 2021b) or as *co-use* (adults sharing a digital experience with children but not offering active mediation) (Dore & Zimmermann, 2020; Nikken & Schols, 2015; Scott, 2022). Undesirable interaction outcomes such negative affect (e.g. frowning) were reverse coded so the direction of effects was always positive. Several interaction outcomes *(k=36)* were excluded as being 'neutral', that is, neither desirable nor undesirable (SupMats S.7), resulting in a final total of 170 *adult-child interaction* effect sizes. To support specific analysis

Table 2a
Characteristics of the included studies (18 reports, 15 unique studies, 17 comparisons): **sample characteristics**.

Report (n)	Study (N)	Comparison (C)	Authors	Year [a]	Dyads [c]	SES	Region [a]	Child age range [a]	Child age: Mean (range)
1	1	1	Antrilli & Wang	2022	40	High	North America	3–5 years	5y10 (5y2-6y8)
2	2	2	Boteanu et al.	2016	86	Unknown	North America	3–5 years	5y8 (4-8y)
3	3	3	Cingel & Piper	2017	18	Unknown	North America	3–5 years	I = 4y11 C = 5y2 (4-7y)
4	4	4	Hrobon COMPARISON 1	2014	29	Unknown	North America	3–5 years	5y0 (4y3-5y11)
		5	Hrobon COMPARISON 2	"	"	"	"	"	"
5	5	6	Korat & Or	2010	24	High	Other (Middle East)	3–5 years	5y9 (5y6-6y5)
6	6	7	Korat & Schneor	2019	64	Low	Other (Middle East)	3–5 years	5y7 (5y7-6y9)
7	7	8	Kucirkova et al.	2021	26	High	Europe	3–5 years	No mean (3-4y)
8	8	9	Lurie	2021	78	High	North America	3–5 years	3y11 (3y0-4y11)
9			Stuckelman	2019	42	"	"	"	"
10			Stuckelman, Strouse & Troseth	2022	75	"	"	"	3y11 (3-5y)
11	9	10	Munzer et al.	2019a	37	Unknown	North America	Under 3	2y5 (2-3y)
12			Munzer et al.	2019b	"	"	"	"	"
13	10	11	Munzer et al.	2021	72	Unknown	North America	Under 3	2y6 (2-3y)
14	11	12	Okomura & Kobayashi	2021	36	Unknown	Other (Asia)	Under 3	2y2 (2y0-2y2)
15	12	13	Strouse, Troseth & Stuckelman COMPARISON 1 [d]	2023 [d]	50	High	North America	3–5 years	I = 3y10 C = 3y10 (2y9-4y10)
		14	Strouse, Troseth & Stuckelman COMPARISON 2 [d]	"	"	"	"	"	I = 3y10 C = 4y1 (3y1-5y1)
16	13	15	Stuckelman [e]	2022 [e]	77	Unknown	North America	3–5 years	4y5 (3y9-4y11)
17	14	16	Troseth et al.	2020	32	Low	North America	3–5 years	4y0 (3y0-5y1)
18	15	17	Yang et al.	2022	107	Unknown	Other (Asia)	3–5 years [b]	No mean (3-7y)

[a] Used for moderator analysis.
[b] Inferred from report.
[c] The number of dyads reflects the experimental conditions included in this meta-analysis. In some cases, the full study sample was larger.
[d] In press or under review at the time of searching but published in a peer-reviewed journal prior to coding and analysis.
[e] Doctoral thesis. A journal article based on this thesis was published in 2023. An author-supplied preprint of this article was used to supplement data coding. However, since it had not been accepted for publication at the time coding/analysis was completed, the 2022 thesis was entered as the primary manuscript and coded as non–peer-reviewed.

of interactions likely to promote children's oral language development, *adult–child language interactions* (*k=130*) were extracted from the '*cognitive instructive'* category. These were sub-divided into two further categories (*language quantity*, *language quality)* on the basis that the quality of parent language input (e.g. parents' use of diverse and sophisticated vocabulary) has a greater impact on children's language skills than the quantity (Rowe et al., 2017).

Wider outcome effects (k = 20) were defined as non-behavioural parent, child or dyadic outcomes assessed at post-test. They were further coded into *affective-relational outcomes* (e.g. positive parent–child relationships) and *cognitive outcomes* (e.g. children's language, literacy or mathematical skills).

To ensure precision in coding, both raters independently coded a randomly drawn sample of 9 included reports (50% of reports, 63.7% of effect sizes). Disagreements were resolved by discussion. Since intercoder reliability was high (89% for report-level codes, range: 56–100%; 96% for outcome-level codes, range: 90–100%), one rater coded the remaining publications. The two codes with unsatisfactory reliability were intervention session frequency and exposure period (both 56%). Findings for these analyses should be interpreted with caution.

2.4. Analysis

Since we explicitly aimed to examine heterogeneity, we included all relevant effect sizes from included studies (Pustejovsky & Tipton, 2022). This introduced the likelihood of interdependence between related effect sizes drawn from the same study. Common

Table 2b
Characteristics of the included studies: **digital media and intervention characteristics**.

Report (n)	Study (N)	Authors	Media	Digital activity [b]	Specific design feature AV = audio-visual	Design feature category [b]	Design feature type [b] AVH = audio, visual or haptic (touch)	Developer [a]	Exposure period [a] (sessions/ reads [a])
1	1	Antrilli and Wang (2022)	Touch-screen	Other app	Touchscreen interactivity *(rotation movement in tangram game mirrors physical tangram manipulation)*	Favourable conditions [d]	Other content-relevant feature	Commercial	1 session (1)
2	2	Boteanu et al. (2016)	Touch-screen	*E-book* or rhyme app	Conversation prompts (e.g. questions) embedded in e-book	Direct support	Content-relevant conversation support	Researcher	1 session (1)
3	3	Cingel and Piper (2017)	Touch-screen	*E-book* or rhyme app	Variety of interactive haptic (touch) features *(most but not all content-relevant)*	Favourable conditions [d]	Content-relevant interactive AVH features	Researcher enhanced	1 session (1)
4	4	Hrobon (2014) COMP 1	Touch-screen	*E-book* or rhyme app	Variety of content-relevant interactive audio-visual features e.g. hotspots	Favourable conditions	Content-relevant interactive AVH features	Commercial	>1 session <1 week (3)
		Hrobon (2014) COMP 2	"	"	Variety of distracting interactive audiovisual features e.g. hotspots, games	Distractor (absence of)	Distracting interactive AVH features (absence)	"	"
5	5	Korat and Or (2010)	Un-known	*E-book* or rhyme app	Variety of content-relevant interactive audio-visual features e.g. hotspots	Favourable conditions	Content-relevant interactive AVH features	Researcher	1 session (1)
6	6	Korat and Shneor (2019)	Un-known	*E-book* or rhyme app	Dictionary feature embedded in e-book	Favourable conditions	Content-relevant interactive AVH features	Researcher	1–2 weeks (4)
7	7	Kucirkova et al. (2021)	Touch-screen	*E-book* or rhyme app	Personalised e-book content	Favourable conditions	Other content-relevant feature	Researcher	1–2 weeks (3)
8	8	Lurie (2021)	Touch-screen	*E-book* or rhyme app	Conversation prompts (e.g. questions) embedded in e-book	Direct support	Content-relevant conversation support	Researcher enhanced	1–2 weeks (10)
9		Stuckelman (2019)	"	"	"	"	"	"	"
10		Stuckelman et al. (2022)	"	"	"	"	"	"	"
11	9	Munzer et al. (2019a)	Touch-screen	*E-book* or rhyme app	Variety of distracting interactive audio-visual features e.g. hotspots	Distractor (absence of)[d]	Distracting interactive AVH features (absence)	Commercial	1 session (1)
12		Munzer et al. (2019b)	"	"	"	"	"	"	"
13	10	Munzer et al. (2021)	Touch-screen	E-book or *rhyme app*	Full-text auto-narration *(automatic play-sing-along in rhyme app)*	Inhibitor (absence of)[d]	Auto-narration (absence)	Commercial	1 session (1)
14	11	Okumura and Kobayashi (2021)	Touch-screen	Other app	Touchscreen interactivity *(shapes in a shape game change in response to touch)*	Favourable conditions [d]	Other content-relevant feature	Commercial	1 session (1)
15	12	Strouse et al. (2023) COMP 1	Touch-screen	*E-book* or rhyme app	Conversation prompts (e.g. questions) embedded in e-book	Direct support	Content-relevant conversation support	Researcher enhanced	1 session (2)
		Strouse et al. (2023) COMP 2	"	"	Full text auto-narration	Inhibitor (absence of)	Auto-narration (absence)	"	"
16	13	Stuckelman (2022)	Touch-screen	Other app	Parent interaction 'nudges' *(interaction ideas embedded in creative co-play game)*	Direct support	Other content-relevant feature	Researcher enhanced	1–2 weeks (10)
17	14	Troseth et al. (2020)	Touch-screen	*E-book* or rhyme app	Conversation prompts (e.g. questions) embedded in e-book	Direct support	Content-relevant conversation support	Researcher enhanced	1 session (2 reads) [c]

(*continued on next page*)

Table 2b (*continued*)

Report (n)	Study (N)	Authors	Media	Digital activity [b]	Specific design feature AV = audio-visual	Design feature category [b]	Design feature type [b] AVH = audio, visual or haptic (touch)	Developer [a]	Exposure period [a] (sessions/ reads [a])
18	15	Yang et al. (2022)	Touch-screen	*E-book* or rhyme app	Bilingual conversation prompts (e.g. questions) embedded in e-book	Direct support	Content-relevant conversation support	Researcher	>1 session <1 week (2)

[a] Used for moderator analysis.
[b] Used for sub-group analysis.
[c] Two reads: effect sizes denoting a JME measure within an individual read coded as 1 session, effect sizes for outcome assessed post-reading coded as 2 sessions.
[d] Inferred from report.

Table 2c
Characteristics of the included studies: **study design or publication factors**.

Report (n)	Study (N)	Authors	Publication year [a]	Random allocation of conditions [a]	Publication peer reviewed [a]	Research context [a]	Activity match [a]	Methodological quality
1	1	Antrilli & Wang	2022	Yes	Yes	Other	Same	Moderate
2	2	Boteanu et al.	2016	Yes	Yes	Lab	Same	Low
3	3	Cingel & Piper	2017	No/unspecified	No	Lab	Same	Moderate
4	4	Hrobon COMPARISONS 1 + 2	2014	Yes	No	Lab	Different but matched	Moderate (2 comparisons)
5	5	Korat & Or	2010	Yes	Yes	Home	Different but matched	High
6	6	Korat & Schneor	2019	Yes	Yes	Home	Same	High
7	7	Kucirkova et al.	2021	No/unspecified	Yes	Lab	Same	Moderate
8	8	Lurie	2021	Yes	No	Home	Same	High
9		Stuckelman	2019	"	No	"	"	"
10		Stuckelman, Strouse & Troseth	2022	"	Yes	"	"	"
11	9	Munzer et al.	2019a	Yes	Yes	Lab	Different but matched	Moderate
12		Munzer et al.	2019b	"	Yes	"	"	"
13	10	Munzer et al.	2021	Yes	Yes	Lab	Different but matched	Moderate
14	11	Okomura & Kobayashi	2021	No/unspecified	Yes	Lab	Same	Moderate
15	12	Strouse, Troseth & Stuckelman [b] COMPARISONS 1 + 2	2023 [b]	Yes	Yes	Other	Same	High (2 comparisons)
16	13	Stuckelman [c]	2022 [c]	Yes	No	Home	Same	High
17	14	Troseth et al.	2020	Yes	Yes	Other	Same	High
18	15	Yang et al.	2022	Yes	Yes	Home	Same	Moderate

[a] Used for moderator analysis.
[b] In press or under review at the time of searching but published in a peer-reviewed journal prior to coding and analysis.
[c] Doctoral thesis. A journal article based on this thesis was published in 2023. An author-supplied preprint of this article was used to supplement data coding. However, since it had not been accepted for publication at the time coding and analysis was completed, the 2022 thesis was entered as the primary manuscript and coded as non–peer-reviewed.

methods of addressing interdependence, such as effect size averaging, can lead to information loss and bias in meta-analytic findings (e.g. Cheung & Chan, 2004). Instead, we employed robust variance estimation (RVE) with correlational weights to generate multilevel meta-analysis models (Hedges et al., 2010; Tanner-Smith et al., 2016; Pustejovsky & Tipton, 2022). RVE is recommended when sample sizes are small, as they were in our review (Zejnullahi & Hedges, 2024). It also allowed us to group interdependent effect sizes within study clusters, accounting for likely interdependence by assuming a within-study correlation among all pairs of effect sizes (Pustejovsky & Tipton, 2022). This included dependence arising from common features of a research group or lab (hierarchical effects) and arising from measurement, such as multiple measures of a common outcome construct (correlated effects) (Pustejovsky & Tipton, 2022). RVE methods enable dependent effect sizes to be included in a single meta-regression model, even when the exact form of the dependence is unknown (Pustejovsky & Tipton, 2022). As recommended by Harrer et al. (2021), we assumed a correlation coefficient of 0.6 (moderate-to-strong) to accommodate varying correlation scores among observed outcomes. We also reran the main group analyses assuming weak (0.3), moderate (0.5), and strong (0.8) correlation coefficients to test the robustness of findings (Akoglu, 2018). Three RVE meta-analyses were conducted at the study comparison level (C = 17): 1) *overall adult–child interactions*; 2) *adult–child language interactions*; and 3) *wider outcomes (affective-relational & cognitive)*.

To assess heterogeneity, we used the I^2 value, which represents observed variance among studies on a scale of 0–100%, with benchmarks for moderate, substantial and high heterogeneity set at 30%, 50% and 75% (Higgins et al., 2019). Continuous-level moderators were centred by subtracting the mean value of the variable. Moderating variables are marked [a] in Tables 2a–c. Several extracted variables (including socio-economic status) could not be used, primarily due to a lack of detail in included reports (SupMats S.7). To minimise the risk of Type II error we generated separate mixed-effects models for each moderator variable (Raudenbush & Bryk, 2002). We also conducted several sub-group analyses (marked [b] in Tables 2a–c; see also Tables 3–5).

2.5. Assessing methodological quality

We assessed the methodological rigour of included studies using a tool designed for a meta-analysis on the effectiveness of electronic storybooks (Savva et al., 2022). It encompassed eight weighted criteria: 3 points indicated factors of high importance, 2 moderate importance and 1 some importance. If a criterion was fully met, the full score (3, 2 or 1) was awarded. If any aspect of the criterion was missing a score of 0 was awarded. It was considered a fatal flaw to have any 'high importance' factors missing. We added a ninth criterion of our own and adapted Savva et al.'s cut-offs as follows: a sum score of 17–22 and no fatal flaws indicated High Quality, 10–16 indicated Moderate Quality, and 0–9 indicated Low Quality (Appendix C). Two raters independently assessed all studies. Inter-rater reliability (Cohen et al., 2017) was moderate ($\kappa = 0.57$). Disagreements were resolved through discussion. Of the 17 included comparisons, seven were graded as being of high methodological quality, nine as moderate and one as low (Table 2c, SupMats S.8). On this basis, we included all 17 comparisons in our primary analyses. However, we reran all three RVE meta-analyses including only the seven high-quality comparisons to assess potential bias arising from our inclusive approach.

2.6. Accounting for outliers and possible publication bias

We used contour-enhanced funnel plots (Appendix E) to identify outlying studies at either extreme of the distribution i.e. effect sizes which were significantly high or low with high standard error scores (see Harrer et al., 2021; Peters et al., 2008). Contour-enhanced plots allow us to distinguish possible publication bias from other forms of asymmetry. We reran the analyses excluding identified outliers to establish their influence on our findings. Several methods were also used to assess publication bias, including visual examination of the funnel plots, Egger's weighted regression test and the "trim and fill" method (further detail in

Table 3
Results of the three-level meta-analysis models: overall effects on adult-child interactions and wider outcomes (RQs 1 & 2).

	C (of 17 comparisons)	*k* (effect sizes)	*g*	*SE*	95% CI	Test Statistic (df), *t*	*p*	I^{2}[a]
EFFECTS ON OVERALL ADULT-CHILD INTERACTIONS								
Main effect	16	170	0.56	0.26	0.01, 1.10	(16.4), 2.15	0.05*	93.2
Participant								
Child	9	50	0.20	0.22	−0.32, 0.71	(7.78), 0.88	0.40	89.87
Parent	13	101	0.74	0.32	0.05, 1.42	(13.5), 1.41	0.04*	94.53
Dyad	9	19	0.66	0.30	−0.03, 1.35	(2.2), 7.93	0.06#	81.28
Interaction type								
Co-use	2	3	1.27	0.66	−7.05, 9.58	(1), 1.94	0.30	62.83
Affective-relational	5	19	−0.17	0.30	−1.03, 0.69	(3.62), −0.58	0.60	91.09
Cognitive-instructive	16	142	0.72	0.29	0.12, 1.33	(16.3), 2.53	0.02*	92.87
Technical	3	6	−0.18	0.38	−2.28, 1.93	(1.6), −0.46	0.70	94.93
EFFECTS ON ADULT-CHILD LANGUAGE INTERACTIONS – subset of overall adult-child interactions								
Main effect	15	130	0.78	0.31	0.11, 1.44	(14.6), 2.5	0.03*	93.20
Participant								
Child	9	34	0.40	0.31	−0.31, 1.11	(7.86), 1.3	0.23	90.31
Parent	12	86	1.1	0.38	0.27, 1.94	(11.8), 2.87	0.01**	93.96
Dyad	8	10	0.55	0.35	−0.28, 1.38	(6.96), 1.56	0.16	82.66
Interaction type								
Language quality	15	99	0.66	0.34	−0.07, 1.40	(13.4), 1.94	0.07#	93.97
Language quantity	9	31	1.01	0.25	0.44, 1.59	(7.71), 4.07	0.004**	88.68
EFFECTS ON WIDER OUTCOMES (AFFECTIVE-RELATIONAL & COGNITIVE) - sub-group analyses only reported where *k* ≥ 2								
Main effect	8	20	0.30	0.25	−0.29, 0.88	(6.58), 1.21	0.27	82.10
Participant								
Child	8	16	0.37	0.24	−0.22, 0.95	(6.52), 1.51	0.18	81.69
Parent	2	3	0.18	0.09	−0.96, 1.32	(1), 1.99	0.30	0.00
Outcome type								
Affective-relational	2	5	0.08	0.00	0.02, 0.13	(1), 17.9	0.04*	0.00
Cognitive	7	15	0.33	0.29	−0.39, 1.05	(5.63), 1.13	0.30	85.89

# $p < .01$, *$p < .05$, **$p < .01$, ***$p < .001$.
[a] I^2 = the observed variance among studies on a scale of 0–100%, with benchmarks for low, moderate and high heterogeneity set at 25%, 50% and 75% respectively (Higgins et al., 2003).

Table 4
Results of the three-level meta-analysis models: subgroup analyses assessing digital media and intervention factors (RQ3b).

	C (of 17 comparisons)	*k* (effect sizes)	*g*	*SE*	95% CI	Test Statistic (df), *t*	*p*	$I^{2(a)}$
EFFECTS ON OVERALL ADULT-CHILD INTERACTIONS								
Digital activity								
E-book/rhyme app	13	160	0.64	0.30	−0.01, 1.28	(13.5), 2.12	0.05*	93.51
Other app	3	10	0.10	0.10	−0.39, 0.59	(1.7), 1.03	0.43	66.15
Design feature category								
Favourable conditions for social interaction	7	44	0.52	0.66	−1.1, 2.13	(5.95), 0.79	0.46	95.38
Direct support for social interaction	5	78	0.74	0.15	0.37, 1.11	(5.4), 4.99	0.003**	77.56
Absence of distractors	2	15	−0.00	0.15	−1.90, 1.90	(1), −0.01	0.99	96.88
Absence of potential inhibitor	2	33	0.12	0.21	−2.54, 2.79	(1), 0.58	0.67	94.11
Design feature type (AVH = audio-visual-haptic)								
Content-relevant interactive AVH features	4	33	0.84	1.15	−2.82, 4.51	(2.98), 0.74	0.52	95.93
Content-relevant conversation support	4	74	0.84	0.12	0.52, 1.16	(3.9), 7.35	0.002**	75.75
Absence of distracting interactive AVH features	2	15	−0.00	0.15	−1.9, 1.9	(1), −0.01	0.99	96.88
Absence of auto-narration	2	33	0.12	(0.21	−2.54, 2.79	(1), 0.58	0.67	94.11
Other	4	15	0.02	0.10	−0.33, 0.36	(2.63), 0.15	0.89	57.88
EFFECTS ON ADULT-CHILD LANGUAGE INTERACTIONS – subset of overall adult-child interactions								
Digital activity								
E-book/rhyme app	13	125	0.85	0.35	0.10, 1.59	(12.7), 2.45	0.03*	93.33
Other app	2	5	0.22	0.07	−0.68, 1.13	(1), 3.14	0.20	83.17
Design feature category								
Favourable conditions for social interaction	6	35	0.54	0.78	−1.47, 2.56	(4.97), 0.69	0.52	96.18
Direct support for social interaction	5	59	0.91	0.16	0.48, 1.34	(4.44), 5.68	0.003**	64.65
Absence of distractors	2	5	1.77	0.94	−10.2, 13.7	(1), 1.87	0.31	90.23
Absence of potential inhibitor	2	31	0.22	0.33	−3.92, 4.35	(1), 0.66	0.63	94.01
Design feature type (AVH = audio-visual-haptic)								
Content-relevant interactive AVH features	4	26	0.77	1.21	−3.08, 4.63	(2.98), 0.64	0.57	96.52
Content-relevant conversation support	4	58	0.99	1.15	0.56, 1.42	(3.66), 6.57	0.004**	62.06
Absence of distracting interactive AVH features	2	5	1.77	0.94	−10.2, 13.7	(1), 1.87	0.31	90.23
Absence of auto-narration	2	31	0.22	0.33	−3.92, 4.35	(1), 0.66	0.63	94.01
Other	3	10	0.04	0.18	−1.02, 1.09	(1.49), 0.21	0.86	71.83
EFFECTS ON WIDER OUTCOMES (AFFECTIVE-RELATIONAL & COGNITIVE) - sub-group analyses only reported where *k* ≥ 2								
Digital activity								
E-book/rhyme app	7	18	0.40	0.25	−0.24, 1.03	(5.58), 1.56	0.17	79.81
Other app	1	2	–	–	–	–	–	–
Design feature category								
Favourable conditions for social interaction	5	12	0.42	0.36	−0.61, 1.44	(3.86), 1.15	0.32	85.37
Direct support for social interaction	2	7	0.29	0.23	−2.67, 3.24	(1), 1.24	0.43	70.10
Design feature type (AVH = audio-visual-haptic)								
Content-relevant interactive AVH features	3	9	0.66	0.50	−1.58, 2.91	(1.93), 1.31	0.32	79.18
Content-relevant conversation support	2	7	0.29	0.23	−2.67, 3.24	(1), 1.24	0.43	70.11
Other	2	3	−0.09	0.40	−5.17, 5.0	(1), −0.21	0.87	82.75

$^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

[a] I^2 = the observed variance among studies on a scale of 0–100%, with benchmarks for low, moderate and high heterogeneity set at 25%, 50% and 75% respectively (Higgins et al., 2003).

Appendix D).

3. Results

3.1. Study characteristics

The search yielded 17 comparisons (*C*) across 15 studies (*N*), presented in 18 reports (*n*) (Table 2a).

Sample characteristics (Table 2a): of the 15 studies, most (*N* = 10) were conducted in North America. The majority (*N* = 12) targeted children aged 3–5 years, with three targeting under 3s and none targeting children aged 6 or older. Sample sizes were generally small, varying from 18 to 107 dyads (*M* = 51.67, *SD* = 25.50). Only seven studies reported data on socioeconomic status (SES): five included primarily high-SES families and two primarily low-SES families.

Digital media and intervention characteristics (Table 2b): all studies which specified the type of mobile device (N = 13) used touchscreens. Most involved e-books (*N* = 11) or rhyming apps (*N* = 1) with three testing other apps. None examined digital games (e.g. video games). Of the 17 study comparisons, 7 assessed features theorised to provide *favourable conditions for social interaction*. Five of these assessed e-book/rhyme app features, including interactive audio-visual features (*C* = 2), haptic (touch) feedback (*C* = 1), an embedded dictionary (*C* = 1) and personalised content (*C* = 1); while two game-based app studies assessed the effects of content-relevant touchscreen interactivity. Six comparisons examined features offering *direct support for social interaction*, including conversation prompts embedded into e-books (C = 5) and parent interaction 'nudges' embedded in a creative co-play game (C = 1). Two comparisons assessed the effects of removing distracting audio-visual hotspots from an e-book (i.e. an *absence of potential distractors*); and two tested for the possible *inhibiting* effects of auto-narration.

Five of the 15 studies used commercially developed media, five used researcher-developed media and five used commercial media enhanced by researchers. Treatment duration varied from a single exposure (*N* = 7) to 10 exposures across 2 weeks (N = 2). The mean number of sessions was 2.87 (SD = 3.27).

Study publication and design factors (Table 2c): of the 18 reports, 13 were peer-reviewed. The rest (n = 5) were conference papers, theses and or articles under review. Of the 15 studies, almost half (*N* = 7) were conducted in laboratory settings, with others conducted in the home (*N* = 5) or mixed settings (*N* = 3). Most studies (N = 12) randomly allocated participants to conditions and used the same activity for both intervention and control conditions (N = 11). Of those using different activities (e.g. different e-books), all attempted to match these on criteria other than the design feature being manipulated.

Study outcomes (Table 3): of the 17 comparisons tested across the 15 studies, all except one (Yang et al., 2022)[2] reported adult–child interaction effects (*C* = 16, *k* = 170), most commonly cognitive-instructive interactions (*C* = 16, *k* = 142). All except two (Antrilli & Wang, 2022; Yang et al., 2022) reported adult–child language interaction effects (*C* = 15, *k* = 130). Only eight tested wider adult-child outcomes at post-test (*k* = 20). These were largely cognitive (*C* = 7, *k* = 15): primarily child language outcomes (*k* = 14), with one mathematical outcome.

3.2. RQ1: effects on adult-child interactions (Table 3)

The first meta-analysis investigated effects on overall adult-child interactions (*C* = 16, *k* = 170), revealing a medium positive effect (g = 0.56; 95% CI [0.01, 1.10]; p = .05). Effects were only significant for *parent interactions* (g = 0.74; [0.05, 1.42]; p = .04) with no significant effects for *dyadic* or *child interactions*. In relation to interaction type, a medium effect was identified for *cognitive-instructive interactions* (g = 0.72; [0.12, 1.33]; p = .02) but no effects for *co-use, technical or affective-relational interactions*.

The second meta-analysis investigated effects on adult-child language interactions (*C* = 15, *k* = 130), revealing a medium significant effect (g = 0.78; [0.11, 1.44]; p = .03). A large significant effect was seen for *parent language interactions* (g = 1.1; [0.27, 1.94]; p = .01) but no effects for *dyadic* or *child interactions*. A large significant effect was seen for *language quantity* (g = 1.01; [0.44, 1.59]; p = .004), with a medium effect for *language quality* (g = 0.66; [−0.07, 1.40]; p = .07) close to the threshold for statistical significance.

3.3. RQ2: effects on wider outcomes (Table 3)

The third meta-analysis examined effects on *wider outcomes* (e.g. cognitive, affective). No overall effect was identified. In relation to outcome type, a significant effect was identified for *affective-relational outcomes* (g = 0.08; [0.02, 0.13]; p = .04) but not for *cognitive outcomes*.

3.4. RQ3: moderator and sub-group analyses (Tables 4 and 5)

As expected, there was considerable heterogeneity throughout, with high I^2 values ≥ 75% for 35 of the 47 analyses (74%). We

[2] Yang et al. (2022) only made formal comparisons between intervention and control conditions for child outcomes but was included in the meta-analysis because they also examined adult–child interactions for the intervention group.

Table 5
Moderator analysis (mixed effects models, sub-group analyses): sample factors (RQ3a), digital media and intervention factors (RQ3b), study factors (RQ3c).

	Effects on overall adult-child interactions							Effects on wider (affective-relation & cognitive) outcomes						
	Test Statistic (df), F, p	I^{2}[a]	*C* (of 17)	Dyads	*g*	*SE*	95% CI	Test Statistic (df), F, p	I^{2}[a]	*C* (of 17)	Dyads	*g*	*SE*	95% CI
SAMPLE FACTORS														
Child age (RQ3a)	(2, 167) 43.95, <0.0001	95.0												
Under 3			3	25	0.18	0.28	−0.38, 0.74							
3–5 years incl.			13	145	0.80***	0.12	0.56, 1.03							
Region	(3, 166), 63.33, <0.0001	94.6						(3, 17), 69.15, <0.0001	73.6					
Europe			1	5	−0.16	0.61	−1.36, 1.03			1	1	0.47	0.58	−0.66, 1.59
America			12	135	0.51***	0.12	0.28, 0.75			5	11	−0.08	0.20	−0.46, 0.30
Other			3	30	1.72***	0.26	1.22, 2.22			2	8	0.96***	0.20	0.58, 1.35
DIGITAL MEDIA														
Developer (RQ3b)	(3, 166) 56.87, <0.0001	94.7						(3, 17), 24.91, <0.0001	73.9					
Commercial			6	31	0.10	0.25	−0.40, 0.60			4	3	−0.29	0.33	−0.93, 0.35
Researcher			4	37	1.40***	0.23	0.94, 1.86			9	3	0.9***	0.19	0.55, 1.27
Researcher enhanced			6	102	0.63***	0.14	0.36, 0.90			7	2	0.04	0.25	−0.44, 0.52
Session frequency	(1, 168) 3.91, 0.05	95.0	16	170	0.08*	0.04	0.00, 0.15	(1, 19), 16.78, <0.0001	65.8	8	20	0.45***	0.11	0.23, 0.66
Exposure period	(3, 166) 69.25, <0.0001	94.5						(3, 17), 36.07, <0.0001	65.2					
Single session			10	128	0.44***	0.12	0.21, 0.68			3	9	−0.07	0.19	−0.44, 0.31
>1 session, ≤1 week			2	4	−0.57	0.82	−2.19, 1.06			3	6	0.36[a]	0.21	−0.05, 0.77
1–2 weeks			4	38	1.66***	0.22	1.22, 2.10			2	5	1.29***	0.22	0.85, 1.74
STUDY AND PUBLICATION FACTORS														
Peer reviewed (RQ3c)	(2, 167) 39.64, <0.0001	95.1						(2, 18), 8.83, 0.01	83.1					
Yes			12	23	0.73***	0.12	0.50, 0.97			5	14	0.57**	0.19	0.19, 0.94
No			4	147	0.49	0.31	−0.12, 1.10			3	6	−0.00	0.33	−0.66, 0.65
Publication year	(1, 168) 1.07, <0.30	95.1	16	170	0.03***	0.03	−0.03, 0.10	(1, 19), 0.01, 0.94	83.8	8	20	−0.01	0.07	−0.14, 0.13
Random allocation	(2, 167) 42.39, <0.0001	95.0						(2, 18), 6.75, 0.03	84.1					
Yes			13	156	0.76***	0.12	0.53, 0.99			6	15	0.50**	0.19	0.12, 0.88
No			3	14	0.09	0.39	−0.67, 0.85			2	5	0.17	0.36	−0.54, 0.88
Research context	(3, 166) 61.75, <0.0001	94.6						(3, 17), 23.89, <0.0001	74.3					
Home			4	46	1.39***	0.21	0.99, 1.79			2	8	0.96***	0.20	0.57, 1.35
Lab			8	45	0.13	0.21	−0.28, 0.54			4	7	0.09	0.26	−0.41, 0.60
Other			4	79	0.63***	0.16	0.32, 0.93			2	5	−0.15	0.27	−0.68, 0.39
Activity match	(2, 167) 51.82, <0.0001	94.7						(2, 18), 7.33, 0.03	84.5					
Same			11	132	0.89***	0.12	0.65, 1.13			6	18	0.48**	0.18	0.13, 0.82
Different matched			5	38	0.04	0.23	−0.42, 0.49			2	2	−0.18	0.61	−1.37, 1.01

[#] p < .01, *p < .05 , **p < .01 , ***p < .001.
[a] I^2 = the observed variance among studies on a scale of 0–100%, with benchmarks for low, moderate and high heterogeneity set at 25%, 50% and 75% respectively (Higgins et al., 2003).

examined sources of heterogeneity using sub-group and moderator analyses.

RQ3a sample characteristics: *child age* was a significant moderator of effects on adult-child interactions (Table 5), with a significant effect only seen for studies of children aged 3–5 years (g = 0.80; 95% CI [0.56, 1.03]; p < .001). *Geographic region* was a significant moderator of effects on adult-child interactions and on wider outcomes (Table 5).

RQ3b digital media and intervention characteristics: in relation to type of digital activity (Table 4), only studies of *e-books/rhyme apps* had a significant effect on overall adult-child interactions ($g = 0.64$; [−0.01, 1.28]; $p = .05$) and adult-child language interactions ($g = 0.85$; [0.10, 1.59]; $p = .03$), with no effects for *other apps*. In relation to design feature category and type, only *direct support for social interaction* had a significant effect on overall adult-child interactions (g = 0.74; [0.37, 1.11]; p = .003) and adult-child language interactions (g = 0.91; [0.48, 1.34]; p = .003). Specifically, *content-relevant conversation*-support had a large effect on overall adult-child interactions (g = 0.84; [0.52, 1.16]; p = .002) and adult-child language interactions (g = 0.99; [0.56, 1.42]; p = .004). These were all studies of conversation prompts embedded into e-books.

In relation to the intervention itself, developer type was a significant moderator of effects on adult-child interactions and wider outcomes (Table 5). Significant positive effects were found only for *researcher developed media* (interactions: $g = 1.40$; [0.94, 1.86]; $p <$ .001; wider outcomes: $g = 0.9$; [0.55, 1.27]; $p < .001$) and *researcher-enhanced media* (interactions: g = 0.63; [0.36, 0.90]; p < .001), with no effects for *commercially designed media*. Both session frequency and exposure period contributed to heterogeneity (Table 5), although findings from the sub-group analyses were mixed and difficult to interpret.

RQ3c study design and publication characteristics: publication type was a significant moderator of effects on adult-child interactions and wider outcomes (Table 5), with only *peer-reviewed articles* reporting significant effects (interactions: $g = 0.73$; [0.50, 0.97]; $p < .001$; wider outcomes: $g = 0.57$; [0.19, 0.94]; $p < .01$). Publication year did not influence the analysis outcome.

In relation to study design, use of random allocation, research context and activity matching were all significant moderators of effects on adult-child interactions and wider outcomes (Table 5). Significant effects were identified for studies employing *random allocation* (interactions: $g = 0.76$; [0.53, 0.99]; $p < .001$; wider outcomes: $g = 0.50$; [0.12, 0.88]; $p < .01$), *data collection in the home* (interactions: $g = 1.39$; [0.99, 1.79]; $p < .001$; wider outcomes: $g = 0.96$; [0.57, 0.1.35]; $p = .01$) and *studies using the same activity for both intervention and control conditions* (interactions: $g = 0.89$; [0.65, 1.13]; $p < .001$; wider outcomes: $g = 0.48$; [0.13, 0.82]; $p < .01$).

3.5. Sensitivity analyses and publication bias

Rerunning the main group analyses assuming weak (0.3), moderate (0.5), and strong (0.8) within-study correlations among all pairs of effect sizes, rather than a correlation coefficient of 0.6 as assumed for the main analysis, did not influence the findings.

Retaining only the seven *high-quality comparisons* (Table 2c) resulted in a larger effect size for overall adult–child interactions ($g =$ 0.95; 95% CI [−0.002, 1.9]; $p = .05$ **vs** $g = 0.56$) and for adult–child language interactions ($g = 1.11$; [−0.02, 2.21]; $p = .047$ **vs** $g =$ 0.78) (Appendix E Table E1). The effect size was also larger for wider outcomes (g = 0.76 vs g = 0.30) but remained non-significant.

In terms of outliers, the funnel plots identified several outliers (Appendix E). For overall adult-child interactions there was some evidence of asymmetry for studies with smaller standard errors (Figure E.1). The presence of more non-significant studies than expected suggests the asymmetry stemmed from reasons other than publication bias (e.g. study methodology). Additional tests of publication bias supported this conclusion. The Egger's test was non-significant ($t = 0.81$, df = 14, $p = .43$) and the trim and fill analysis identified no hypothetical unpublished studies. Removing outliers identified using funnel plots (n = 4 comparisons) resulted in a slightly smaller effect size for overall adult-child interactions but one which remained statistically significant ($g = 0.45$; [0.15, 0.76]; p $= .008$ **vs** $g = 0.56$). Findings were similar for studies examining adult-child language interactions: the trim and fill analysis identified no additional hypothetical unpublished studies, indicating no publication bias. Removing outliers identified using funnel plots (n = 7 comparisons) made no difference to the effect size ($g = 0.78$; [0.37, 1.19]; $p = .003$ **vs** $g = 0.78$).

For wider outcomes, the funnel plot demonstrated moderate asymmetry (Figure E.2). Studies with larger standard errors tended to have smaller effects than the estimated true effect and to be non-significant, indicating reasons other than publication bias underlying the asymmetry. Studies with smaller standard errors tended to have *larger* effects than expected and were more likely to be statistically significant, indicating possible publication bias (i.e. the equivalent non-significant studies were missing). The trim and fill analysis identified three additional hypothetical unpublished studies although, curiously, the analysis including these additional studies demonstrated a statistically significant effect which was *larger* than the original ($g = 0.63$, [0.06, 1.19]; $t = 2.46$; $p = .03$). There were insufficient studies for Egger's test. Removing outliers (n = 2 comparisons) resulted in a smaller effect size which remained non-significant ($g = 0.23$; [−0.22, 0.68]; $p = .20$). Taken together, these analyses suggest findings on wider outcomes may be biased and should be interpreted cautiously.

Results for all analyses were identical when using the L0, R0, and Duval and Tweedie estimators.

4. Discussion

This meta-analysis synthesised evidence from experimental studies assessing the impact of digital design features on interactions between adults and children aged 2–7 years when using media together ($n = 627$ parent–child pairs; $k = 190$ effects). Findings suggest that adult–child social interactions can be influenced by manipulating the design of digital media, both overall ($g = 0.56$) and specifically in relation to language interactions ($g = 0.78$). These effects held after correcting for potential bias arising from outlying studies, although the effect for overall adult-child interactions was slightly smaller (g = 0.45). Excluding studies graded as being of low or moderate quality resulted in larger effect sizes (overall interactions g = 0.95; language interactions g = 1.11) suggesting that our inclusion of lower-quality studies potentially underestimated, rather than inflating, identified effects.

4.1. Are larger effects seen for some types of interaction than others? (RQ1a)

Effects for parent behaviours were two-to-three times the size of those for child behaviours, consistent with the idea that parent behaviour change precedes – and is instrumental in – child behaviour change and that adults mediate children's learning through media use.

The largest effects were seen for cognitive-instructive interactions aimed at supporting children's understanding of, and engagement with, digital content (Ewin et al., 2021b). Specifically, the large effect on parent language use ($g = 1.1$) suggests promise for digital design in supporting parents to use language-enhancing interactions when using media with their preschool children. This is an important finding because preschool language skills underpin children's later literacy, wider academic and life outcomes (Duncan et al., 2007; Morgan et al., 2015; Roulstone et al., 2011); because children's interactions with adults form the greatest influence on those early language skills; and because parents tend 'naturally' to use fewer high-quality language-supporting strategies when sharing digital media with their children than when sharing traditional media such as books or games (Ewin et al., 2021a, 2021b). If adult-child interactions with media can be encouraged and enhanced via intelligent design then – rather than screentime *displacing* language-enhancing interactions – it could instead *contribute* to children's 'language nutrition' (Zauche et al., 2016). Our findings suggest that developers should be encouraged and supported to build interaction-supporting features into the media they design for young children.

This may matter most for disadvantaged preschool children, who have both the highest levels of media use and the greatest risk of language delay (Huskinson & Tench, 2020; Waldfogel & Washbrook, 2010). We were unfortunately unable to examine socioeconomic status (SES) as a moderator of effects because few studies reported SES data. At least two studies (Korat & Shneor, 2019; Troseth et al., 2020) identified significant positive impacts on parent language input in low-SES populations, indicating promise for digital design as a means of early language intervention. However, more work is needed to confirm whether design-based interventions can successfully shape adult-child interaction in low-SES families.

Cautiously promising is the medium-sized effect for language interaction quality ($g = 0.66$). This matters because the *quality* of parent language input (e.g. the number of conversational turns; use of diverse and sophisticated vocabulary) has a greater impact on children's language skills than the *quantity* of input (Romeo et al., 2018; Rowe et al., 2017). The effect fell just below the threshold for statistical significance ($p = .066$). However, given the known limitations of p-values as a benchmark in research (e.g. Greenland et al., 2016) and the fact that our ability to detect effects was limited by the small sample sizes of included studies, it is worthy of further investigation. Future work should focus design and research efforts on improving interaction quality rather than quantity, building on studies showing success in this regard (e.g. Korat & Shneor, 2019; Troseth et al., 2020).

Studies to date have focused largely on shaping adult-child language interactions ($k = 142$ of 170 interaction effects). Whilst these are important — for the reasons identified above — it is worth considering what is *not* being studied. For example, only three study comparisons ($k = 6$) examined technical interactions aimed at helping children to use the device or its features (e.g. menus) and no significant effects were identified. As they grow up, children will need to develop the technical confidence and skills they need for the digital world ahead, and paediatric guidelines recommend that parents support and encourage children's digital media literacy (e.g. Ponti, 2023). Future research and design might usefully focus on how best to support parents to do this during everyday interactions.

4.2. What are the effects of these same design features on wider child and parent outcomes? (RQ2)

The main analyses offered little evidence that positive effects for adult-child interaction translated into benefits for wider outcomes. The one significant effect, for affective-relational outcomes, was negligible and based on 5 effect sizes from 2 study comparisons. All five related to parent-child enjoyment which, while important, merely lays the ground for more substantial outcomes such as child learning.

Several of the design and methodology-focused sub-group analyses *did* identify medium-to-large significant effects on wider outcomes (e.g. for studies employing random allocation, studies conducted in the home and researcher-developed media); and some of the individual studies displaying these features found significant impacts for child language learning (e.g. Korat & Shneor, 2019; Yang et al., 2022). This suggests that robustly designed studies *may* be able to achieve effects on wider outcomes but that improvements in intervention and study design are needed to maximise and detect potential impacts. The mixed methodological quality and small sample sizes of included studies, alongside evidence of possible publication bias for wider outcomes, make it difficult to draw firm conclusions from currently available evidence.

4.3. Do effects differ by population characteristics? (RQ3a)

Studies to date have been more successful in shaping joint media engagement with children aged 3–5 than interactions with children under 3 years. However, given the small number of studies targeting under 3s, it would be premature to draw firm conclusions. More research is needed to study adult-child joint media use during children's first 3 years and the extent to which it can be shaped. Whilst we may not wish to encourage *more* screen time for babies and toddlers, given the social nature of learning during these formative years, it is vital that any digital time is adult-supported and as nurturing as it can be.

4.4. Which digital media types and design features showed the greatest promise? (RQ3b)

To date, research has focused largely on enhancing the design of e-books accessed via mobile touchscreen devices, with significant adult-child interaction effects identified *only* for these studies. No effects were found for design features embedded into other apps. This may be due to specific features of the 3 studies examining other app types. Alternatively, adult-child interactions may be easier to enhance in the e-book context, either because design features can more easily be tied to the clear structure and defined content; or because parents are primed to interact in the context of books, while co-use with other apps is less familiar. However, given that preschool children spend twice as much time on average playing games than reading books on mobile devices (Rideout & Robb, 2020), it will be important for future research to establish whether adult-child interactions can be enhanced outside the e-book context. Notably, no studies were identified which examined adult-child co-play with digital games (e.g. video games). This is an important area for future research since digital games could provide a highly engaging context for adult-child interactions, and at least one study (Perone et al., 2021) has shown benefits arising from video game co-play. It will also be important to examine adult-child co-play on devices other than mobile touchscreens, since these may limit shared interaction due to their size (Hiniker et al., 2018).

We found that researcher-developed and researcher-enhanced media offer a more promising context for interaction-enhancing design than commercial media, likely because they are more aligned with principles of child development and learning (Bindman et al., 2021). No significant effects were found for studies without researcher involvement in design. This indicates that industry-research partnerships may be important to achieve optimal interaction-enhancing design.

In terms of the design features themselves, effects were only found for features which *directly* targeted adult-child interactions. This suggests that developers should design with social interaction in mind, since providing 'favourable conditions' in the hope that positive interaction will happen naturally is likely to be insufficient. Some of the interventions which did not generate effects (e.g. Kucirkova's personalised e-books) may be more successful if further enhanced to more directly support joint media engagement.

The strongest evidence was found for conversation prompts embedded into e-books, with a large positive effect on adult-child interaction ($g = 0.84$–0.99). Three studies explored the effects of the same enhanced e-book (Troseth et al., 2020; Strouse et al., 2023; Lurie, 2021; Stuckelman, 2019; Stuckelman et al., 2022). Parents using the enhanced e-book talked more, used higher quality language (e.g. a wider range of words, longer conversations, more cognitively challenging language) and displayed more positive interactions with children than those using a non-enhanced version. Since these features are known to support children's oral language skills (e.g. Rowe & Snow, 2020) this bodes well for potential child impact. Parents also showed improved interactions when reading new books without embedded prompts, suggesting they learned new strategies from the enhanced e-book and transferred this learning to new contexts. This has important implications for early intervention, indicating that embedding prompts into e-books could achieve wider impacts on adult-child reading interactions.

Two further conversation prompt studies (Boteanu et al., 2016; Yang et al., 2022) indicate that positive effects on adult-child interactions can be achieved using different e-books and prompt designs, and in different populations. However, since these studies did not examine transfer effects, more research is needed to examine whether the 'parent learning' finding holds for other e-book and app designs. Further work is also needed to establish whether conversation prompts — or other types of parent behaviour 'nudges' — can be effective when embedded into other types of media (e.g. apps, games), since the one study which attempted this did not find positive results (Stuckelman, 2022).

More effort is also needed to untangle findings on interactive audio, visual and haptic (AVH) features. No significant effects were identified for content-relevant interactive features, or for the *absence* of distracting features, despite effect sizes of reasonable magnitude. This is likely due to heterogeneity between studies, reflecting the fact that interactive AVH features tend to vary greatly in design and purpose. Our analysis attempted to distinguish between those which were content-relevant (i.e. supportive of content-related interactions) and those which were potentially distracting. However, even among features coded as content-relevant there was great variability (Table 2b). Some were audio, some visual, some haptic; and designs ranged from highly targeted word-level features (e.g. narrating, animating or defining words) to interactive games designed to promote story comprehension. These variations may generate very different effects. In addition, most of the AVH studies examined a package of interactive features (Table 2b), making it hard to disentangle individual effects. More nuanced research is needed.

Finally, this analysis indicates – albeit based on only two studies – that auto-narration does not inhibit adult-child joint media engagement. This has direct practical implications, since it suggests that e-book designers can include auto-narration without fear of inhibiting parent-child interaction.

4.5. *What can we learn about study design? (RQ3c)*

The moderator analyses provided valuable information to guide research design. The first conclusion is that study quality matters. Retaining only high-quality studies resulted in a larger significant effect size ($g = 0.95$ vs $g = 0.56$). Significant effects on adult-child media interactions were identified for studies employing random allocation of conditions ($g = 0.76$) and studies in which the same activity was used for both intervention and control conditions ($g = 0.89$), with no effects identified for studies without these features. The findings on activity matching suggest that no matter how carefully two different activities (e.g. e-books) are matched, creating a perfect match may be impossible. This is plausible, since variations in book format (e.g. narrative complexity) can influence adult–child interactions (Holme et al., 2022). Even small differences may create 'noise' in the data and reduce the likelihood of detecting effects. Finally, studies in the home showed greater effects ($g = 1.39$) than laboratory studies ($g = 0.13$), likely because they offer a more natural environment for media use. Allowing adult and children to use media at home — though more challenging to control — may get us closer to understanding the effects of media on social interactions.

4.6. Limitations

A primary limitation of our analysis was the small number of studies identified and the small sample sizes of those studies, reflecting the emerging nature of the field. This limited options for sub-group and moderator analyses and may also have limited our ability to detect effects. In addition, only seven of the 17 included comparisons (6 of 15 studies) were graded as being of high methodological quality. The fact that effects were larger when low-to- moderate quality studies were excluded suggests our inclusive approach may have under- (rather than over-) estimated the true effect of media design features on adult-child interactions. Future work should focus on conducting well powered and methodologically rigorous studies to provide robust evidence to guide media design. It will be particularly important to establish whether the benefits identified for adult-child interaction translate into benefits for wider child and adult outcomes.

A lack of information regarding SES in many of the studies meant we could not examine effects for low-SES families, limiting the value of our findings for informing early intervention. It is also notable that many studies were conducted in the laboratory or involved assessing adult-child interactions during a single exposure. Our findings show that adult-child interactions can be shaped under controlled conditions. However, more work is needed to provide evidence on implementation fidelity and impact in more authentic environments.

Finally, available evidence relates primarily to e-books used on touchscreen devices by children aged 3–5, and their effects on adult-child language interactions. Our analysis cannot tell us how to shape adult-child interaction with other apps, digital games or devices; with children under 3; or in relation to other domains of interaction − for example, parental support for children's digital skills and media literacy. This limits our ability to guide interaction-enhancing design, and thus to harness children's screen time to provide nourishing interactions in the most holistic sense. Further experimental research is needed in these key areas. However, to provide guidance for interaction-enhancing design we will need to extend beyond experimental studies. While robust, such studies are by nature narrow in scope and designed to test highly specific features. In developing a comprehensive set of interaction-supporting design principles – building on the founding work of Takeuchi and Stevens (2011) and Bindman et al. (2021) - the field will also need to draw on wider theory, observational and qualitative work, and on input from families themselves.

5. Conclusions

Children learn more from media when adults join and support their use; but evidence suggests that parents do not always use high quality interactions during joint media engagement. Our meta-analysis is the first to synthesise evidence on whether adult-child interactions can be enhanced through digital design. Findings indicate potential for design-based intervention, particularly in relation to enhancing adult language input. This is notable when one considers the cost-effectiveness and scalability of digital intervention. Compared to interventions based on physical media, support for positive parent–child social interaction can be embedded into digital media (e.g. e-books, apps) and delivered to families via download, with minimal need for external intervention such as workshops, training packs or other resources.

Our findings suggest that developers should be encouraged and supported to design with interaction in mind and, specifically, to consider building conversation prompts into e-books as standard practice. Conversation-enhanced e-books have potential for use by parents, early education providers and early interventionists to provide children with language-enriching interactions.

Much remains to be done, however, to generate comprehensive and robust evidence which can guide design, practice and policy. Future work should focus on 1) conducting well powered and methodologically rigorous studies; 2) examining potential benefits for child learning; and 3) establishing whether the positive effects identified to date can be achieved outside the e-book context, outside the laboratory, and for low-SES families. Progressing the field will require educationalists, digital researchers and developers to learn from each other. Digital researchers and developers can draw from education in ensuring that design and evaluation is tightly focused on principles of child development and learning. Likewise, educationalists could be more imaginative in moving beyond a focus on e-books to harness the potential offered by wider digital media – for example, by exploring the potential of augmented reality games (e.g. Sobel et al., 2017) as an engaging interaction context. Together, we can work to ensure that young children's screen time is maximally nourishing for their learning and development.

Author statement

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Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.edurev.2025.100665.

APPENDIX A. Supplementary information on the search process

Table A.1
Full search syntax

Digital context	(app OR apps OR "computer program*" OR "digital book*" OR "digital story*" OR "digital stories" OR "e book*" OR ebook* OR "electronic book*" OR "electronic story*" OR "electronic stories" OR "multimedia book*" OR "multi media book*" OR "multimedia stor*" OR "multi media stor*" OR "interactive stor*" OR "mobile game*" OR "digital game*" OR "electronic game*" OR "computer game*" OR "video game*" OR "mobile play" OR "digital play" OR "electronic play" OR "digital toy*" OR "augmented reality" OR "AR" OR "digital activit*" OR "mobile device*" OR "digital device*" OR "electronic device*" OR "interactive device*" OR "mobile technology" OR "mobile media" OR "mobile learn*" OR tablet* OR ipad* OR touchscreen* OR "touch screen*" OR smartphone* OR "smart phone*" OR "handheld device*" OR e-reader* OR "digital reader*")
	AND
Adult partner	(parent* OR caregiver* OR mother* OR father* OR paternal OR maternal OR grandparent* OR grandmother* OR grandfather* OR adult* OR teacher* OR educator*)
	AND
Child partner	(child* OR "pre school*" OR preschool* OR nursery OR kindergarten* OR preK* OR "pre K*" OR "pre kindergart*" OR primary OR elementary OR "early learn*" OR "early child*" OR "early years" OR toddler* OR "age* 2" OR "age* 3" OR "age* 4" OR "age* 5" OR "age* 6" OR "age* 7" OR "2 year* old*" OR "3 year* old*" OR "4 year* old*" OR "5 year* old*" OR "6 year* old*" OR "7 year* old*" OR "first grade*" OR "grade 1")
	AND
Interaction between partners	("co use" OR "co using" OR "co view*" OR joint OR interact* OR conversation* OR share* OR together OR mediat* OR support* OR "parent child" OR "mother child" OR "adult child" OR "teacher child" OR "educator child" OR "father child")
	NOT
Excluded terms	(undergraduate* OR "college student*" OR graduate* OR "higher education" OR "further education" OR "university student*" OR "post-graduate" OR postgraduate OR adolescent OR teen* OR "young adult*" OR "high school*" OR "secondary education" OR "secondary school*" OR "secondary student*" OR health* OR "safety literacy" OR drug* OR medic* OR disease* OR nutrit* OR hospital* OR patient* OR emergency OR pain OR dent* OR cancer OR nurs* OR pregnan* OR fitness OR energy OR CAD OR chemical OR diet* OR addiction OR consumer OR engineering OR finance* OR physics OR biological)

Table A.2
Details of experimental comparisons included and excluded

Study sample	Authors	Conditions	Comparisons included (and rationale)	Condition/s excluded (and rationale)
1	Antrilli & Wang	1. Rotate to manipulate tangrams 2. Tap to manipulate	1 vs 2 *(assesses nature of touchscreen manipulation)*	None
2	Boteanu	1. E-book with expert-created conversation prompts 2. E-book with machine-generated prompts 3. No prompts	1 vs 3 *(assesses expert-created conversation prompts)*	*We excluded Condition 2 (machine generated prompts) to reduce heterogeneity. All other studies of conversation prompts included in this review used prompts generated by researchers.*
3	Cingel & Piper	1. Interactive haptic (touch) features 2. No haptic feedback	1 vs 2 *(assesses haptic features)*	None
4	Hrobon	1. Book 1 with content-relevant interactive audio-visual (AV) features 2. Book 1, no AV features 3. Book 2 with distracting interactive AV features 4. Book 2, no AV features	1 vs 2 *(assesses content-relevant AVH features)* 4 vs 3 *(assesses absence of distracting AV features)* *Both comparisons were included because they assess different design features.*	None
5	Korat & Or	1. E-book with selected content-relevant interactive AV features	1 vs 2 *(assesses content-relevant AV features)*	None

(continued on next page)

Table A.2 (*continued*)

Study sample	Authors	Conditions	Comparisons included (and rationale)	Condition/s excluded (and rationale)
		2. Different e-book with many distracting interactive AV features		
6	Korat & Schneor	1. E-book with dictionary 2. E-book without dictionary	1 vs 2 *(assesses dictionary)*	None
7	Kucirkova et al.	1. Personalised e-book 2. Non-personalised e-book	1 vs 2 *(assesses personalisation)*	None
8	Lurie Stuckelman (2019) Stuckelman, Strouse & Troseth	1. E-book with conversation prompts 2. E-book, no prompts 3. Choice of whether to use prompt or no-prompt version	1 vs 2 *(assesses conversation prompts)*	*We excluded Condition 3 to reduce heterogeneity. In this condition, families had the choice of whether to use the e-book enhanced with conversation prompts or a version without prompts. Although this assesses real-world use of the intervention, it was very different to all other studies included in this review, which tested effects when parent-child dyads were provided with the enhanced version only.*
9	Munzer et al. (2019a) Munzer et al. (2019b)	1. E-book with distracting interactive AV features 2. Basic e-book	2 vs 1 *(assesses absence of distracting AV features)*	None
10	Munzer et al. (2021)	1. Rhyming app with auto-narration 2. App, no auto-narration	1 vs 2 *(assess auto-narration)*	None
11	Okomura & Kobayashi	1. Game with contingent touch *(shapes change on-screen in response to touch)* 2. Game with non-contingent touch *(shapes changes on-screen misaligned with user touch)* 3. No touch *(users watch as shapes change on-screen)*	1 vs 3 *(assesses contingent touchscreen interactivity)*	*Guided by our framework of categorisation for interaction-supporting design features, we excluded Condition 3 (non-contingent touch) on the basis that this is not an ecologically valid design feature. The shapes changed on-screen in a manner which was mis-aligned with user touch (i.e. randomly). Although useful within the study to assess the value of contigency, this condition did not provide useful information within scope of this review.*
12	Strouse, Troseth & Stuckelman	1. E-book with conversation prompts and auto- narration 2. Ebook with auto-narration but no prompts 3. Ebook, no narration no prompts	1 vs 2 *(assesses conversation prompts)* 2 vs 3 *(assesses auto-narration)* *Included both comparisons because these assess different design features. Note: these comparisons shared a common condition.*	*Although all conditions were included*, we did not separate out the comparison 1 vs 3 *on the basis that the two individual comparisons which were included are more informative. The excluded comparison combines two design features theorised to have different effects on adult-child interaction (prompts were expected to have a positive effect, while auto-narration may be a potential inhibitor).*
13	Stuckelman (2022)	1. App with parent nudges 2. App, no nudges	1 vs 2 *(assesses parent nudges)*	None
14	Troseth et al.	1. E-book with conversation prompts 2. E-book, no prompts	1 vs 2 *(assesses conversation prompts)*	None
15	Yang et al.	1. E-book with conversation prompts 2. E-book, no prompts	1 vs 2 *(assesses conversation prompts)*	None

APPENDIX B. Formula for calculating Hedges' *g*

The calculation for Hedges' g was as follows (Borenstein et al., 2009):

$$g = \frac{M_1 - M_2}{s_p}\left(1 - \frac{3}{4(df - 1) - 1}\right)$$

APPENDIX C. Criteria for assessing methodological quality

Table C.1
Criteria for assessing methodological rigour (Savva et al., 2022)

No.	Criterion	Maximum number of points
1	Random assignment	3 points
2	Control group intervention	3 points
3	Sufficient participant description	3 points
4	Treatment conditions explicitly described	2 points
5	Operationalised measures	3 points
6	Reliability of measures reported	2 points
7	Treatment fidelity ensured	3 points
8	Effect size reported	1 point
Additional criterion (our own)	Reliability of measures sufficient* (Cohen, Manion & Morrison, 2017)	2 points
	Maximum total	**22 points**

*Rule of thumb: Cohen's kappa (0.70 and above) or percentage agreement (80% and above) met.

APPENDIX D. Further detail on methods used to assess publication bias

First, we visually examined funnel plots. Asymmetry within the plots was assessed using Egger's weighted regression test, which checks for a correlation between effect sizes and their standard errors, with a significant correlation indicating potential bias (Egger et al., 1997; Torgerson, 2006). The test assesses whether the intercept of the regression model is significantly larger than zero, with a significant result indicating plot asymmetry – and thus possible publication bias. We also used the "trim and fill" method which adjusts for missing studies due to publication bias. This approach identifies and 'trims' outliers in the funnel plot, simulating the removal of potentially biased studies. It then 'fills' the plot by adding hypothetical missing studies to balance the asymmetry and estimates how the overall effect size changes with the inclusion of these 'missing' studies. Finally, we applied the L0 and R0 estimators developed by Duval and Tweedie (2000). The L0 estimator considers the scenario where missing studies are on the left (negative) side of the plot, and R0 estimator considers missing studies on the right (positive) side. By comparing these estimators, we investigated how the direction of bias might affect our results.

APPENDIX E. Findings – supplementary tables and figures

Table E.1Sensitivity analyses

			Effect Size			
	C (of 17 comparisons)	*k* *(effect sizes)*	*g (S.E.)*	95% CI	Test Statistic	I^2
EFFECTS ON OVERALL ADULT-CHILD INTERACTIONS						
Main effect	16	170	0.56 (0.26)	[0.01, 1.10]	t(16.4) = 2.15, p = .05*	93.20
High quality comparisons only	7	123	0.95 (0.41)	[-0.002, 1.9]	t(7.96) = 2.3, p = .05*	89.80
Outliers removed	12	146	0.45 (0.14)	[0.15, 0.76]	t(10.7) = 3.27, p = .008**	90.86
EFFECTS ON ADULT-CHILD LANGUAGE INTERACTIONS						
Main effect	15	130	0.78 (0.31)	[0.11, 1.44]	t(14.6) = 2.5, p = .03*	93.20
High quality comparisons only	7	99	1.11 (0.46)	[-0.02, 2.21]	t(6.92) = 2.41, p = .047*	90.22
Outliers removed	8	78	0.78 (0.17)	[0.37, 1.19]	t(6.45) = 4.6, p = .003**	91.06
EFFECTS ON WIDER OUTCOMES (AFFECTIVE-RELATIONAL & COGNITIVE)						
Main effect	8	20	0.30 (0.25)	[-0.29, 0.88]	t(6.58) = 1.21, p = .27	82.10
High quality comparisons only	2	7	0.76 (0.75)	[-8.71, 10.2]	t(1.0) = 1.02, p = .49	86.13
Outliers removed	6	14	0.23 (0.15)	[-0.22, 0.68]	t(3.17) = 1.6, p = .20	50.20

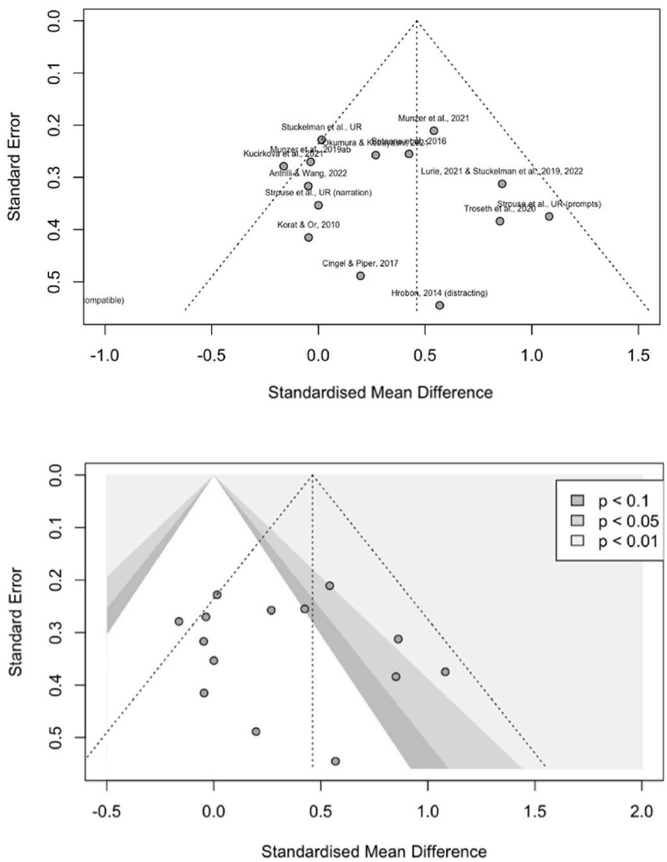

Fig. E.1. *Funnel plot (showing study labels) and contour-enhanced funnel plot for overall adult-child interaction effect sizes: standard error by standardised mean difference (Hedges' g)*

Note: In the contour-enhanced funnel plot, the circles represent included studies. The dotted line shows the idealised funnel shape we expect studies to follow, with the central line representing the estimated true effect. Studies in the light grey and mid grey areas are statistically significant ($p < 0.5$ and $p < .01$). Studies in the dark grey and white areas are non-significant. Examining the symmetry of the plot can identify potential bias. Studies in the top part of the plot (those with smaller standard errors), should lie closely together and not far away from the pooled effect size. In the lower part of the plot, with increasing standard errors from smaller studies, the funnel opens up and effect sizes are expected to scatter more heavily to the left and right of the pooled effect. Asymmetry (i.e. more studies which are larger or smaller than the expected true effect) indicates potential bias. If studies tend to have larger effects than expected, particularly where these are statistically significant, this indicates potential publication bias (i.e. there are 'missing' studies with non-significant effects which may not have been published). If studies tend to have smaller effects than expected (i.e. there are 'missing' studies with larger effects), this suggest that bias may stem from other factors e.g. methodological factors. For more information on contour-enhanced meta-analysis funnel plots and the methods followed, see Harrer et al. (2021) and Peters et al. (2008).

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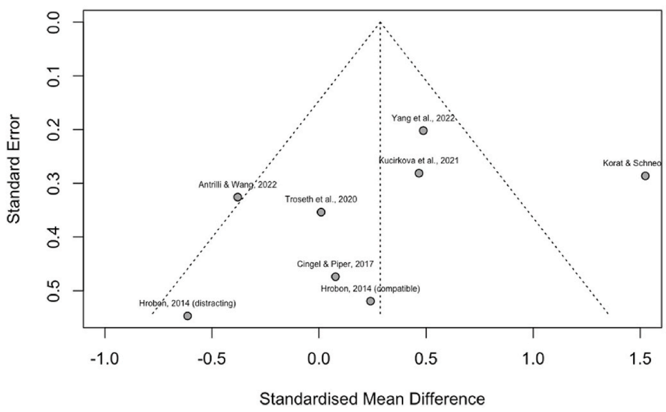

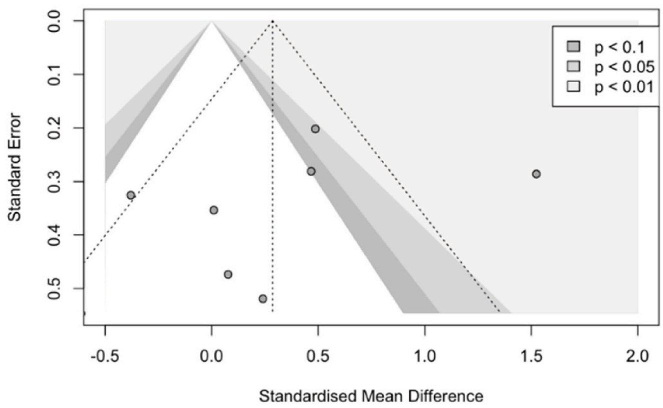

Fig. E.2. *Funnel plot (showing study labels) and contour-enhanced funnel plot for wider outcome effect sizes: standard error by standardised mean difference (Hedge's g)*

Note: see Figure E1 for additional explanation.

Data availability

Data will be made available on request.

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* = included report, ** = supplementary report

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