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The efficacy of psychosocial interventions for grief symptoms in bereaved children and adolescents: A systematic review and meta-analysis

Christina Hanauer [a,1], Berit Telaar [a,1], Rita Rosner [a], Bettina K. Doering [b,*]

[a] Department of Psychology, Catholic University Eichstaett-Ingolstadt, Germany
[b] Department of Psychology, Kiel University, Germany

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ABSTRACT

Background: The present meta-analysis investigates the efficacy of psychosocial interventions in bereaved children and adolescents.
Method: We conducted a systematic review searching PsycINFO, PsycARTICLES, PubMed, MEDLINE, PSYNDEX, Web of Science, CINAHL and ERIC. Random-effects meta-analyses examined the effect of interventions on symptoms of grief, posttraumatic stress disorder (PTSD) and depression in controlled and uncontrolled studies.
Results: We included 39 studies ($n = 5.578$). Post-treatment, preventive interventions demonstrated a significant effect on grief (uncontrolled studies: $g = 0.29$, 95%CI [0.09;0.48]; controlled studies: $g = 0.18$, 95%CI [0.03;0.32]). For symptoms of PTSD and depression, only uncontrolled preventive studies yielded significant effects (PTSD: $g = 0.24$, 95%CI [0.11;0.36]; depression: $g = 0.28$, 95%CI [0.10;0.45]). Interventions targeting youth with increased grief-related distress demonstrated a significant effect in uncontrolled studies on grief ($g = 1.25$, 95%CI [0.94;1.57]), PTSD ($g = 1.33$, 95%CI [0.85;1.82]) and depression ($g = 0.61$, 95%CI [0.45;0.77]). A controlled effect size could only be calculated for PTSD symptoms ($g = 0.71$, 95%CI [0.15;1.27]).
Limitations: Interventions varied widely, contributing to high heterogeneity. Only a small number of studies with mostly limited quality could be analysed.
Conclusions: Psychosocial interventions may ameliorate grief symptoms in bereaved youth, especially when targeting youth with elevated grief distress. However, the effects observed in uncontrolled studies are substantially reduced when controlling for the natural course of bereavement. Given the increasing number of children worldwide bereaved through ongoing crises, research on interventions is surprisingly sparse.

1. Introduction

A substantial number of children and adolescents experience bereavement: Prevalence rates of childhood bereavement range from 7 % for a close family member to 62 % for a close relative (Burns et al., 2020; Paul and Vaswani, 2020). While bereavement is associated with various mental and physical health problems, such as concentration difficulties, fatigue and an increased vulnerability to subsequent stressors, most children adapt to it over time (Kennedy et al., 2018; Lytje and Dyregrov, 2019). A minority, however, develops mental health problems (Kentor and Kaplow, 2020; Keyes et al., 2014), e.g., depression, anxiety, posttraumatic stress disorder (PTSD), conduct disorder, or prolonged grief disorder (PGD). PGD was introduced recently in ICD-11 (WHO, 2018) and DSM-5-TR (APA, 2022). The introduction of this new diagnosis offers important opportunities: with the establishment of specified diagnostic criteria, it can advance research and improve the development of effective treatments. At the same time, the diagnosis poses challenges to researchers in the field of childhood bereavement (Dyregrov and Dyregrov, 2013): little is known about the reliability and validity of these criteria sets in children and adolescents (Boelen et al., 2019; Kaplow et al., 2018; Melhem et al., 2013). Additionally, PGD criteria for children require a developmentally informed adaption (Kaplow et al., 2012). Nevertheless, it is evident that a subgroup of bereaved children and adolescents needs professional support (Kentor and Kaplow, 2020). Worldwide crises as the COVID-19 pandemic (Kumar, 2023), the opioid crisis (Hulsey et al., 2020) and ongoing wars and armed conflicts (Kadir et al., 2019) are likely to increase this number and highlight the need to identify evidence-based support

* Corresponding author at: Department of Psychology, Kiel Universty, Grippstr. 2, 24118 Kiel, Germany.
E-mail address: doering@psychologie.uni-kiel.de (B.K. Doering).
[1] Christina Hanauer and Berit Telaar are joint first authors.

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options for bereaved youth.

The scope of bereavement care includes a wide range of interventions that target bereaved children and adolescents with clinical symptoms of depression, PGD or PTSD as well as non-pathological grief-related distress. The mode of delivery and intensity of interventions range from the provision of (un-)structured leisure activities in an (open-)group format to individual psychotherapy. Interventions also vary widely on other characteristics such as the degree of involvement of significant others (e.g., parent), staff qualification and intervention content. This is in line with a tiered approach in bereavement care (Jones et al., 2015) and may be important to serve different needs of mourners (Wimpenny et al., 2007). At the same time, there is still uncertainty about what intervention works best for whom: How much support does the individual child or adolescent need, what programme is best suited to their needs and resources? An important first step to answer these urgent questions is to quantify the overall efficacy of bereavement interventions for children and adolescents.

Two previous meta-analyses have examined the effects of interventions for bereaved children and adolescents (Currier et al., 2007; Rosner et al., 2010). The first included 13 randomized and non-randomized controlled studies and combined preventive and therapeutic interventions. It calculated 75 effect sizes for the different outcomes indicating adjustment to bereavement across the 13 studies and reported a non-significant overall effect size of 0.14 (95 % CI [0.00; 0.28]) based on the averaged outcome measures per study. Importantly, post-hoc analyses suggested that studies that excluded distressed children or did not apply selection criteria based on the children's pre-treatment functioning had poorer outcomes. Additionally, longer time since the loss was associated with less favorable outcome (Currier et al., 2007). The second meta-analysis included 27 uncontrolled and controlled studies. Analysed separately, uncontrolled studies yielded a significant overall effect size of 0.49, controlled studies of 0.35 (95 % CI [0.15; 0.57]). Again, the overall effect size was based on the average effect size for all different outcomes per study. A subsequent analysis compared studies with participants who showed some level of distress, impairment or clinical diagnosis (categorized as *psychotherapy*) to studies with non-symptomatic participants (*prevention*). This moderator effect was significant for uncontrolled studies, demonstrating better treatment effects for psychotherapy; in controlled studies, only a statistical trend in the same direction emerged. Additionally, in uncontrolled studies, longer treatment duration, a time since loss of >12 months and confrontational elements in treatment yielded significantly higher effect sizes (Rosner et al., 2010). Thus, there is preliminary evidence that psychosocial interventions addressing bereavement in children and adolescents may have a positive effect and that this effect is more pronounced when targeting participants who experience more grief-related distress. At the same time, controlling for the natural course of grief attenuates this effect substantially, potentially to the point of non-significance.

Apart from these meta-analyses, systematic reviews have summarized interventions for subgroups of grieving children or specific intervention types. With their specific focus, these reviews acknowledge that the circumstances of the loss (e.g., the age and developmental stage of the child or the bereavement-related changes in the caregiving environment (Alvis et al., 2023)) can lead to very different bereavement experiences and support needs. For parentally bereaved children, a review reported small negative to large positive effects of interventions, depending on the outcome under consideration (Bergman et al., 2017): Large effects were observed for children's traumatic grief and parent's feelings of being supported. For children mourning the death of a sibling, a review reported preliminary positive effects (Ridley and Frache, 2020). Finally, a review combining the two subgroups (death of a parent or a sibling) reported mixed results for the respective interventions. Importantly, this review stresses the fact that only few studies actually measure symptoms of grief (de López et al., 2020). Other reviews have focused on specific age groups (e.g., pre-school aged children (Chen and Panebianco, 2018)) or specific types of psychosocial interventions for bereavement (e.g., grief camps (Clute and Kobayashi, 2013)). Both reviews found some evidence for the effectiveness of interventions, but regard their findings as tentative due to a limited number and low quality of primary studies, which often lack statistical power.

Whereas specific systematic reviews provide important insights, they cannot quantify the effects of psychosocial interventions for bereaved children and adolescents. Furthermore, the respective reviews did not examine systematically the differences between preventive and therapy interventions, even though this distinction seems to be relevant (Currier et al., 2007; Rosner et al., 2010). Meta-analyses can quantify the overall effects; however, the last systematic searches were conducted in 2006 (Currier et al., 2007; Rosner et al., 2010). This underlines the need for an updated meta-analysis. Additionally, all previous meta-analyses based their effect sizes on the averaged effect size for a respective study across all outcomes, thus combining very different indicators of increasing adjustment (e.g., satisfaction with life) and decreasing pathology (e.g., posttraumatic stress). However, to assess the efficacy of grief interventions, there is a clear need to distinguish their efficacy on grief symptoms (primary outcome) from that on other secondary outcomes (e.g., symptoms of depression and PTSD).

Thus, we conducted a systematic review and meta-analysis to examine the efficacy of psychosocial interventions (i.e., prevention or therapy) in bereaved children and adolescents for our primary outcome (grief) and secondary outcomes (PTSD, depression). Depending on the number of available studies, we also aimed to investigate whether intervention or client characteristics moderate intervention efficacy.

2. Method

2.1. Preregistration and reporting

The review was preregistered with PROSPERO (CRD42020202003) and is reported following the PRISMA guidelines (Page et al., 2021).

2.2. Search strategy

We conducted a literature search (last update April 2023) in PsycINFO, PsycARTICLES, PubMed, MEDLINE, PSYNDEX, Web of Science, CINAHL and ERIC. Guided by PICO terms (Oxman et al., 1993) and informed by previous grief-specific meta-analyses (Johannsen et al., 2019; Wittouck et al., 2011), the search string for all databases was *(grief OR griev* OR bereave* OR mourning) AND (child* OR adolescent*) AND (intervention OR counselling OR counseling OR treatment OR therapy OR psychotherapy OR support) AND (effect* OR efficacy OR benefit* OR evidence*) NOT (pregnancy OR pregnant OR prenatal OR perinatal)*. For studies that met eligibility criteria, we performed a snowballing procedure to search for additional relevant publications in their reference lists. Additionally, we screened the reference lists of previous meta-analyses.

2.3. Inclusion and exclusion criteria

We included studies with (1) a psychosocial intervention for bereaved persons (e.g., cognitive-behavioral, emotional-supportive and/or psychoeducation) that was delivered in a professional context. There were no restrictions on delivery format, setting and type, and no restrictions concerning time since loss. However, interventions were only eligible if they (2) started after the loss. Study participants had to be (3) children and/or adolescents with a maximum age of 18 years (no lower limit specified), who were (4) bereaved of a live-born person. Further criteria were that the study was (5) quantitative, included (6) an evaluation of the intervention, and (7) pre-post measurement of the study's primary outcome. We did not set restrictions on particular instruments, but required a measurement facilitated via a validated instrument for our primary outcome (i.e., no ad-hoc devised instruments). Finally, (8) report language had to be English or German. We excluded

(1) systematic reviews, (2) exclusively qualitative studies, (3) case studies, (4) opinion papers or editorials and (5) study protocols. We predefined self-reported grief as primary outcome and caregiver-reported grief, and symptoms of PTSD and depression as secondary outcomes. Thus, we excluded studies that (6) did not report any of our primary or secondary outcomes.

2.4. Study selection and data extraction

After removal of duplicates, two researchers screened independently the titles and abstracts of the studies for eligibility using the software Covidence. One was mandatorily one of the two first authors; the second person was either the other first author or a research assistant who had received training for the screening procedure. The remaining studies were screened full-text by both first authors independently. Disagreements were solved through discussion; if necessary, a third researcher (BKD) was consulted. The PRISMA flowchart was generated using the tool by Haddaway et al. (2022). For eligible studies, the following data were extracted independently by the first authors: study characteristics (authors, year of publication, title, country), sample characteristics (sample size, mean age, gender, description of population, mean time since loss, relation to the deceased, cause of death), interventions characteristics (prevention vs therapy, content, number of sessions, duration, setting, qualification of the person providing the intervention, type of control group, attrition), outcome descriptions (instrument, time to post measurement, time to follow-up measurement) and means and standard deviations of outcome measures for grief, symptoms of depression, and symptoms of PTSD. Interventions were classified as preventive interventions vs therapy (Rosner et al., 2010; Wittouck et al., 2011): an intervention was coded as therapy, if the respective study defined a symptom criterion in their inclusion criteria (e.g., scoring above a predefined cut-off on a grief questionnaire) and specified therapeutic techniques (e.g., exposure, cognitive restructuring).

2.5. Quality assessment

Following the approach of Johannsen et al. (2019), we assessed the quality of all included studies via a modified JADAD scale (Jadad et al., 1996). The respective domains were: (1) randomization (one point, if randomization was mentioned; two points, if the method was appropriate; if the method of randomization was inappropriate, 1 point was deducted); (2) blinding (one point, if blinding was mentioned; two points, if blinding was appropriate, i.e. blinded rating of symptoms); (3) withdrawals and dropouts (1 point, if the participant flow was described appropriately); (4) primary outcome (1 point, if stated a priori); (5) statistical power analysis (1 point, if conducted); (6) participants were included based on symptom levels (1 point, if reported). Since a further item from Johannsen and colleagues regarding the usage of specific questionnaires for pathological grief was deemed inappropriate - as no comparably established measures for grief in children and adolescents exist - we assigned (7) one point if a study used a validated measure for self-reported dysfunctional grief. Instruments were classified in accordance with Zhang et al. (2023). Controlled studies could therefore achieve a score of 0–9 points. Uncontrolled studies could achieve 0–5 points, as randomization and blinding could not be fulfilled in this case.

2.6. Data analysis

Using Comprehensive Meta-Analysis, separate analyses were conducted for the primary outcome and the secondary outcomes, as well as for preventive and therapy interventions and for the time points of measurement. Since only five studies reported intent-to-treat analyses, calculations were based on data from study completers. We used Hedges' *g* as effect size (ES) for all studies, representing standardized mean differences corrected for possible small sample bias (Borenstein et al., 2021). Pooled ESs were calculated using a random-effects model. ESs of 0.2 are considered as small, 0.5 as medium and 0.8 as large effects (Cohen, 1988).

If studies were controlled, the ES was the standardized mean difference of treatment and control group at post-test or follow-up, respectively. If studies were uncontrolled, the ES was the standardized mean difference between post-test and pre-test or between follow-up and pre-test. Study ESs were calculated via group means and standard deviations. If these were not reported, ESs were estimated using other statistical ratios, e.g., *F*-values, *t*-values or *p*-values. If data were not sufficiently reported, we contacted the study authors. If this was not successful, we excluded the study. If studies reported more than one measure for a specific outcome, we chose validated over ad-hoc measures and dysfunctional grief over normal grief measures. If a study reported results for several treatment groups or for specific subgroups (e.g., male vs. female) or subscales, we aggregated the outcomes averaging single ESs. To account for potential outliers, we conducted sensitivity analyses.

Heterogeneity was explored using the Q and I^2 statistics, with I^2 values of 25 % considered as low heterogeneity, 50 % medium and 75 % high heterogeneity (Higgins et al., 2003). If a significant Q-test or I^2 indicated heterogeneity in our primary outcome, moderator analyses were planned as subgroup analyses for categorical data (e.g., group vs. individual setting, comorbidities, type of loss), provided that more than three studies reported the respective data. Meta-regression was planned for metric data (e.g., age, time since loss, study quality, number of sessions) if at least ten studies reported sufficient data (Deeks et al., 2019).

Publication bias was assessed using funnel plots (Light and Pillemer, 1984), if the pooled ES comprised at least five studies (Rothstein et al., 2005). If the funnel plots indicated a possible publication bias, we additionally report the respective adjusted ES using the trim and fill method (Duval and Tweedie, 2000). As a low number of included studies negatively affects the power of Egger's test (Egger et al., 1997) it was only calculated, if a pooled ES comprised >10 studies (Sterne et al., 2011).

3. Results

The study selection procedure is shown in Fig. 1. The literature search in electronic databases identified 6398 studies. After removal of duplicates, abstracts and titles of 3939 records were screened, with 3785 records excluded and 5 reports not retrievable. The remaining 149 reports were assessed for eligibility in full-text. Of these, 116 were excluded (see Fig. 1 for reasons of exclusion). Cross-referencing (snowballing) and cross-checking with previous meta-analyses identified an additional nine studies. Data from 39 reports were extracted independently, with an interrater reliability of 91.2 %.

If studies reported on samples with varying losses or trauma (Carver, 1999; Saltzman et al., 2001; Unterhitzenberger et al., 2020), we extracted only the data of the subsample of participants who had experienced the death of a loved one. In one study investigating a two-phase therapeutic approach (Hill et al., 2019), baseline data were reported insufficiently for phase II; thus, only data for phase I were extracted. Due to insufficient reporting for the separate experimental groups, we treated the study by Tonkins and Lambert (1996) as uncontrolled and calculated a pooled ES for the experimental group and delayed intervention group. The study by McClatchey et al. (2009) reports a control group only for pre- to post-assessment, as the control group received an intervention after post-assessment. Thus, we included the data from both groups in the pooled pre-post ES for controlled studies, but only included data from the uncontrolled intervention group in our pre-follow-up analysis. Two studies (Adams, 1994; Linder et al., 2022) appeared to meet inclusion criteria; however, as they did not use validated instruments to assess grief, they were not included in the meta-analysis. Effect sizes were computed for self-reported symptoms of grief ($k = 22$), symptoms of PTSD ($k = 19$) and symptoms of depression ($k = 25$). Caregiver-reported symptoms of grief were not analysed due to a

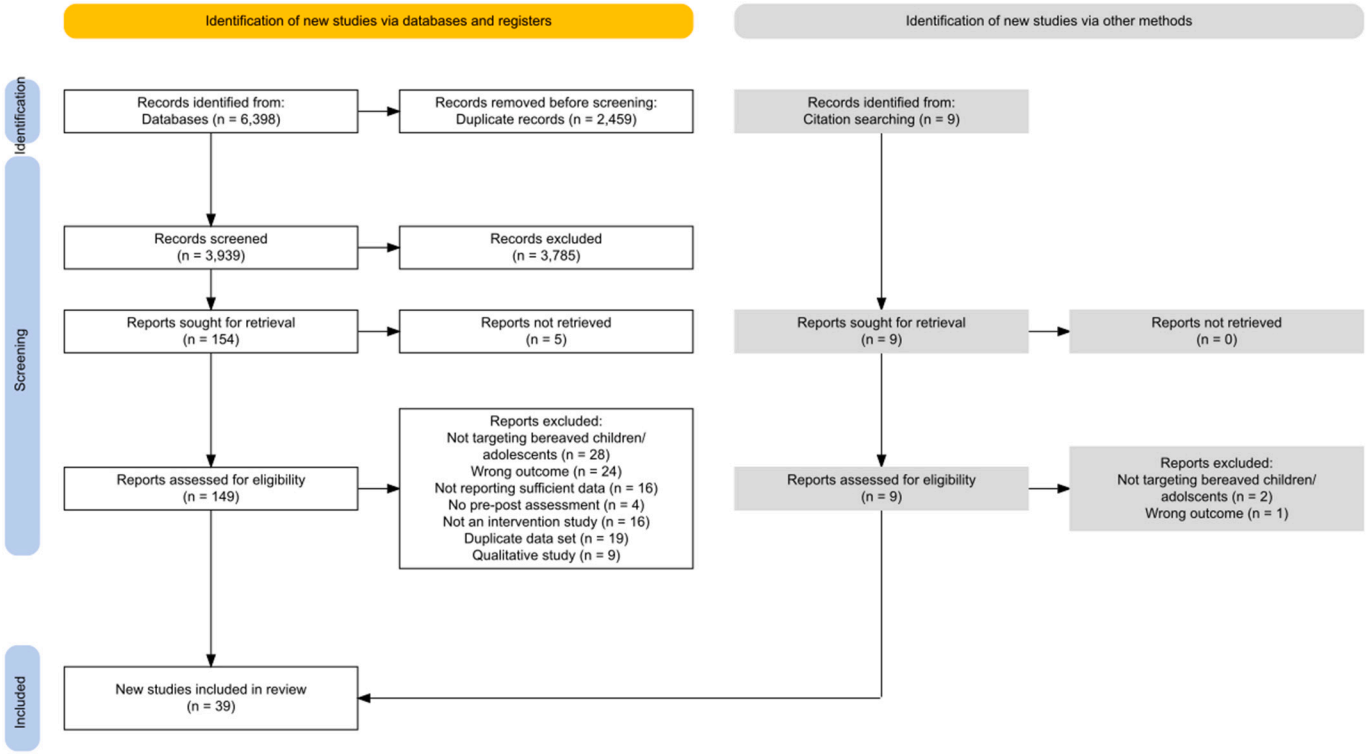

Fig. 1. PRISMA flowchart of article selection.

low number of studies reporting this outcome.

3.1. Study and sample characteristics

Of the 39 studies, 15 described a controlled design (Appendix A), whereas 24 were uncontrolled (Appendix B). Most studies ($k = 27$) were identified as prevention studies; the remaining 12 studies included participants based on a symptom criterion and were classified as therapy studies.

A total of 5578 children and adolescents participated in the studies. The studies' median sample size was 40 (range: 6–1689 participants). Only 29 studies reported the mean age of the participants (12.1 (±2.1) years, range: 8–17). The median proportion of female participants was 52.5 % ($k = 35$). Most studies were conducted in western countries ($k = 28$; US, Netherlands, Canada, Australia, Germany). Only 21 studies reported the mean time since loss, which ranged between 4 and 157.6 months with a median of 16.3 months. Of the studies specifying the relationship to the deceased ($k = 31$), 15 reported varying relations to the deceased. Those focusing on uniform relationships to the deceased most frequently investigated parental bereavement (87.5 %). Cause of death was specified by 25 studies and 28 % of these focused exclusively on sudden bereavement (i.e., accident, homicide, suicide). Interventions varied widely. While some studies provided detailed descriptions of manualized evidence-based treatments, e.g., based on Trauma-focused Cognitive Behavioral Therapy (TF-CBT), others described interventions such as a one-weekend grief camp with different activities. Accordingly, the number of sessions and duration of intervention varied greatly between 1 and 21 sessions (median 12, $k = 29$). Interventions were mostly delivered as group sessions ($k = 28$), while eight studies used an individual and three a mixed setting. Occupational background of the person delivering the intervention ranged from licensed therapists to lay counselors. Most studies ($k = 37$) reported the time between baseline and post measurement (median 8.0 weeks), whereas only 26 studies reported the attrition rate at post measurement (median 9.3 %, range 0–84 %). Few studies reported a follow-up measurement ($k = 11$), conducted after a median of 17 weeks (range: 4–312). Most of the controlled studies used a waitlist or no treatment control group ($k = 11$), three studies used an active control group and one study compared the intervention to usual care.

3.2. Study quality

Study quality was assessed separately for controlled (Appendix C) and uncontrolled studies (Appendix D). Controlled studies had a mean quality rating of 5.4 points (range: 2–9). A total of 20.0 % of studies fell in the range of low quality (0–3), 60.0 % of studies had a moderate quality (4–7). Only three studies demonstrated a high study quality (> 7 points). For uncontrolled studies, a modified version of the quality scale was used (cf. method section), with a maximum value of five points. Uncontrolled studies had a mean quality score of 2.5 (range: 1–4).

3.3. Meta-analysis: preventive studies

3.3.1. Uncontrolled studies

Uncontrolled preventive studies showed a significant pre-post effect size of $g = 0.21$, 95%CI [0.01, 0.42], $p = .048$ for symptoms of grief ($k = 7$). Heterogeneity was high ($Q = 25.64$, $p < .001$, $I^2 = 76.60$) and sensitivity analysis identified one statistical outlier (Greenwald et al., 2017). Excluding this study resulted in a significant ES ($g = 0.29$, 95 % CI [0.09; 0.48], $p = .004$; see Fig. 2). Heterogeneity was reduced, but remained at a high level ($Q = 17.44$, $p = .004$, $I^2 = 71.33$). The funnel plot indicated a small risk of bias (Appendix F). Using the trim and fill method resulted in an adjusted ES of $g = 0.19$, 95 % CI [−0.01; 0.38]. Since only three studies reported effect sizes at follow-up, no pooled ES was computed. The follow-up effect sizes were $g = 0.55$, 95 % CI [0.20; 0.90], $p = .002$ (Malone, 2010); $g = 1.03$, 95 % CI [0.70; 1.37], $p < .001$ (McClatchey et al., 2009) and $g = 1.22$, 95 % CI [0.66; 1.77], $p < .001$ (Griffiths et al., 2022).

Concerning our secondary outcomes, five studies reported data for PTSD and ten studies for depressive symptoms. The ES for PTSD symptoms was significant, $g = 0.42$, 95 % CI [0.06, 0.78], $p = .021$. Heterogeneity was high ($Q = 24.82$, $p < .001$, $I^2 = 83.88$). Sensitivity analysis

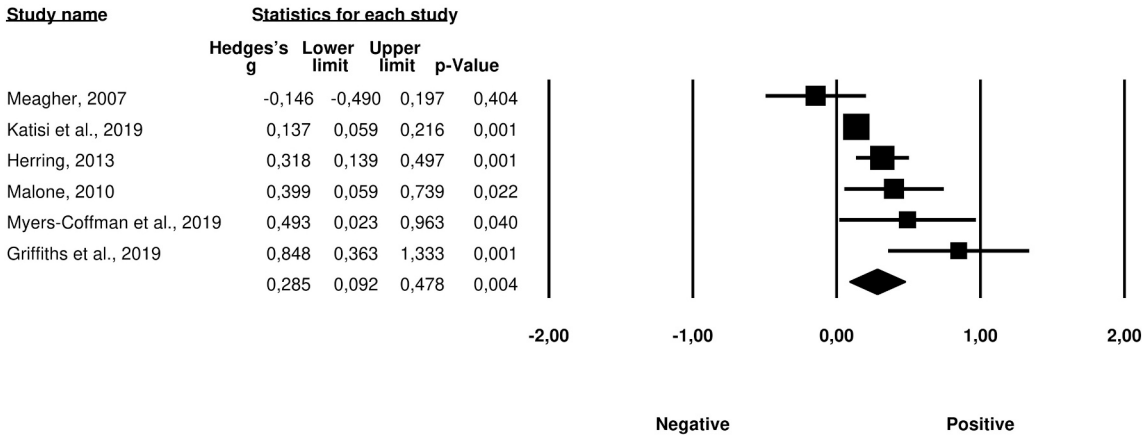

Fig. 2. Forest plot of pre-post effect sizes of uncontrolled preventive studies on symptoms of grief (k = 6).

identified one statistical outlier (Jarero et al., 2008). Excluding this study resulted in a significant ES of $g = 0.24$, 95 % CI [0.11; 0.36], $p < .001$ and reduced heterogeneity ($Q = 1.21$, $p = .752$, $I^2 = 0.00$). For depressive symptoms, the effect was non-significant, $g = 0.96$, 95%CI [−1.03, 2.95], $p = .344$. Sensitivity analysis identified one statistical outlier (Pandya, 2018). Excluding this study, a significant ES of $g = 0.28$, 95 % CI [0.10; 0.45], $p = .002$ was obtained. Heterogeneity was moderate, $Q = 17.77$, $p = .023$, $I^2 = 54.97$. The funnel plot indicated no risk of bias (Appendix F). At follow-up, only two studies reported data for PTSD and no study reported data for depressive symptoms. The forest plots (Appendix E) for pre-post effect sizes of PTSD and depressive symptoms can be obtained from the supplementary material.

3.3.2. Controlled studies

The controlled preventive studies yielded a non-significant pooled ES of $g = 0.01$, 95 % CI [−0.30; 0.31], $p = .960$ for grief symptoms at post measurement ($k = 5$). Excluding one outlier (Unterhitzenberger and Rosner, 2014) resulted in a significant ES of $g = 0.18$, 95 % CI [0.03; 0.32], $p = .019$ (see Fig. 3). Heterogeneity was low, $Q = 0.63$, $p = .889$, $I^2 = 0.00$. Since only two studies reported grief levels at follow-up, no computation of a pooled ES was feasible. The respective follow-up effect sizes were $g = -0.69$, 95 % CI [−1.99; 0.60], $p = .292$ (Brown et al., 2020) and $g = 0.30$, 95 % CI [0.03; 0.56], $p = .030$ (Sandler et al., 2010).

For the secondary outcomes of controlled preventive studies the ES was $g = 0.22$, 95 % CI [−0.04, 0.48], $p = .098$ for PTSD symptoms ($k = 4$) and $g = 0.10$, 95 % CI [−0.26, 0.45], $p = .603$ for depressive symptoms ($k = 7$) at post-treatment. Forest plots can be obtained from Appendix E. At follow-up, only one study reported results for PTSD and depression symptoms, respectively.

3.4. Meta-analysis: therapy studies

3.4.1. Uncontrolled studies

Uncontrolled therapy studies yielded a significant ES of $g = 1.25$, 95 % CI [0.94, 1.57], $p < .001$ on grief symptoms ($k = 7$) at post-measurement, with medium to high heterogeneity ($Q = 21.17$, $p = .002$, $I^2 = 71.66$). Fig. 4 shows the respective effect sizes in the forest plot. As the funnel plot indicated a small risk of bias (Appendix F), the trim and fill method was used and resulted in an adjusted effect size of $g = 1.14$, 95 % CI [0.80; 1.48]. No analysis could be performed for follow-up.

For secondary outcomes, the pooled ES was $g = 1.33$, 95%CI [0.85, 1.82], $p < .001$ ($k = 6$) for symptoms of PTSD. Heterogeneity was high ($Q = 40.52$, $p < .001$, $I^2 = 87.66$). The funnel plot (Appendix F) indicated no risk of bias. The pooled ES for depressive symptoms was $g = 0.61$, 95%CI [0.45, 0.77], $p < .001$ ($k = 5$) and heterogeneity was low ($Q = 1.60$, $p = .808$, $I^2 = 0.00$). Forest plots for both measurements can be obtained from Appendix E. No studies reported sufficient follow-up data for PTSD and depressive symptoms, respectively.

3.4.2. Controlled studies

Only three controlled therapy studies provided data for grief symptoms at post measurement and two studies for data at follow-up, thus precluding the computation of pooled effect sizes. The individual effect sizes for grief symptoms at post measurement and follow-up are displayed in Table 1.

For the secondary outcomes, the pooled ES for PTSD symptoms at post measurement ($k = 4$) was significant, $g = 0.71$, 95%CI [0.15, 1.27], $p = .013$. Heterogeneity was high ($Q = 16.96$, $p = .001$, $I^2 = 82.31$). The

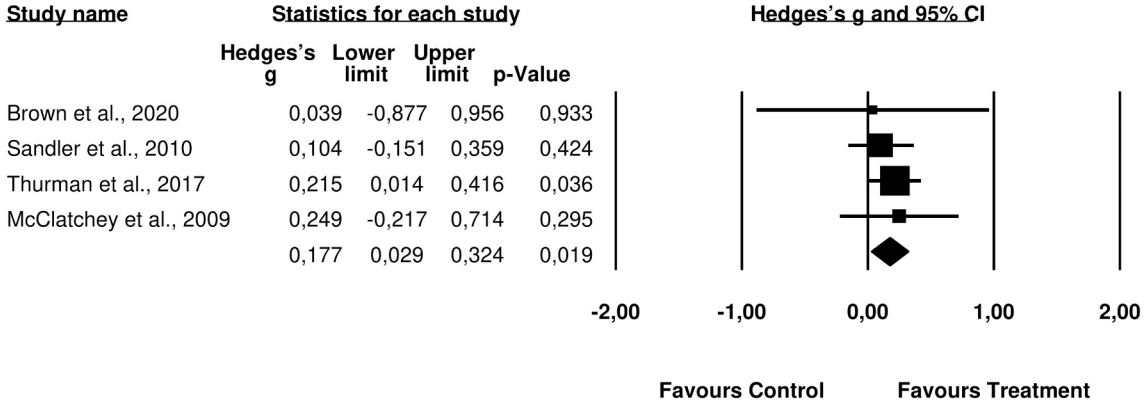

Fig. 3. Forest plot of effect sizes of controlled preventive studies on symptoms of grief (k = 4).

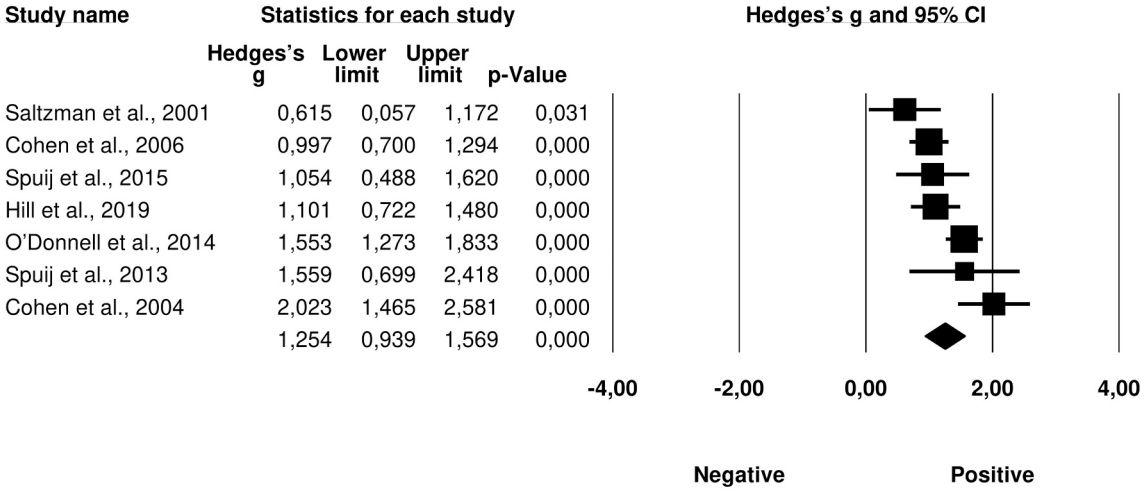

Fig. 4. Forest plot of pre-post effect sizes of uncontrolled therapy studies on symptoms of grief (k = 7).

Table 1
Effect sizes for included controlled therapy studies on symptoms of grief at post-measurement (k = 3) and follow-up (k = 2).

Study	Post-Measurement				Follow-Up			
	Hedges' *g*	95 % LL	95 % UL	*p*-Value	Hedges' *g*	95 % LL	95 % UL	*p*-Value
Boelen et al. (2021)	0.23	−0.13	0.60	.209	0.64	0.15	1.13	.011
Dorsey et al. (2020)	0.31	0.16	0.47	<.001	0.39	0.23	0.54	<.001
Kalantari et al. (2012)	0.66	0.15	1.17	.011				

forest plot can be obtained from Appendix E. Due to a low number of studies reporting respective data, we did not compute pooled effects sizes for symptoms of depression at post measurement ($k = 3$) and follow-up ($k = 2$) or symptoms for PTSD at follow up ($k = 3$).

3.5. Moderator and subgroup analyses

Table 2 provides a summary of the results of the present meta-analysis. Moderator analyses were planned for analyses with sufficient *k*, significant effect size and significant test for heterogeneity. Only two analyses fulfilled these conditions: grief symptoms at post measurement in uncontrolled preventive and uncontrolled therapy studies. However, the small number of studies included in both analyses ($k < 10$) did not allow performing meta-regression (Deeks et al., 2019) and subgroup analyses for categorical predictors were not feasible due to a highly uneven distribution of studies across the subgroups (Richardson et al., 2019).

4. Discussion

The purpose of the present review and meta-analysis was to quantify the efficacy of psychosocial interventions for bereaved children and adolescents. We classified studies as preventive interventions, if they did not specify elevated levels of distress as inclusion criterion and/or did not specify therapeutic techniques. Evidence for preventive interventions is mixed. In uncontrolled studies, adjusting for publication bias reduced the positive effect on grief symptoms and led to a non-significant result. However, controlled studies resulted in a significant, though small effect. Concerning symptoms of depression and PTSD, results demonstrated a positive small effect for preventive interventions only in uncontrolled studies. Turning to therapy studies, our results show a large effect on grief symptoms at post-treatment in uncontrolled studies even after adjusting for possible publication bias. Due to the low number of controlled studies, no effect sizes could be computed; however, the findings of the respective single controlled studies align with a positive trend. Concerning symptoms of PTSD, significant effects were

Table 2
Summary of effect sizes by intervention and outcome at post-measurement.

Intervention	Design	Outcome	Post-Measurement				
			k	Hedges' *g*	95 % LL	95 % UL	*p*-Value
Prevention	Uncontrolled	Grief	6	0.29†	0.09	0.48	.004
		PTSD	4	0.24	0.11	0.45	.002
		Depression	9	0.28	0.10	0.45	.002
	Controlled	Grief	4	0.18	0.03	0.32	.019
		PTSD	4	0.22	−0.04	0.48	.098
		Depression	7	0.10	−0.26	0.45	.603
Therapy	Uncontrolled	Grief	7	1.24‡	0.94	1.57	<.001
		PTSD	6	1.33	0.85	1.82	<.001
		Depression	5	0.61	0.45	0.77	<.001
	Controlled	Grief	3	n.a.			
		PTSD	4	0.71	0.15	1.27	.013
		Depression	3	n.a.			

Note: † after trim- and fill-adjustment: $g = 0.19$, 95 % CI [−0.01; 0.38]; ‡ after trim- and fill-adjustment: $g = 1.14$, 95 % CI [0.80; 1.48]; n.a.: not applicable.

observed in both uncontrolled studies (large effect) and controlled studies (moderate effect). Uncontrolled studies also demonstrated a moderate effect for depressive symptoms, but due to the small number of studies, no controlled effect size could be computed. The positive results should be considered preliminary, as they rely on a very limited number of studies. Additionally, the stability of all the aforementioned effects remains unclear, since only few studies reported follow-up data. Six out of eight significant meta-analyses demonstrated at least moderate heterogeneity, even after the exclusion of statistical outliers. It would have been highly desirable to assess the reasons for this heterogeneity. However, due to the small number of studies, no moderator analyses could be performed. Thus, we cannot say whether there are specific client, intervention or study characteristics that influence the reported effect sizes.

Based on the very limited evidence, psychosocial interventions for bereaved children and adolescents may reduce grief symptoms. However, for preventive interventions, this effect is small, especially when controlling for the natural course of grief over time. Interventions aimed at children and adolescents with elevated bereavement-related distress demonstrate larger effects. Compared to previous meta-analyses of psychosocial interventions in bereaved children and adolescents (Currier et al., 2007; Rosner et al., 2010), we used a different analytical approach to improve the interpretability and quality of the meta-analytical results. We conducted a more fine-grained analysis focusing on symptoms of grief, PTSD, and depression, separately, instead of combining different outcomes. To be included in our analysis on symptoms of grief, studies had to use a validated instrument to assess grief. Additionally, criteria of study inclusion and exclusion differ, e.g., as we included only studies with a pre-post measurement. Nevertheless, important similarities in the results emerge. First, interventions that target more distressed participants are more efficacious. Second, the positive effects observed in uncontrolled studies are substantially reduced when controlling for the natural course of bereavement, but remain significant.

Thus, it seems that preventive interventions can lead to small, but positive effects on grief. However, the investigated programs vary widely. Regarding the treatment format, they ranged from bereavement camps (McClatchey et al., 2009) to weekly group sessions (Thurman et al., 2017) and some, but not all, included a caregiver in the intervention. Differences emerged also with regard to program content. As a common denominator, most programs provided psychoeducation about grief and bereavement. In addition, many programs used interventions based on cognitive behavioral therapy, e.g., confronting experiences of loss and grief, cognitive restructuring or the use of coping skills. Other programs, however, focused more on joint activities and positive group experiences (Hartwig and Marlow, 2022; Nettina, 2005), while still others did not target grief symptoms directly but indirectly through behavior change methods to influence variables on the family-level (e.g., caregiver-child relationship) and child-level (e.g., coping skills) (Sandler et al., 2013). Unfortunately, the present analysis could not identify characteristics of efficacious programs (i.e., moderators). Thus, it remains unclear which approach to bereavement support is most helpful for whom. Additionally, the clinical significance of the reported effect sizes cannot be determined from our findings.

Turning to therapy studies, their efficacy in uncontrolled studies is promising. However, more controlled studies are needed. In our meta-analysis we were able to include only five RCTs investigating bereavement interventions for children and adolescents with elevated symptom levels of PGD or PTSD. Four of the five studies focused on interventions targeting traumatic childhood grief (Ahmadi et al., 2018; Dorsey et al., 2020; Kalantari et al., 2012; Unterhitzenberger et al., 2020). However, previous research has shown that children can experience elevated grief-related distress also after natural deaths (Brown et al., 2007; Melhem et al., 2011) and the new diagnosis of PGD is not restricted to traumatic losses. Thus, investigating treatment approaches that accommodate different bereavement experiences is of high relevance. Only one included RCT (Boelen et al., 2021) focused on PGD independently of the circumstances of the death. Due to its active control group (i.e., supportive counseling) and our approach to calculate effect sizes, the study was accorded only a low and non-significant post-treatment effect size in our analysis. Nevertheless, this CBT-based treatment could be a promising approach, especially since the evaluation also reported favorable results regarding the stability of the treatment effects. Thus, based on the presence of follow-up evaluations and the methodological quality of the respective trials, CBT grief-help (Boelen et al., 2021) and TF-CBT including grief specific elements (Dorsey et al., 2020) may serve as seminal starting points to develop and investigate future treatment approaches.

4.1. Strengths and limitations

The present meta-analysis aggregates the current evidence of the efficacy of psychosocial interventions for bereaved children and adolescents systematically. It is the first comprehensive review to examine the efficacy on grief symptoms, symptoms of PTSD and depression separately. Thus, it provides a more fine-grained analysis that can inform further research and clinical practice.

At the same time, our results must be interpreted with caution due to the following limitations. First, while our inclusion criteria aimed to maximize the interpretability of the obtained results, they also restricted the number of studies that could be included in the meta-analysis. Obviously, the small number of studies in the separate analyses challenges the robustness of the effects and did not allow for the calculation of pooled effect sizes in certain analyses. Additionally, our age criterion excluded some studies that would have been of interest to the present analyses (Layne et al., 2008). Second, methodological shortcomings and the quality of the primary studies limit the interpretability of our findings. Many studies were based on very small sample sizes, reported incomplete data and described the procedure and treatment approach insufficiently. This problem pertained also to the statistical analyses: As most studies reported no intent-to-treat analyses, our calculations are based on completer data. Third, the original studies demonstrated substantial heterogeneity. Studies differed in sample and loss-related characteristics, setting and content of the intervention, qualification of the person providing the intervention and cultural contexts. The limited number of studies precluded moderator analyses that could have quantified the influence of these variables. Study heterogeneity also affects the risk of bias assessment, as the trim and fill method may adjust inappropriately for publication bias in the presence of high heterogeneity (Rothstein et al., 2005). Fourth, the stability of the reported positive effects remains unclear as only few studies reported follow-up data. Lastly, the controlled studies used different control conditions, i.e., waitlist vs. active control group or treatment as usual. Due to the small number of included studies, these were combined in our respective analyses, thus underestimating treatment effects for studies with active control groups.

4.2. Future directions

The present meta-analysis suggests that based on the limited available evidence, psychosocial interventions can support bereaved children and adolescents. At the same time, our results highlight that this field of research must strive to overcome several problems that at present limit our confidence in this statement. Thus, our results should inform future research.

First and foremost, the overall quality of the included studies was moderate. Many trials did not provide essential information concerning their samples (e.g., loss-related characteristics, baseline differences between the intervention groups) and were obviously underpowered. Most studies did not provide any information concerning treatment integrity. Almost none of the trials was pre-registered. All this introduces risk of bias and lowers the confidence in the studies' respective findings. Thus,

the most urgent task for the field is to improve overall trial quality. As a first step, we need more controlled trials, especially for preventive interventions. Uncontrolled trials can serve the important goal to identify potentially helpful interventions. However, an intervention's efficacy must be examined in controlled designs. Grief-related distress is likely to wax and wane with the course of time. Thus, results from uncontrolled studies can hardly be interpreted as evidence for the efficacy of the intervention, but may reflect natural recovery, reversion to the mean effects or therapist attention effects (i.e., placebo effects).

Second, we must strive to improve the assessment of outcomes in psychosocial interventions for bereaved children and adolescents. Many evaluations of bereavement care still rely on self-devised ad-hoc questionnaires (Wilson et al., 2021) and were thus excluded from the present meta-analysis concerning grief symptoms. The 22 studies reporting on grief and included in our review used 12 different questionnaires, with some assessing adaptive grief reactions and others measuring various forms of dysfunctional grief. This clearly illustrates that there is no gold standard in measuring grief for children and adolescents. The most frequently used instruments in the included studies were the Inventory of Prolonged Grief for Children (IPG—C) (Spuij et al., 2012) and the Extended Grief Inventory (EGI) (Dalton and Krout, 2005). Whereas the psychometric properties of the EGI have been criticized (Unterhitzenberger and Rosner, 2016; Zhang et al., 2023), the IPG-C has demonstrated good psychometric properties (Spuij et al., 2012). Future research would benefit from choosing validated outcome measures based on their psychometric properties, age-appropriateness and specificity for (dysfunctional) grief (Zhang et al., 2023). Additionally, interviews that are developmentally informed and map to the current DSM-5-TR and ICD-11 classification criteria of PGD are urgently needed (Kaplow et al., 2018).

Third, studies should specify more clearly their target population. In accordance with previous meta-analyses (Rosner et al., 2010; Wittouck et al., 2011), we classified interventions as preventive if they did not specify therapeutic techniques and/or did not specify elevated levels of distress as inclusion criterion. As a result, at least two studies that used therapeutic interventions (TF-CBT, EMDR) were classified as preventive in our analysis, because they did not specify a symptom threshold (Brown et al., 2020; Jarero et al., 2008). If studies provided more detailed information on their target population (i.e., criteria of inclusion and exclusion), this would greatly increase the interpretability of their findings. Thus, studies should clearly indicate whether they offer the intervention to participants solely based on the fact of having experienced bereavement, based on elevated levels of bereavement-related distress, the presence of risk factors (e.g., traumatic circumstances of the death), or whether they ascertain the presence of a mental disorder (i.e., PGD). Our results suggest that this differentiation contributes to a better understanding of the efficacy of psychosocial interventions. Providing this information could thus improve our understanding and subsequent provision of bereavement care.

Fourth, next to positive effects, potential negative effects of bereavement interventions warrant more attention. More than a decade ago, Schut and Stroebe (2011) stated that one important task in the evaluation of bereavement counseling is to investigate potentially harmful side effects. In the context of psychotherapy, negative effects are known to exist (Linden, 2013) and assessment instruments are under research (Herzog et al., 2019). Qualitative and quantitative studies among adult populations indicate that bereavement counseling may not be helpful for all clients and that clients may even experience negative effects associated with the counseling (Aoun et al., 2018; Gallagher et al., 2005). However, negative effects are rarely assessed and reported in bereavement interventions in general, and even less in bereavement interventions specifically for children and adolescents. Future studies should therefore systematically assess negative effects in the evaluation of bereavement interventions for children and adolescents.

Finally, our meta-analysis demonstrates that there is a dearth of studies investigating the treatment of childhood PGD and stipulates further research. The prevalence of PGD among help-seeking bereaved children and adolescents is substantial (e.g., 12.4 % in (Boelen et al., 2019)) and evidence-based treatments are urgently needed. Yet, only twelve studies included in our meta-analysis were classified as therapy studies compared to 22 studies in the most recent meta-analysis of psychological interventions for grief in adults (Johannsen et al., 2019). Additionally, of the twelve studies classified as therapy in our meta-analysis, most interventions focused on traumatic losses. Only few studies addressed PGD independently of the circumstances of the death. Thus, conclusions about the efficacy of grief-specific psychotherapy after non-traumatic losses remain even more tentative. Additional controlled trials, preferably focusing not only on traumatic losses, are needed in order to broaden our knowledge concerning the provision of evidence-based psychotherapy for bereaved children and adolescents.

5. Conclusion

Based on the limited evidence, psychosocial interventions for bereaved children and adolescents may have positive effects. The stability of the effects remains uncertain due to a lack of follow-up data. The results are encouraging with regard to the helpfulness of supporting bereaved children and adolescents. At the same time, they highlight the need to improve the methodological quality of the studies in order to advance our knowledge about evidence-based bereavement care.

CRediT authorship contribution statement

Christina Hanauer: Data curation, Formal analysis, Investigation, Visualization, Writing – original draft. **Berit Telaar:** Data curation, Formal analysis, Investigation, Visualization, Writing – original draft. **Rita Rosner:** Conceptualization, Funding acquisition, Writing – review & editing. **Bettina K. Doering:** Conceptualization, Funding acquisition, Investigation, Methodology, Supervision, Writing – original draft, Writing – review & editing.

Declaration of competing interest

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Appendix A. Supplementary data

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