Psychosocial Interventions for Edge of Care Families in the Early Years: A Systematic Review and Meta-Analysis

Despite the recognised need for early interventions to prevent maltreatment and family separation in families involved with child protection services (CPS), evidence for children aged 0–5 years remains scarce and inconclusive. This systematic review and meta-analysis aimed to evaluate the effectiveness of psychosocial interventions delivered to these families, in reducing risk of harm, improving parenting quality, and supporting parental functioning. The review included families with children aged 0–5 years engaged with CPS and deemed at risk of entering the care system. A systematic search of databases (e.g., PubMed, CINAHL, PsychINFO) was conducted for randomised controlled trials (RCTs) from 1990–2024. Eligible studies were assessed for risk of bias using the Revised Cochrane Risk-of-Bias Tool. Meta-analyses used random effects models to estimate standardized mean differences (SMD) or odds ratios (OR). Narrative synthesis was provided for outcomes not appropriate to include in meta-analyses. Fifteen RCTs (n = 2232 families) were included. Interventions did not demonstrate consistent effects on reducing subsequent maltreatment, as measured by official records (OR = 0.88, 95% CI: 0.75–1.02) or parent-reported risk of harm (SMD = -0.07, CI: -0.25–0.11). However, significant improvements were observed in parental sensitivity (SMD = 0.53, CI: 0.30–0.76), attachment organisation (OR = 2.17, CI: 1.64–2.87), and parental functioning (SMD = -0.21, CI: -0.36– -0.06). In conclusion, psychosocial interventions show promise in improving parenting among edge-of-care families but lack consistent evidence for reducing maltreatment risk. Future research should prioritise larger trials with standardised outcome measures to strengthen this evidence base.

Introduction

Young children rely on nurturing care and protective and enriching interactions with caregivers to foster optimal development (Britto et al., 2017). Early exposure (i.e., during infancy and preschool period) to adverse experiences, such as abuse and neglect, have a significant impact on child development due to the brain being most sensitive to environmental input during this period (Harden et al., 2016).

There are large numbers of families involved with child protection services (CPS) worldwide. Children of these families are considered to be "on the edge of care", as they are at risk of being separated from their birth families and entering the care system. In England, 50 780 children were subject to a child protection plan in 2023, with approximately 33% of these involving children under 5 years of age (NSPCC, 2023). There is also data to show that in the UK children under one year constitute a significant proportion of cases undergoing care proceedings and this has remained stable over a 10-year period (Broadhurst et al., 2018).

Children who are under the care of CPS are often exposed to chronic and co-occurring adversities, such as domestic violence, parental substance use disorder or mental health problems, and poverty. These factors place children at increased risk of experiencing abuse, neglect or being exposed to violence, and are associated with significant adverse developmental outcomes (e.g., attachment problems, and poor emotional, behavioural, and educational outcomes) and disease vulnerability (Gilbert et al., 2009; Lippard & Nemeroff, 2020).

In circumstances where child protection involvement is unable to mitigate the risk of significant harm to the child, children will be removed from parental care into alternative out-of-home care; an outcome which is associated with substantial societal and healthcare costs (Conti et al., 2017). In addition to being costly, children can be negatively affected by multiple

placement transitions that are characteristic of out-of-home care, further exacerbating the developmental impact of adverse experiences on the child (Brown & Ward, 2014). Where early intervention is not provided to families, children are more likely to experience cumulative adverse experiences, which can lead to the development of more complex needs and create further difficulties in children obtaining placements in alternative care (Morse, 2019). Thus, interventions that aim to prevent removal to out-of-home care by supporting families with young children involved with CPS to reduce risks and increase the quality of parenting, are a high priority for the social care system.

A challenge for clinicians and care providers who support young children and their families with significant child protection concerns is a lack of clarity with regard to what interventions are effective in reducing risk and increasing the likelihood of children remaining in the care of their biological parent(s). Of the interventions that have been developed for families of young children to reduce risk of harm, there is a focus on improving outcomes in one or more of the following areas: parental functioning or well-being (e.g., addressing mental health problems or substance use), parenting skills, and the quality of the parent-child relationship. Interventions vary and may be more skills-based (e.g., Parent-Child Interaction Therapy, Parent Training, Reminiscing and Emotion Training), with a relational approach (e.g., Attachment and Bio-behavioural Catch-up, Circle of Security, parent-child psychotherapy), or tailored to parents with substance use problems (e.g., Parents Under Pressure).

While some of these interventions have evidence of their effectiveness—for example, the clinical guidelines in England recommend video feedback interventions, behaviour training and parent-child psychotherapy for children who are at high risk of entering or re-entering the care system (NICE, 2015)—it is not clear how effective these interventions are for families with

children 0-5 years old and/or what particular interventions are most effective for those at risk of entering the care system during this critical period of development. Of the systematic reviews that exist, most have focused on interventions where children are exposed to one specific type of maltreatment (e.g., physical abuse, Kennedy et al., 2016), evaluated the impact of one specific intervention (Grube & Liming, 2018) or one kind of intervention (e.g., relationship-based interventions, Bergsund et al., 2023), or have included samples of children that are at increased risk for maltreatment but not exclusively children who are already engaged with CPS (Casillas et al., 2016; Elkan et al., 2000; Higgins et al., 2006; Levey et al., 2017; Selph et al., 2013).

It is also rare for reviews to focus on families of children in the early years exclusively. Casillas et al., 2016, and Landers et al., 2018 are exceptions but are limited in drawing conclusions about the highest standard of evidence relating to interventions for families and their children already involved with CPS. Casillas et al., (2016) is a meta-analytic review focusing on home visitation programmes and explored the impact of different implementation factors on programme outcomes. Studies included in this review targeted a combination of universal and at-risk populations, rather than those already involved with CPS, and encompassed a mixture of study designs. They conclude that home visitation programmes are most effective in reducing maltreatment and improving parenting, and less effective at improving children's health or behaviour. Their results also suggest that supervision of practitioners and the fidelity of programme delivery are associated with better outcomes. Landers et al., (2018) is a scoping review of evidence-based interventions for families with young children involved with CPS that included a wide range of study designs. The review did not include quality ratings of the studies included, nor did it include a meta-analysis; however, they conclude that there was a relative lack of studies that had used rigorous RCT designs to evaluate interventions and few studies which

included measures of maltreatment despite the reduction of maltreatment being a desired outcome.

The limited conclusions that can be drawn from existing reviews presents researchers, commissioners, and practitioners with problems in terms of knowing which interventions have the best outcomes and therefore what programmes should be invested in, particularly where it relates to very vulnerable children in the early years, who are on "the edge" of entering the care system. This review aims to address this evidence gap by providing a rigorous appraisal and synthesis of the evidence regarding psychosocial interventions for families with children aged 0-5 years involved with CPS, to evaluate their effectiveness in reducing risk of harm and improving parenting, to inform guidance and policy, and to identify priorities for future research.

Research Questions

Are existing interventions effective in reducing risk of harm (e.g., maltreatment or abuse potential) for children aged 0-5 years who are involved with child protection services and thereby at risk of entering the care system?

Are existing interventions effective in improving parenting quality or parental functioning in parents of children 0-5 who are at risk of entering the care system?

Method

Search strategy and inclusion criteria

Our protocol was pre-registered with PROSPERO *[reference hidden for review]*, and the only deviation involved a shift following the search stage from an umbrella review of reviews to a focus on primary studies only. This was due to the inadequate reporting of results in the reviews, in addition to the wide variability in study design and quality. The identified reviews

were nevertheless used to identify individual eligible studies. We also did not undertake the planned component analysis due to a lack of resources in terms of time and funding.

Initially, as planned in the protocol, the searches were for systematic reviews and meta-analyses. To be eligible, review articles had to include at least one eligible RCT, which had to be: a trial of a psychosocial intervention(s); delivered with families with children aged 0-5; and in which the child was on the edge of care—i.e., already involved with child protection/welfare services. Exclusion criteria were: absence of a description, assessment, or screen for this definition of "edge of care"; interventions that were not delivered or targeted at any point during the early years period; not available in the English language; child not living with family of origin (e.g., in foster care); did not use systematic review or meta-analysis methodology. See Table 1 for search terms.

We searched the following databases in July 2020 (time-limited to since 1990) for systematic reviews of RCTs: ASSIA, CINAHL, EMBASE, PsychINFO, PubMed, Cochrane, DARE, JBI, Campbell and Prospero. Duplicates were removed using Zotero reference manager. Reference lists were checked for other relevant reviews and the Prospero protocol authors contacted to request full texts if already published or submitted. Screening of titles and abstracts was completed by three members of the team, and 15% (n=40) full texts checked together, with 100% agreement. The resulting selection was 24 reviews which had at least one primary study that was eligible based on information available in the reviews' text. From this process, which was followed by an inspection of the primary studies themselves (described next), our wider team reached a consensus that 5 primary studies from these reviews were eligible.

Following the decision to focus on primary studies only (as explained above as a protocol deviation), the searches were updated from the date of the searches conducted in the three

reviews more relevant to our research questions (which had all been in 2018) until the present date, March 2024 – maintaining the same search terms and criteria except that we removed 'review' and 'meta-analysis' and added filters to restrict to RCTs, in order to find the primary studies directly. Where it was available, we restricted searches to 'children' and 'peer-reviewed' documents. The same databases were searched, except those aimed at reviews, meaning we searched: PubMed, Proquest (previously ASSIA), CINAHL, EMBASE and PsychINFO. Duplicates were removed using Zotero reference manager. We applied the exact same inclusion and exclusion criteria to the primary studies as described above. Selection based on title and abstract was done individually by three members of the team, and inspection of full texts was conducted, at a minimum, in pairs. Ambiguous studies were discussed with the wider team until a consensus was reached.

Following this process, we obtained 21 articles, corresponding to 15 unique studies. See flow diagram in Fig. 1 for a breakdown of numbers.

Analytic plan

Data extraction was conducted by four members of the team, with at least one second person checking accuracy. When sufficient data to calculate effect sizes was not reported in the articles, or when it was not clear if samples were overlapping across publications, authors were contacted for further detail. Effect sizes for the relevant outcomes were extracted for the meta-analyses: the primary outcome was risk of significant harm, and the secondary outcomes were quality of parent-child relationship, observed parenting quality, self-reported parenting, and parental functioning. As the focus of this review was on risk of harm and parenting, we did not report child outcomes. For this reason, any articles that only reported child outcomes were excluded (e.g., follow-up reports relating to studies that were included, presenting data on child's

cortisol regulation or negative affect, were excluded from this review, Cicchetti et al., 2011; Lind et al., 2014; Bernard et al., 2015). When post-treatment and short-term follow-up assessments were repeated for the same measure (e.g., 3- and 6-months follow-ups), effects across time were averaged. Long-term follow-up effects (i.e., years later) were not included in meta-analyses because they were seldom available, but are presented in the narrative synthesis. An exception to this was the meta-analysis of official reports of maltreatment, where the outcome was reported for a single time-period which ranged 1-7 years across studies and in these cases, they were included in the meta-analysis because it was not a follow-up but rather the main post-intervention outcome (MacMillian, 2005; Johnson-Reid, 2018; Lee, 2018; Hall, 2021; Oxford, 2016; Valentino et al., 2019). In all meta-analyses, when available, we used intent-to-treat data (when only completer/per-protocol data were available, this can be identified by differences between *N* randomised (Table 2) and *n* analysed in each meta-analysis (Figures 3-9)). Due to the small number of studies available for each meta-analysis, we aggregated effect sizes when there was more than one from the same study, rather than conducting multivariate meta-analysis (Villodas et al., 2021 and Skowron et al., 2024 for sensitivity; Villodas et al., 2021 for self-reported parenting; Johnson-Reid et al., 2018, Barlow et al., 2019, and Skowron et al., 2024 for parental functioning). If different effect sizes were in different directions, they were reversed to be consistent (Villodas et al., 2021 and Skowron et al., 2024 for negative/insensitive parenting scales; Villodas et al., 2021, MacMillan et al., 2005 and Skowron et al., 2024 for inconsistent discipline/negative attributions scales). Effect sizes were converted to Standardised Mean Differences (SMD) or Odds Ratio (OR) when appropriate (i.e., for continuous scores and frequencies, respectively), and all meta-analyses were conducted using the more conservative random effects model, using R studio. Heterogeneity of effect sizes was assessed using the Q

statistic, and when it was found to be high, reasons for this were explored (e.g., by visually inspecting the forest plots and comparing the measures used in the studies), but subgroup analysis was not possible due to the small *k*. Because meta-analyses were based on few studies, which creates difficulty in estimating the between-study variance, sensitivity analyses were ran to compare CIs under the Hartung-Knapp-Sidik-Jonkman (HKSJ) method to the default Wald-type method; when conclusions are different this is described under Results.

Results are described in a narrative synthesis alongside the meta-analytic findings, when these are available. When there were insufficient effect sizes for computing a meta-analysis, but a study reported relevant outcomes (e.g., studies measuring substance use), these findings are included in the narrative synthesis only.

Risk of bias assessment

Overall risk of bias judgements were made based on the MRC Risk of Bias tool for randomised trials (Sterne et al., 2019). Two independent raters assessed the risk of bias for each RCT with the Revised Cochrane risk-of-bias tool for randomised trials (RoB 2) which includes five domains that may pose a risk of bias: random sequence generation, allocation concealment, blinding of participants and personnel, blinding of outcome assessment, incomplete outcome data, and selective reporting. No studies were excluded based on their risk of bias rating.

Results

Sample Characteristics

Fifteen independent studies were included, with a total sample size of 2232 families. See Table 2 for study and participant characteristics. The majority of studies were conducted in the USA; where not in the USA, 1 was conducted in the UK, 2 in Canada, and 1 in Spain. Where precise child age was reported, the mean age of the children pre-treatment was 3 years (mean

range: 9 months-5 years). Parents included in the interventions were most often mothers or female caregivers. Samples varied in their ethnic make-up with many studies having a high number of participants from ethnic minority backgrounds. Where socio-economic class was reported, all studies involved a high proportion of families living in poverty or in receipt of benefits.

Intervention Characteristics

The majority of interventions were delivered in the parent's home. Interventions ranged from 4 weeks to 2 years in duration. See Table 2 for further detail.

Overview and description of interventions

Of the 15 independent studies included, interventions varied in their emphasis or delivery but most had multiple components with the aim of promoting positive parenting and reducing risk of harm.

Lee et al. (2018) and Johnson-Reid et al. (2018) evaluated well-established home-visitation programmes: the Healthy Families America (HFA) programme and Parents as Teachers (PAT), applying them to families involved with CPS. Both PAT and HFA are paraprofessional home-visiting programmes with child development and parenting elements which are offered for up to 3 years if the child is enrolled at birth. Elements of the programme are used flexibly by the practitioner depending on family needs. The study by Lee et al. (2018) described outcomes from a sub-sample of mothers who were involved with CPS from a larger Healthy Families study. MacMillan et al. (2005) tested a novel home-visiting programme based on a manual that had been developed during a pilot study (MacMillan & Thomas, 1993) that was then further refined during the RCT. Components of the intervention are similar to those included in other home-visiting programmes, including goal setting, problem solving, parenting

principles, and signposting parents to other community resources relating to their well-being or safety concerns.

Hall et al. (2021) evaluated the use of a child-welfare led intervention which has previously observed favourable outcomes using quasi-experimental designs in the United States amongst parents with substance use problems. The Sobriety Treatment and Recovery Teams (START) program is designed for families with co-occurring child maltreatment and substance use, working in collaboration with child welfare, substance use treatment providers, and the courts. The START model is goal focused, supporting parents to gain quick access to treatment services and workers are trained in using motivational interviewing. Each child welfare worker is paired with a family recovery mentor who is a parent with history of involvement with child welfare services and who has achieved sustained recovery from a substance use disorder.

Some of the programmes included in this review were underpinned by attachment theory; these include Attachment and Bio-behavioural Catch up (ABC; Bernard et al., 2012) and Infant-Parent Psychotherapy (IPP; Cichetti et al., 2006). The original sample by Bernard and colleagues (2012) reported different outcomes in multiple publications and followed children up over time (Lind et al., 2020, Raby et al., 2021; Zajac et al., 2020). ABC was originally designed for use with foster parents but in this RCT it was adapted for use with parents identified by CPS as being at risk for maltreatment. ABC aims to reduce frightening maternal behaviours associated with disorganised attachment relationships and enhance sensitive and nurturing care. IPP does not have a direct focus on changing caregiving behaviour and instead works at the level of the caregiver's internal representation, with the assumption that improving how the mother perceives herself, and herself in relation to her infant, will enable sensitivity and responsiveness, thus fostering security in the parent-child relationship (Cichetti et al., 2006). A number of

interventions used video-feedback as a core treatment element with the aim of improving parental sensitivity; this included Promoting First Relationships (Oxford et al., 2016), Attachment Video-Feedback Intervention (Eguren et al., 2023) and the intervention by Moss et al. (2011).

Three studies evaluated interventions underpinned by behavioural theory focusing on parent's acquisition of positive parenting and discipline skills (e.g., Skowron et al., 2024; Whitaker et al., 2020; Villodas et al., 2021). Parent-Infant Interaction Therapy (PCIT) which was evaluated in two studies (Skowron et al.,2024 and Villodas et al., 2021) is a structured parenting programme, which involves the clinician providing live coaching to the parent of learned strategies. The Behavioural Training Programme evaluated in Whitaker et al. (2020) includes didactic modelling and practice as key components of the parenting intervention.

Valentino et al (2019) focused on the evaluation of a Reminiscing and Emotion Training (RET) programme. RET is designed to facilitate emotionally supportive parent-child communication, which has been associated with improved memory, language development and emotion regulation in children. Maltreating parents have been identified as having deficits in how they talk about emotions with their children, and thus this has been identified a target for intervention. In RET, families are assigned a family coach who provides training to parents to increase the elaboration and sensitive guidance during day-to-day conversations with their children. The intervention is structured and short-term. Outcomes from this study have been published within multiple publications (Edler et al., 2024; Speidel et al., 2020; Valentino et al., 2019).

Two studies specifically targeted parents with substance use problems (Barlow et al., 2019; Donohue et al., 2014). The Parents Under Pressure (PuP) program draws on theoretical

frameworks from attachment, behavioural parenting, and adult psychopathology and includes 20 modules that can be used flexibly by the practitioner based on the families' needs. It aims to improve the quality of caregiving and improve parental emotion regulation. Donohue et al. (2014) evaluated a modified version of Family Behaviour Therapy (FBT) which has been previously evaluated in a non-parenting population of adults who use substances. In this study, FBT worked with family and friends of the person engaged with substances and included components related to contingency management, communication training, coping skills and parenting strategies.

Quality Appraisal

Fig. 2 displays the outcomes from the quality appraisal. Seventy-one percent (15/21) of studies were judged to be at low risk of bias. Only 1 was judged to be at high risk of bias due to the method of randomisation. The remaining studies were judged to have some concerns in at least one domain but not at high risk of bias in any area. Other concerns of possible risk of bias were due to lack of allocation concealment, lack of blinding, lack of detail/problems with how missing data was managed, and difficulty to ascertain if selective reporting had occurred.

Primary outcome: Risk of Significant Harm

See Table 3 for a synthesis of the study outcomes. A total of 11 studies included outcomes related to risk of significant harm. Two separate meta-analyses were conducted for the effect sizes relating to official reports (e.g., social care records) and parental self-report (e.g., Child Abuse Potential Inventory, or CAPI). See forest plots in Fig. 3 and 4. The meta-analysis for self-reported risk of significant harm ($k = 5$) did not show differences between treatment and control groups post-treatment (g = -.48 CI = -1.23–0.28), but heterogeneity was very high (Q = 34.84, $p < .001$). One study was an outlier (Villodas et al., 2021), showing a much larger effect

size and a confidence interval (CI) that did not overlap with the remaining studies, but a sensitivity analysis excluding this study (k = 4), despite resulting in much lower heterogeneity (Q = 2.18, p = .54), showed a smaller overall estimate and no evidence of a treatment effect (g = -.07 CI = -.25–.11). We report these findings in Fig. 3 without the outlier.

The meta-analysis for official reports of significant harm (k = 6) showed group differences that just passed the threshold for statistical significance . While most effect sizes favoured treatment, their CIs were wide, and the larger study (Hall et al., 2021) did not favour the treatment group (OR = .84, CI = .72–.98, Q = 5.82, p = 0.32). See Fig. 4. This effect was not validated by the sensitivity analysis using the HKSJ CI method (OR = .84, CI = .69–1.01, Q = 5.82, p = 0.32), meaning that due to the small number of studies and uncertainty, the evidence for an effect is inconclusive.

There were additional relevant findings not captured by those two meta-analyses. Although there was no main effect of treatment in terms of subsequent maltreatment reports in the study by Johnson-Reid et al. (2018), they did find moderated treatment effects based on prior CPS history and maternal depression. Among families with no prior child protection involvement, the treatment group had fewer re-reports to CPS than the control group (11.5% versus 50%, x^2= 6.22, p=.012). For families where the mother scored in the clinically significant range of depressive symptoms at the start of treatment, there was no difference in re-reports, whereas there was a significant difference between the treatment and control group amongst mothers who did not report clinically significant depressive symptoms (25.7% vs 67.6%, x^2=10.96, p<.001, RR: 0.43).

In addition to incidence of subsequent maltreatment included in the meta-analysis, Hall et al. (2021) also reported on the incidence of any out of home care and reunification. Although

there was no statistically significant difference in terms of incidence of subsequent maltreatment (included in the meta-analysis) or out of home care or reunification in either the intent-to-treat or per protocol analysis, there was a relative, but not statistically significant, difference with respect to out of home care favouring the treatment group amongst those families who actually received treatment. Amongst the subgroup of families who had received treatment, the treatment group had fewer episodes of out of home care (24.7% versus 31.5%) and for those who did have an out of home placement, a greater number in the treatment group were reunited (60.5% versus 47.4%).

Although Oxford et al. (2016) did not find any significant group differences in subsequent maltreatment, they did find that children in the intervention group were less likely to be removed from the home within the subsequent year (i.e., 5.6% of children removed from the home compared to 13.0% in the control group; hazard ratio of 2.5, p=.04, CI = 1.03–6.10).

Secondary outcome: Quality of parent-child relationship

Attachment. Four studies included outcomes related to the parent-child attachment relationship. We ran separate meta-analyses for organisation and security of attachment, respectively.

First, organised vs disorganised classifications were available for the studies using the Strange Situation Procedure (k = 3). There was a significant difference between groups in terms of organisation of attachment at post-treatment, with the treatment groups being twice as likely to be classified as organised than controls, which is a substantial effect (OR = 2.17, CI 1.64–2.87). The Q statistic indicated low heterogeneity (Q = 1.70, p = 0.43). See Fig. 5.

For security of attachment, first we ran the analysis with all available effect-sizes (k = 4), which included Strange Situation categorical ratings and one continuous score based on a Q-sort

assessment (Oxford et al., 2016). There were no significant group differences (g = .81, CI = -.06–1.68), and heterogeneity was very high (Q = 20.00, $p < 0.001$). We repeated the analysis having excluded the single continuous, Q-sort effect size (Oxford et al., 2016), with $k = 3$. This analysis, including only outcomes from the Strange Situation procedure, showed significant group differences in attachment security (versus insecurity), favouring the treatment group (OR = 3.03, CI = 1.06–8.67); however, heterogeneity remained high (Q = 10.49, $p < 0.01$), with a much larger effect size and wide CI from one of the studies (Cicchetti et al., 2006). Nevertheless, all the CIs from these Strange Situation effect sizes ($k = 3$) were within the treatment-favouring side, supporting the conclusion that the interventions led to improved attachment security. However, caution is warranted given that this result was not robust using either the Wald-type or the HKSJ CI methods, the latter providing an even wider CI (.27–33.71), therefore the true effect size could range from negligible to large. See Fig. 6.

Although in the study by Oxford et al. (2016) there was no main effect of treatment on secure base behaviour there was a significant indirect effect via increased parental sensitivity, and this was more evident in parents with a history of physical abuse (Pasalich et al., 2019).

Similarly, in later follow-up investigations with the Bernard et al (2012) sample, parents from the intervention group demonstrated greater secure base script knowledge when their child was 8 years old, than parents in the control condition and this was positively associated with observed parental sensitivity during a structured task (Raby et al., 2021). Children from this study also reported higher levels of attachment security when they were 9 years old (Zajac et al., 2020).

Observed Parenting: Sensitivity. Seven studies included measures of observed parental sensitivity and these showed significant group differences favouring the treatment group (g

= .80, CI = .27–1.33). One outlier (Villodas et al., 2021) had a CI outside the remaining CIs, and heterogeneity was very high (Q = 55.59, $p < .001$). Therefore, the analysis was repeated while excluding the outlier, which reduced heterogeneity significantly, even though it remained relatively high (Q = 14.99, $p = .01$). Results were maintained, with a moderate effect indicating improved sensitivity at post-treatment in the intervention arm (g = .53, CI = .30–76). While this benefit on parental sensitivity seems moderate, some residual heterogeneity suggests that effects still vary across programmes. See Fig. 7.

Oxford et al. (2016) also measured child atypical affective communication as part of a home observation and found a small but significant post-intervention effect in favour of the treatment group.

The Bernard et al. cohort was followed up at 36 months and 8 years post-treatment where parental sensitivity was observed. At 36 months, parental sensitivity was observed during a waiting task using second by second coding, with parents who engaged in the ABC intervention demonstrating more sensitivity than those from the control condition, although the difference only approached significance (Lind et al., 2020). As described above, when children were 8 years old, there was an indirect intervention effect for parental sensitivity, via its effect on parents' secure base script knowledge (Raby, 2021).

In addition to parental sensitivity, Eguren et al. (2023) reported other aspects of observed parenting using the Emotional Availability Scales. Post-intervention, parents in the treatment group were also observed to provide more structuring and to be less intrusive when compared to the control group, and these differences were significant. There were no differences between groups in terms of parental non-hostility.

Self-Reported Parenting. Six studies assessed parents' self-reported parenting quality or attitudes (e.g., Alabama Parenting Questionnaire; k = 6) but showed no group differences (g = .36, CI = -.06–.78), with significant heterogeneity (Q = 31.91, p < .001). Removing the outlier (Villodas et al., 2021) reduced heterogeneity (Q = .61, p = 0.96) but there was still no significant treatment effect (g = .14, CI = -.01–.28). The sensitivity analysis using the HKSJ CI method suggested a treatment effect (g = .14, CI = .05–.22), however the null result reported above, using the Wald-type, is likely more accurate, and together these results indicate that any benefit is modest at best. See Fig. 8.

Self-Reported and Observed Parenting Context and Environment. Outcomes that were not included in the meta-analysis included a subset of scales from the Protective Factors Survey (Whitaker, 2020), the Confusion Hubbub and Order Scale questionnaire to measure household chaos (Eguren et al., 2023), and the HOME as a measure of household adversity (MacMillian, 2005). Of these three outcomes only Eguren et al. (2023) demonstrated a difference between the treatment and control group, with parents in the treatment group reporting significantly less household chaos post treatment.

Skowron et al. (2024) included the Parent Attributions Test (PAT) as an outcome, to measure any change in how parents attribute responsibility or their locus of control (either to the parent or the child) in different hypothetical parenting scenarios. This study found no difference between groups in terms of parental attributions.

Secondary Outcome: Parental functioning

Seven studies assessed parental functioning which included self-reported measures of parents' mental health difficulties, parenting stress and self-regulation (k = 7). There was a weak-to-moderate group effect in parental functioning, favouring the treatment group (g = -.41, CI =

-.84– -.02), but very high heterogeneity (Q = 34.05, $p < .001$). Again, heterogeneity was greatly reduced by excluding the outlier (Villodas et al., 2021) (Q = 7.81, $p = .17$), and while the effect reduced in size, it remained significant favouring the treatment arm (g = -.21, CI = -.36– -.06). This reflects a small but statistically significant reduction in parents' mental-health difficulties and stress. See Fig. 9.

A number of outcomes related to parental functioning were not included in the meta-analysis. Donohue et al. (2014) evaluated the impact of treatment on the number of days of drug and alcohol use and number of days of incarceration immediately post treatment and 4 months after treatment. They found a significant main effect for drug and alcohol use over time with a reduction in use in the treatment group, and a marginally significant main effect with respect to the number of days incarcerated, with the treatment group demonstrating fewer days of incarceration during and following the intervention period.

Jonson-Reid et al. (2018) included an assessment of parental self-reported symptoms of depression at 18 months following the start of the intervention, and in line with measurement at earlier time points, there was no difference in self-reported depression between groups.

A follow-up study of the Barlow et al. (2019) sample included an examination of moderators and mediators of treatment outcomes (Dawe et al., 2021). The authors found that families with children in the older range of eligibility for the study (in this case, up to 2.5 years) showed greater decreases in child abuse potential than families with younger children. Parental substance use did not moderate response to treatment, but parents with higher baseline emotional dysregulation showed greater reductions in child abuse potential across time in both the treatment and control group. They also found that better emotion regulation post-treatment was

associated with greater reductions in child abuse potential at 6-month follow-up (Dawe et al., 2021).

Finally, in the study by Skowron et al. (2024) differences in parents' executive functioning were found in the experimental task (stop-signal task) with the group receiving PCIT showing significantly lower stop signal reaction times indicative of better inhibitory control. This was in contrast to the self-reported measure of executive functioning (Behaviour Rating Inventory of Executive Function, or BRIEF) where there was no difference between groups on overall score. However, when individual subscales of the BRIEF were examined, parents in the PCIT group showed fewer emotional control problems post-treatment than the control group. There were no differences on other aspects of executive functioning captured by the BRIEF (self-monitoring, attentional shifting and behavioural inhibition).

Discussion

Summary of key findings

The need for effective and early intervention to reduce long-term effects of adverse experiences for young children is vital. This meta-analysis and narrative review of RCTs was conducted to address a gap in the literature with respect to the current state of intervention effectiveness for families involved with CPS within the first 5 years of the child's life. Overall, the results suggest that while interventions targeting families with children in the early years, who are on the edge of care, are not consistently effective in reducing risk of significant harm with respect to official reports of subsequent maltreatment or parent-reported potential for abuse and neglect, they nevertheless appear to improve observed parental sensitivity and reduce the risk of disorganised attachment (possibly also insecure attachment), in addition to improving parental functioning. However, it is worth noting that these outcomes were not all measured

across the various studies, hindering the ability to differentiate the effects of various interventions on specific outcomes.

Consistency with the wider evidence base

In relation to our primary outcome, we failed to detect robust statistically significant differences between intervention and control group in maltreatment reports or abuse potential at post-treatment. This is in line with similarly disappointing findings in a large umbrella review covering reviews between 2014–2018, which showed significant combined effects of interventions on maltreatment but not when selecting the best meta-analytic evidence or after correcting for potential publication bias (vanIJzendoorn et al., 2020). In the current review we only included RCTs and more restrictive inclusion criteria, namely families already involved in CPS and with children 0-5 years-old.

There are some indications of sample characteristics that may justify differences in treatment effectiveness, though they come from reviews not restricted to studies of a specific child age or level of risk. Chen and Chan's (2016) review analysed the effectiveness of programs that could be for primary, secondary, or tertiary prevention of maltreatment, and found significant treatment effects, but this contrasts with the non-significant findings in a review of maltreatment prevention focused specifically on families with no known current or past maltreatment (Viswanathan et al., 2018). Euser et al.'s review (2015) found some evidence of a reduction in maltreatment but only amongst already-maltreating samples and not in at-risk samples. Taken together with the remaining literature, these findings suggest that the efficacy of interventions may depend on the target population and level of risk for child maltreatment (Viswanathan et al., 2018; Casillas et al., 2016). In the current review we focused on samples already involved with CPS, but the reasons for this involvement could be varied and

multifaceted. Interestingly, Chen & Chan (2016) obtained significant effect sizes when maltreatment was measured via official substantiated child maltreatment rates as well as in the Parent-Child Conflict Tactics Scale (i.e., parental harsh discipline, corporal punishment and neglect), yet the effect was not statistically significant for studies assessing parents' potential for child maltreatment using the CAPI.

Given the small number of studies which met inclusion criteria for the current review, we chose to include studies that had either official reports of maltreatment, parent-reported maltreatment (e.g., harsh parenting) and/or the potential for child maltreatment (e.g., CAPI), even though these different measures might capture risk for maltreatment differently and self-report measures of risk are particularly subject to bias. A review by Whitcombe-Dobbs & Tarren-Sweeney (2019) that specifically sought to review parenting interventions that included measurement of subsequent maltreatment using official CPS reports amongst maltreating families found only 9 studies that met these criteria, highlighting the relative absence of studies that include subsequent maltreatment as an outcome. Results from this review would suggest that this data is even more scant when focused on outcomes for children 0-5 years. It is also plausible that the inconsistent findings from official reports of child maltreatment in the current review might be, at least partially, justified by measurement error due to the complexities inherent to obtaining this type of data (which may be especially true with younger children) and heterogeneity in reporting criteria (e.g., the definition of child welfare reinvolvement in Edler et al., 2024, may be broader than number of incidents of abuse and neglect, considered in other studies), as well as the variable time windows included in the different studies. Moreover, the only study where the direction of the intervention effect on maltreatment did not favour treatment, Hall et al. (influencing the results of the overall meta-analysis) included other relevant

outcomes such as out of home care and reunification which did favour the treatment group, though this effect was not significant and the sample size small. Overall, the promising trend in reduction of maltreatment recidivism following intervention warrants further interest and research.

Regarding secondary outcomes, our findings align with previous reviews with respect to evidence of programmes improving parenting skills and the quality of parent-child interaction (Castillas et al., 2015; Chen & Chan 2016). Indeed, we found overall positive intervention effects in improving the quality of parenting and attachment, particularly in measures relying in the observation of the parent-child interaction (e.g., attachment organisation and sensitivity), which is promising given the evaluation by a trained rater. On the other hand, the two reviews just cited reported mixed results for parental functioning, while we did find a positive treatment effect. In this review, studies varied greatly in the outcomes and instruments used to assess parental functioning. We were conservative in only including relatively comparable measures in the meta-analysis (i.e., only parent's report, and focused on parenting stress and emotion regulation-related difficulties), which held a significant weak-to-moderate treatment effect. Other outcomes of parental functioning (which we reported in a narrative form only) had more inconsistent findings.

Nevertheless, it is possible that these outcomes do not reflect real nuanced differences in what can be improved, but instead relate to the type of intervention and differences in samples, and thus it is not easy to tease apart their differential impact from the kinds of outcomes each study chooses to measure. We would argue that both mechanisms of change (e.g., change in parenting or relationship quality, parental attitudes and mental health) and outcomes in terms of

substantial harm both need to be measured consistently in every evaluation that wishes to reduce or prevent further maltreatment in families already involved with CPS.

Strengths and limitations of the review

This review provides a focused examination of a highly vulnerable group—children aged 0-5 on the edge of care—filling a notable gap within the literature, with much of the existing evidence for this group of children being 'buried' within reviews focusing on groups of children with wider characteristics in terms of age and risk exposure. Our methodological rigor, including a comprehensive search and critical appraisal of the included studies was undertaken, as was the use of at least two reviewers at key points in the review process.

However, some limitations persist. Although a protocol for the review was published at the outset, it was not possible to complete all aspects of the research as planned, in terms of conducting an umbrella review of reviews. This is because most reviews identified included a diverse group of programmes, designs and samples, the majority of which did not meet our specific inclusion criteria. As a result, we adapted the search strategy such that we searched for primary studies post the search date of the reviews, also including those studies from the identified reviews that were eligible. The additional component analysis that had been planned was also unfeasible, and would have been challenging given the lack of comparable interventions across studies. The published protocol was, however, followed in all other respects.

Relatedly, it was not possible to compare outcomes for different intervention programmes due to there being too few studies and none which had evaluated the same intervention with this particular population of parents and children.

The small number of studies included in each meta-analysis may also render the estimates unstable (and the CIs reported are likely too narrow). We conducted univariate meta-

analyses and sensitivity analyses to account for that, but a cautious interpretation is nevertheless warranted.

Limitations of the Evidence Base

Small sample sizes (meaning most studies were likely underpowered to detect effects), high attrition rates, and challenges in maintaining long-term engagement in many studies highlight ongoing challenges in conducting rigorous research with edge-of-care families. Variability in measurement tools, differences in study designs, and non-random therapist allocation (e.g., Villodas et al. and Skowron et al.) further complicate generalisability and meta-analytic synthesis. High heterogeneity was another limitation in this and other reviews; here, Villodas et al. (2021) emerged as a particular source of heterogeneity, reporting larger effect sizes than other studies, albeit in the same direction (i.e., favouring the treatment group), based on a small sample of dyads. Future studies with larger, multi-site samples could offer more comprehensive insights into intervention efficacy, including an examination of moderation and long-term effects, helping to address the pressing need for more effective and accessible support for edge-of-care families.

Despite our comprehensive searches, we found a relatively small number of studies that met our criteria which aimed to gather evidence specific to the early years and an unambiguous population (i.e., families already involved with CPS). It is plausible that interventions have heterogeneous effects depending on other family characteristics, but the scarcity of studies meant that we were not able to analyse that in respect to important variables such as country (most studies were conducted in the USA), ethnic minorities, non-native speakers and fathers as programme recipients (the vast majority of participating caregivers were mothers). Given the very significant impact that maltreatment and placement of children in the care of authorities has

on children themselves, families and society, investing in finding better ways to intervene would seem an urgent priority, but the evidence base is rather thin.

Directions for future research

This review highlights the need for replication studies and collaborative research efforts, following what is happening for other topics such as the collaboration on attachment transmission synthesis (Verhage et al., 2020). Given the diversity in interventions and outcomes, a standardised, multi-site approach across countries may help build a stronger evidence base, particularly for a challenging population in terms of recruitment and retention. Larger sample sizes and extended follow-up periods are critical for understanding intervention effects, moderation, and long-term outcomes. Additionally, consensus on outcome measurement for edge-of-care interventions would improve comparability and enhance future meta-analytic syntheses.

Recommendations for Policy and Practice

The findings underscore the pressing need for effective interventions to mitigate the risks associated with child maltreatment and family separation, particularly for families already engaged with social care services. While the small number and generally modest sample sizes of studies in this review may limit immediate policy applications, the effects on parental sensitivity and attachment suggest promising avenues for targeted support programs. Additionally, the Bernard et al. (2012 and subsequent articles) study illustrates potential long-term engagement benefits for families at high risk.

Studies such as Hall et al. (2021) and Oxford et al. (2016) showed differences favouring the treatment group in terms of metrics like reunification and removal from home, suggesting there may be other nuanced social care benefits of interventions beyond that reflected by parental

self-reports or official reports on maltreatment recurrence. There are strengths and limitations to different forms of measurement for maltreatment, but there is a rationale to ensure that studies do not only focus on changes in parenting and relationship (although these are important) and also evaluate the effect of interventions in reducing risk of further maltreatment (see Whitcombe-Dobbs & Tarren-Sweeney 2019 for a discussion). The need for robust sample sizes and consensus-driven outcomes emerges as critical for evaluating intervention impact accurately.

Critical Findings

- This review identified 15 RCTs evaluating psychosocial interventions for families with children aged 0–5 at risk of entering care, primarily conducted in the United States.
- While interventions showed consistent improvements in parental sensitivity, attachment organisation, and (to a lesser extent) parental functioning, they did not demonstrate consistent effects on reducing maltreatment recurrence or risk.
- Few studies evaluated outcomes beyond six months post-intervention, and there was high variability in measurement tools and procedures, limiting cross-study comparability and generalisability.
- Challenges such as small sample sizes, high attrition rates, and variability in reporting prevent further analyses and reduce the certainty of findings, highlighting the need for more rigorous and consistent evaluation methods.

Implications for Practice, Policy, and Research

- Practitioners working with families on the edge of care should prioritise interventions that enhance parental sensitivity and improve the parent-child relationship, given the evidence

of their effectiveness. Programs that incorporate video-feedback, attachment-based strategies, or behavioral parenting techniques appear particularly promising for fostering better outcomes. Structured approaches that address parents' emotional regulation and mental health may also provide significant benefits in family settings with co-occurring challenges, such as substance use.

- Policymakers should increase funding for interventions that show clear benefits for families in child protection contexts, increase investment in interventions tailored for children aged 0-5 years, and advocate for consistent evaluation frameworks to ensure effectiveness and comparability across regions.
- Future research needs to involve larger, multi-site trials to evaluate long-term outcomes and standardise measures of maltreatment prevention, attachment, and parental functioning. Research should also explore how factors like parental mental health and socioeconomic context influence intervention success.

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