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Prevention of mental disorders after exposure to natural hazards: a meta-analysis

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ABSTRACT

Question Mental health complaints are increased in survivors of natural hazards and disaster responders. This meta-analysis assessed the efficacy of psychological and psychosocial interventions for the prevention of mental disorders after exposure to natural hazards.

Study selection and analysis We searched Web of Science, PsycINFO and MEDLINE for peer-reviewed randomised controlled trials evaluating preventive interventions targeting symptoms of post-traumatic stress disorder, depression and anxiety. Trials conducted in both, civilians and disaster responders, were included. Random-effect meta-analyses were conducted to assess the efficacy of interventions relative to active and passive control conditions.

Findings The results from 10 included studies (5068 participants) did not find preventive interventions to be superior compared with active or passive control conditions regarding symptoms of post-traumatic stress disorder (g=0.08 and g=0.05) and depression (g=0.13 and g=0.32, respectively). Effects on anxiety symptoms remain unclear. Aggregated effects for all outcomes were significant at follow-up compared with passive controls, but the interpretability is limited by the low number of studies. Intervention effects were not significantly associated with intervention type (psychotherapy vs psychosocial), age or delivery mode (online vs face-to-face). The risk of bias across studies was high.

Conclusions The current evidence does not allow for any recommendations regarding prevention programmes in the aftermath of natural hazards. A larger body of high-quality research is needed to develop effective and evidence-based preventive interventions for disaster survivors and responders.

Study registration https://osf.io/4es65

WHAT IS ALREADY KNOWN ON THIS TOPIC

⇒ Prevalence of mental disorders is increased in survivors of natural hazards and disaster responders.
⇒ The climate change-induced increase in frequency and intensity of natural hazards requires comprehensive disaster response plans, specifically addressing mental health implications.
⇒ While there is evidence supporting the efficacy of interventions for treating manifest mental disorders following natural hazards, research on the efficacy of preventive interventions remains limited.

WHAT THIS STUDY ADDS

⇒ We found no evidence of the overall efficacy of preventive interventions in reducing post-traumatic stress disorder (PTSD), depression or anxiety symptoms at postintervention compared with passive control conditions.
⇒ There is some indication that preventive interventions may have a positive impact on symptom reduction 2–7.5 months after baseline, but this finding should be interpreted with caution due to the small number of (high-quality) studies included in the analysis.
⇒ Two studies on disaster responders found prevention programmes to effectively reduce symptoms of PTSD, depression and anxiety compared with controls.

HOW THIS STUDY MIGHT AFFECT RESEARCH, PRACTICE OR POLICY

⇒ Our results are a call for action to develop and test more effective interventions to prevent mental disorders in the aftermath of natural hazards. In this context, more high-quality randomised controlled trials are needed.
⇒ More research is needed for low and middle-income countries, considering that these countries are home to most of the world's population and experience a disproportionate burden of natural hazards.
⇒ It is imperative for policymakers to prioritise and expedite the approval of funding for timely prevention research projects in the aftermaths of natural hazards.

BACKGROUND

The frequency and intensity of extreme weather events such as floods, storms or wildfires are on the rise due to human-induced climate change.[1] Natural hazards not only have severe consequences on economies or infrastructure[1] but also have a negative impact on mental health of affected civilians[2] and emergency service personnel working during or in the aftermath of disasters.[3] In light of the severe impairment induced by mental disorders,[4] their likeliness to take a chronic course[5] and high treatment costs, prevention programmes for mental health are crucial in disaster management plans.

Previous research has shown that individuals exposed to natural hazards are at higher risk of developing mental disorders compared with non-exposed individuals.[2] In studies of both adult and youth survivors of natural hazards, it has been

found that more than 20% score above cut-offs for PTSD,[6 7] depression[8] and anxiety.[9] Mental health complaints have also been reported for responders of natural hazards, although to a lower extent.[10] A recent meta-analysis found large treatment effects for manifest mental disorders after exposure to natural hazards,[11] yet, less is known about the efficacy of preventive interventions in this context.

Preventive interventions may target the general population (universal prevention), subgroups with higher risk for developing mental health complaints (selective prevention) or individuals who already present subclinical symptoms (indicated prevention).[12] Successful prevention increases quality of life and functioning of people affected and thus reduces societal costs.[13] Preventive interventions in general have been shown to be effective in reducing the incidence of depression[14] and anxiety,[15] whereas findings on PTSD appear mixed.[16] A systematic review of preventive interventions for responders of humanitarian crises found a positive pre–post effect in most studies.[17] Yet, mental healthcare in the aftermath of natural disasters faces several challenges, including damaged infrastructure, precedence of rescue efforts, a shortage of trained mental health professionals, a large number of affected individuals and ongoing stressors (eg, due to loss of job or house). As one possible solution to address these challenges and to bridge the mental health gap, digital interventions have gained increasing attention in the aftermath of disaster.[18–20] They have the potential to reach many disaster survivors at once, even before mental health workers reach the affected area.[21] Other advantages include their relatively low cost, immediate availability once internet connectivity is restored, scalability and availability in remote areas with damaged infrastructure.[18 19] Previous research found digital preventive interventions to be effective for different mental disorders.[22 23]

OBJECTIVE

This systematic review and meta-analysis summarises and evaluates the existing literature of randomised controlled trials (RCTs) of preventive psychological and psychosocial interventions in natural hazard settings to inform future disaster management plans in the context of increasing extreme weather events. Our research question was: In survivors of natural hazards or first responders (P), what is the efficacy of psychological and psychosocial interventions (I) relative to control conditions (C) for the prevention of mental disorders (O) in randomised controlled trials (S)? As a second aim, potential moderators of efficacy such as delivery mode, age of participants and content of interventions were investigated. We focused on outcomes related to PTSD, depression and anxiety as these complaints are most frequently reported in the aftermath of natural hazards.[24]

METHODS

This meta-analysis was preregistered in the Open Science Framework database (https://osf.io/4es65). The meta-analysis complies with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses guidelines.[25]

Search strategy and eligibility criteria

To assess the completeness of the search process, a validation set of six studies was identified by hand search prior to the database search (Berger *et al*[26]; Catani *et al*[27]; Dhital *et al*[28]; Ruggiero *et al*[29]; Steinmetz *et al*[29]; Wu *et al*.[30]) A systematic literature search was conducted, covering all publications from inceptions to 13 February 2024. We searched the electronic databases Web of Science, PsycINFO and MEDLINE (through EBSCOhost). Search terms included variations of natural hazard-related and mental health-related terms (see online supplemental table 1). In addition to keyword searches, a Medical Subject Headings (MeSH) search was carried out in MEDLINE. The systematic database searches were supplemented by hand-searching reference lists of reviews on similar topics (eg, Winders *et al*[17]) and of included studies. There were no restrictions regarding language of publication or publication year, but studies had to be published in peer-reviewed journals to ensure scientific rigour of included trials.

Details on the study inclusion criteria are outlined in online supplemental table 2. In short, studies had to report on a RCT of a psychological or psychosocial intervention aiming for the prevention of mental disorders after exposure to a natural hazard. At least 70% of participants were required to have experienced the natural hazard or participants were first responders in the aftermath of the hazard. Participants may have reported subclinical symptoms, but no more than 30% of participants met criteria for a full diagnosis at randomisation. This criterion was adapted *post hoc* because many studies did not provide this information: if no information on diagnosis rate was reported, studies were included if the mean severity score of the sample was below the respective cut-off on a self-report scale. It was also specified that comorbid mental disorders could be present as long as they did not concern the outcomes of interest in this meta-analysis (PTSD, depression, anxiety), or as long as they were not the target of the intervention. Outcomes had to be related to PTSD, depression or anxiety as rated by participants or clinicians in a validated instrument. All control conditions were eligible.

In a first step, the titles and abstracts of all records were screened against eligibility criteria using Rayyan.[31] In line with established quality criteria for meta-analysis,[32] 10% of hits were screened by two independent reviewers (LW and SV) given the large number of identified records. The inter-rater reliability between the reviewers was assessed using a two-way random effects intraclass correlation coefficient (ICC). The ICC value was 0.98 with an agreement rate of 99.9%, indicating almost perfect agreement. The remaining records were therefore only assessed by one reviewer (LW). In a second step, the same two reviewers independently screened full texts of all studies that appeared eligible after title and abstract screening. Disagreements were resolved by discussion with a third reviewer (LBS). The ICC value for the full-text screening was 0.73 with an agreement rate of 93.3%. Forward and backward searches were performed on the included papers from 29 January 2024 to 2 February 2024, but yielded no further hits. All studies of the validation set were identified in the systematic search.

Data extraction

Data extraction was performed by one reviewer (LW) and double-checked by a second reviewer (SV). Disagreements were resolved by discussion. Details on the study, the natural hazard, the intervention, the sample and the outcome were extracted in a predefined Excel sheet (see online supplemental table 3 for details). Only published information was included. Trials assessing multiple outcomes were included in all respective analyses. The dataset is available on online supplemental file:https://osf.io/58mhd/.[33]

Risk of bias assessment

To assess the risk of bias of included studies, the Cochrane Risk of Bias 2 (RoB 2) tool for randomised trials was used.[34] For each study, the instrument determines the risk of bias (1) arising from

the randomisation process, (2) due to deviations from intended interventions, (3) due to missing outcome data, (4) in measurement of the outcomes and (5) in selection of the reported results. The additional considerations for cluster RCTs for the RoB 2 tool[35] were taken into account for one included cluster RCT.[28] Risk of bias judgements from individual domains were combined to report an overall risk of bias rating for each study. Two independent reviewers (LW and SV) applied the RoB 2. Disagreements between the two reviewers were resolved by discussion with a third researcher (LBS).

Statistical analysis

We calculated between-group effect sizes (Hedges' *g*) across studies for both postintervention as well as follow-up assessments, if applicable. Because we anticipated high heterogeneity between studies, we used random effects models, employing the inverse-variance method to obtain pooled effect sizes. The restricted maximum likelihood estimator was used to estimate the between-study variance. Heterogeneity between studies was furthermore assessed using Higgins' I^2 and by calculating 95% prediction intervals (PI). Outliers were excluded if their 95% CI did not overlap with the 95% CI of the pooled effect. If outliers were identified in the main analysis of an outcome, they were also excluded from subsequent subgroup analyses. Analyses were conducted divided by outcome and control group (passive vs active control). One study[36] included two types of passive control groups. The effects were aggregated before conducting the meta-analyses to prevent dependency of data. Sources of heterogeneity were investigated by calculating subgroup analyses for intervention type (psychotherapy vs psychosocial support), age (adult vs youth samples) and delivery mode (online vs face-to-face). The presence of publication bias could not be assessed as no meta-analysis included the minimum required number of 10 studies.[37] All analyses were conducted using the metafor package in RStudio.[38]

FINDINGS

Figure 1 shows the study selection process. A total of 24 994 unique records were identified in the systematic database searches and screened for eligibility. Out of these, k=10 studies met the eligibility criteria and were included in the meta-analysis. A list of all excluded studies on full-text level with reasons for exclusion is documented in online supplemental table 4.

Study and participant characteristics

The 10 included trials reported on a total of 5068 independent participants with a mean age of 21.8 years. Seven studies included adult samples (≥18 years), and three studies included youth samples (<18 years). 67.80% of the participants identified as female. Most studies were conducted in the US (k=4), the remaining studies were conducted in Canada, China, Nepal, New Zealand, Sri Lanka and Turkey. According to the World Bank classification,[39] six studies were conducted in high-income countries and four studies were conducted in low and middle-income countries. The majority of studies were conducted in the aftermaths of earthquakes (k=4) or hurricanes (k=3), the other reported natural hazards were a tornado, a tsunami and a wildfire. 98.6% of the included participants were directly exposed to the disaster, that is, most of them were present when the disaster occurred or had direct contact with the consequences of the disaster as disaster responders. The samples of eight studies were recruited among survivors of natural disasters,[26–29 36 40–42] the remaining studies were conducted among military rescuers[30]

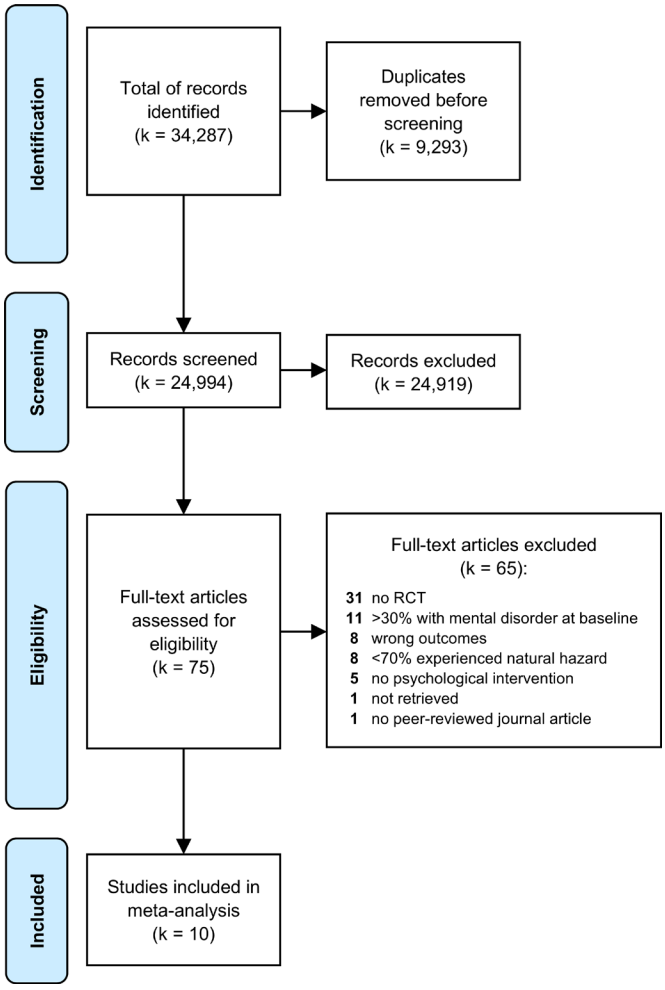

Figure 1 PRISMA flowchart illustrating the study selection process. PRISMA, Preferred Reporting Items for Systematic Reviews and Meta-Analysis.

and (mainly voluntary) disaster responders.[43] Three studies applied indicated prevention, that is, they required the presence of (sub-)clinical symptoms as inclusion criterion for participants.[27 40 41] The time between onset of the natural hazard and baseline assessment was on average 52.0 weeks (SD=48.5; range 3–157). Outcomes were reported by means of clinical interviews (k=4) and self-reports (k=7). Eight studies included a passive control condition, while five studies included an active control condition. It is noteworthy that the active control conditions in three studies comprised the same intervention with different modules or in a different delivery mode. An overview of study characteristics is provided in table 1 (see online supplemental table 5 for more details).

Intervention characteristics

Most of the included studies (k=8) evaluated interventions with psychotherapeutic content,[26 27 29 36 40–43] mainly based on cognitive–behavioural therapy. Content included psychoeducation (about trauma, coping strategies, social support or emotions), relaxation or mindfulness exercises, symptom management and exposure (eg, in sensu or narrative). The remaining two studies applied psychosocial support,[28 30] including a teacher training for daily classroom activities and critical incident stress debriefing and cohesion training. Five interventions were delivered in a group format and five interventions included individual

Table 1 Study characteristics

Study	Total n	Natural disaster (specification; country)	Time since disaster (weeks)	Intervention name	N° sessions (duration in min)	Delivery mode	Comparison	Outcomes (instrument)
Belleville *et al*[40]	136	Wildfire (Fort McMurray; Canada)	104	RESILIENT platform	12 (30)	Online (guided)	Passive: waitlist	PTSD (PCL-5), Depr. (PHQ-9), Anx. (GAD-7)
Berger *et al*[26]	63	Earthquake (Christchurch; New Zealand)	48	ERASE-Stress	3 (400)	Face-to-face	Active: METI	PTSD (PCL-C)
Catani *et al*[27]	31	Tsunami (Indian Ocean; Sri Lanka)	3	KIDNET	6 (60-90)	Face-to-face	Active: MED-RELAX	PTSD (UPID)
Dhital *et al*[28]	1220	Earthquake (Gorkha; Nepal)	71	n.r.	8 (60-120)	Face-to-face	Passive: no intervention	PTSD (CPSS), Depr. (DSRS)
İme[42]	83	Earthquake (Kahramanmaraş; Turkey)	7	Cognitive Behaviour Therapy-Based Group Psychological Counselling Program	8 (75)	Online (guided)	1. Active: same intervention in face-to-face 2. Passive: waitlist	Depr. (DASS-21), Anx. (DASS-21)
Mahaffey *et al*[43]	167	Hurricane (Sandy; USA)	157	Disaster Worker Resilience Training (DWRT) Program	1 (240)	Face-to-face	Passive: waitlist	PTSD (PCL-5), Depr. (PHQ-9)
Paquin *et al*[36]	1058	Hurricane (Harvey; USA)	40	n.r.	4 (15)	Online (unguided)	1. Passive: neutral writing 2. Passive: no intervention	PTSD (IES-R), Depr. (IDAS)
Ruggiero *et al*[29]	987	Tornado (Alabama/Joplin; USA)	38	Bounce Back Now	4 modules	Online (unguided)	1. Active: BBN+ 2. Passive: no intervention	PTSD (NSA-PTSD), Depr. (NSA-Depression)
Steinmetz *et al*[41]	53	Hurricane (Ike; USA)	48	My Disaster Recovery website	6 modules	Online (unguided)	1. Active: TAU 2. Passive: information only	PTSD (MPSS), Depr. (CES-D)
Wu *et al*[30]	1267	Earthquake (Wenchuan; China)	4	512 Psychological Intervention Model (512 PIM)	1 (120)	Face-to-face	1. Active: debriefing only 2. Passive: no intervention	PTSD (SI-PTSD), Depr. (HADS-D), Anx. (HADS-A)

Anx, anxiety; BBN+, Bounce Back Now + modules for parents' mental health; CBT, cognitive behaviour therapy; CES-D, Center for Epidemiologic Studies Depression Scale; CPSS, Child PTSD Symptom Scale; DASS-21, Depression Anxiety Stress Scale; Depr, depression; DSRS, Depression Self-Rating Scale; GAD-7, Patient Health Questionnaire Generalised Anxiety Disorder Scale; HADS-A/D, Hospital Anxiety and Depression Scale—Anxiety/Depression subscale; IASC, Inter-Agency Standing Committee; IDAS, Inventory of Depression and Anxiety Scales; IES-R, Impact of Event Scale-Revised; KIDNET, Narrative Exposure Therapy for Children; MED-RELAX, meditation-relaxation protocol; METI, Managing Emergencies and Traumatic Incidents (organisational programme); MPSS, Modified PTSD Symptoms Scale; n.r., not reported; NSA, National Survey of Adolescents; PCL-5, PTSD Checklist for DSM-5; PCL-C, PTSD Checklist-Civilian version; PHQ-9, Patient Health Questionnaire; PTSD, post-traumatic stress disorder; SI-PTSD, Structured Interview for PTSD; TAU, treatment as usual; UPID, UCLA PTSD Index for DSM-IV.

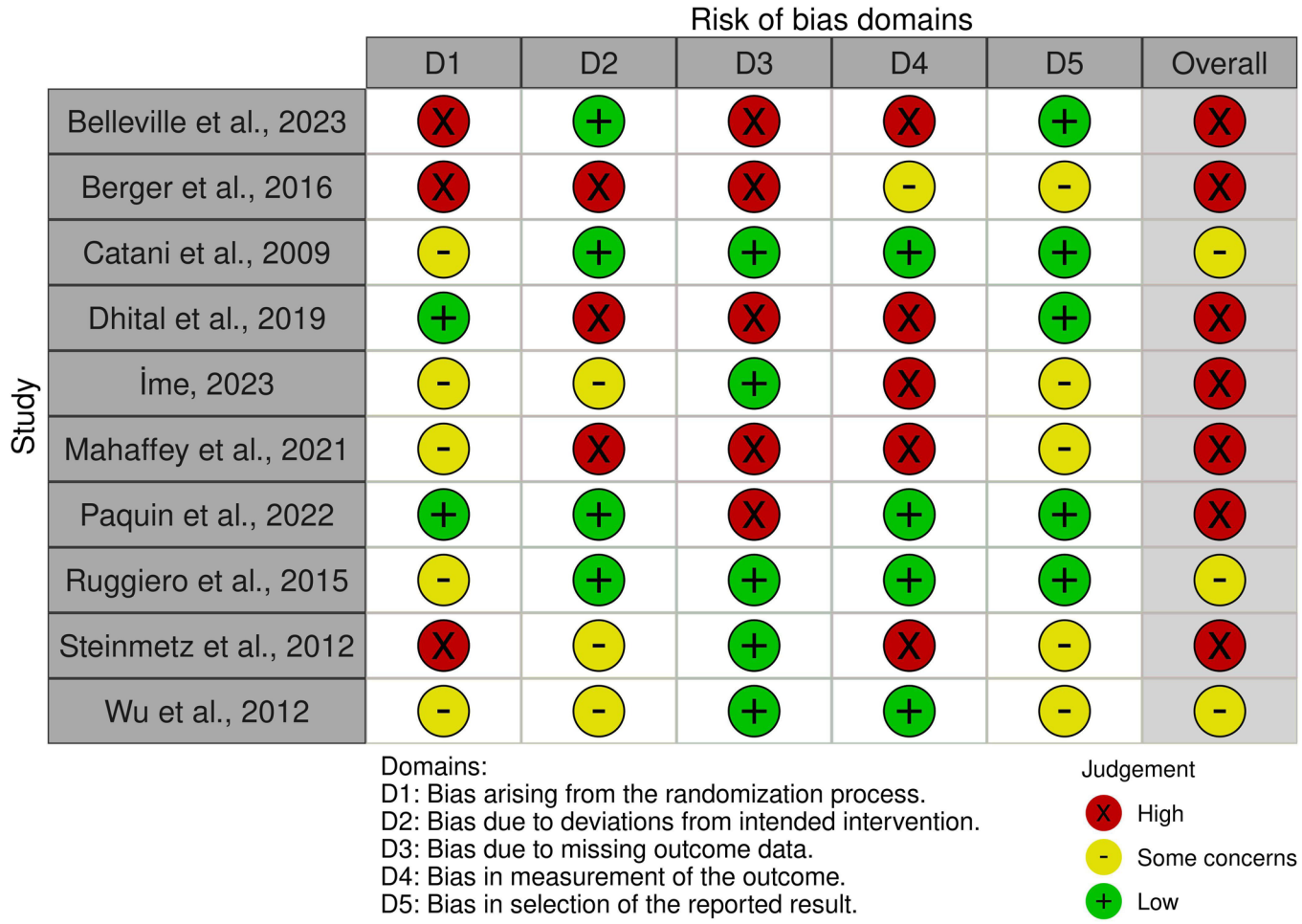

Figure 2 Traffic light plot illustrating the risk of bias ratings for both RCTs and the cluster RCT.[28] RCT, randomised controlled trial.

sessions. The setting of the face-to-face interventions was schools (k=2), refugee camps (k=1) or not specified (k=2). The interventions were carried out on average in 5.3 sessions (SD=3.4; range 1–12), each lasting on average 130.6 min (SD=128.7; range 15–400; some of the interventions were held as whole-day workshops).

Study quality

The overall risk of bias as well as the risk of bias per domain for each study is shown in figure 2. The overall study quality was low. Publications were lacking information in all domains, mostly regarding missing outcome data and measurement of outcomes. Six studies relied on self-report data, whereas four studies analysed data from clinical interviews. Only five conducted intention-to-treat analyses. Only five studies were preregistered, prohibiting a comprehensive assessment of risk of bias due to possible selection of reported results.

Intervention effects on symptoms of PTSD, depression and anxiety

Meta-analyses on intervention effects on PTSD outcome did not yield significant results for passive control groups (g=0.05; 95% CI −0.12 to 0.22; 95% PI −0.37 to 0.47; I^2=82.9%; k=7) nor for active control groups (g=0.08; 95% CI −0.14 to 0.29; 95% PI −0.31 to 0.47; I^2=57.5%; k=5; see figure 3) at postintervention. Statistical heterogeneity was lower for active controls, but still substantial. No statistical outliers were identified. Omitting individual studies from analyses did not significantly change results. At follow-up (on average 30.4 weeks after baseline), a significant effect was found relative to passive controls (g=0.37; 95% CI 0.03 to 0.71; 95% PI −0.19 to 0.93; I^2=85.5%); however, the effect was only based on two studies. No significant effect was furthermore found compared with active controls at follow-up (g=0.39; 95% CI −0.18 to 0.96; 95% PI −0.18 to 0.96; I^2=93.8%, k=4). Only one study[27] compared the PTSD incidence between groups (the prevalence at baseline was 0% in both groups): in the intervention group, the incidence rate was 25% (four children) at postintervention and 18.7% (three children) 6 months later. In the relaxation control group, the incidence rates were 33.3% (five children) and 28.6% (four children), respectively. The differences between the two groups were not significant at either time point.

Preventive interventions did not appear superior in the reduction of depression symptoms compared with passive (g=0.32; 95% CI −0.3 to 0.67; 95% PI −0.66 to 1.30; I^2=96.0%; k=8) and active controls (g=0.13; 95% CI −0.13 to 0.39; 95% PI −0.34 to 0.60; I^2=71.8%; k=4) at postintervention (see figure 3). The study by İme[42] appeared to be an outlier. Removing this study from the dataset resulted in a reduced effect size of g=0.13 (95% CI −0.03 to 0.29) compared with passive controls and a reduced heterogeneity of I^2=80.0%. Omitting the other studies individually did not change the results substantially (g's=0.30–0.38). At follow-up (on average 23.3 weeks after baseline), the preventive interventions appeared superior

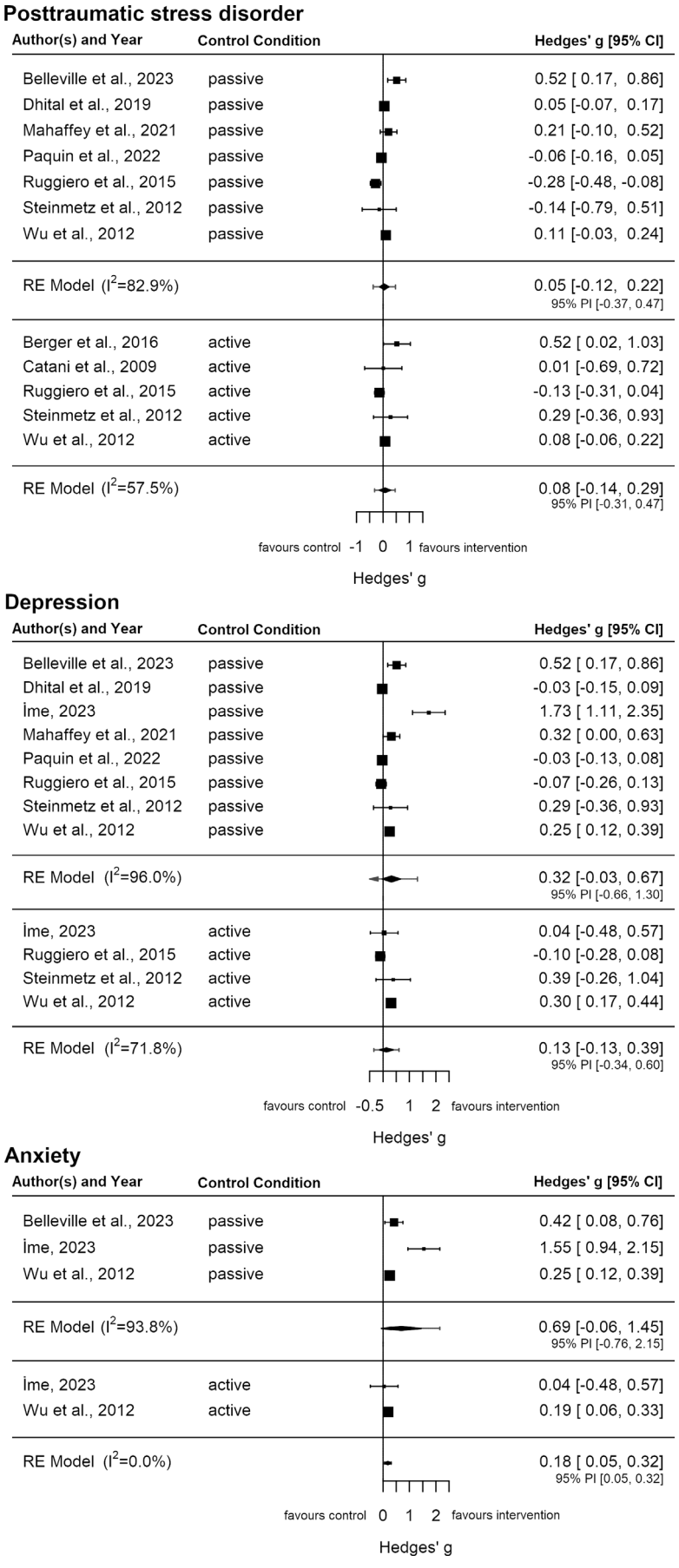

Figure 3 Intervention effects on PTSD, depression and anxiety symptoms versus passive and active controls at post-intervention. PI=prediction interval; PTSD, post-traumatic stress disorder; RE, random effects.

to passive controls in the reduction of depressive symptoms (Hedges' g=0.28; 95% CI 0.16 to 0.40, k=3). Heterogeneity between studies was low (I^2 = 2.9%) and the 95% PI narrow, ranging from g=0.15 to 0.40. Compared with active controls (all comprising of other active interventions), the effect was non-significant (g=0.22; 95% CI −0.10 to 0.53, k=3).

No significant intervention effect compared with passive controls was furthermore found for anxiety outcomes at postintervention, although only three studies reported measures on anxiety (g=0.69; 95% CI −0.06 to 1.45; 95% PI −0.76 to 2.15; I^2=93.8%; k=3; see figure 3). While all three included studies suggested a superiority of the preventive intervention, the aggregated 95% CI covered the null value. The effect was mainly driven by one study.[42] Only two trials compared preventive interventions to active control conditions (g=0.18; 95% CI 0.05 to 0.32; 95% PI 0.05 to 0.32; I^2=0.0%). The effect of preventive interventions at follow-up was significant relative to passive controls (g=0.38; 95% 0.25 to 0.51; 95% PI 0.25 to 0.51; I^2=0.0%; k=2), but not relative to active controls was non-significant (g=0.23; 95% CI −0.08 to 0.54; I^2=42.8%, k=2).

Influence of moderators

Subgroup analyses were restricted to comparisons with passive control conditions. No significant differences were found when comparing online versus face-to-face interventions (PTSD p=0.524; depression p=0.782). The only significant effect was observed for depression symptoms when restricting analyses to adults (g=0.23; 95% 0.02 to 0.43; k=5). Details are found in online supplemental table 6. We did not investigate continuous moderators due to the limited number of included studies. We furthermore did not conduct analyses on the impact of risk of bias on results given the overall limited quality and the extent of missing information in included studies.

DISCUSSION

This is the first meta-analysis to investigate the efficacy of psychological and psychosocial interventions for the prevention of mental disorders after exposure to natural hazards. Aggregated effects did not show a general efficacy of preventive interventions for either PTSD, depression or anxiety. Beneficial effects compared with passive controls may evolve in the long term, however, too few studies were available to draw firm conclusions. The low quality of the included studies furthermore limits the generalisability of findings.

As many people show symptoms of mental disorders months and years after a natural hazard,[9] it is important to provide them with effective mental healthcare to reduce their mental health burden. The existing literature, however, does not allow for any recommendations regarding the optimal timing, content or implementation strategy for preventive interventions after natural hazards. The non-significant findings compared with active controls are likely to be explained by the fact that the majority of included studies applied other interventions as active control conditions (ie, the same interventions with different modules). However, the non-significant effects of preventive interventions relative to waitlist or no intervention are alarming. Only one study[42] found a large intervention effect that appeared to be a statistical outlier in most analyses. The intervention was timely administered one and a half months after the earthquake, however, the study excluded severely affected individuals (ie, those who had lost someone close). The remaining studies predominantly found non-significant smaller or even negative effects. This finding is in contrast to meta-analyses on the overall efficacy of preventive interventions.[14 15] One reason might be that natural hazards disproportionately affect disadvantaged communities and marginalised populations that are more susceptible to distress.[44] It is furthermore noteworthy that even small intervention effects can yield considerable impacts when widely disseminated. On the other hand, both studies that evaluated prevention programmes for natural disaster responders[30 43] found significant effects regarding symptoms of PTSD, depression and anxiety, offering a promising foundation for subsequent studies. Results from one study further suggest that interventions may be particularly effective when delivered prior to a new deployment.[43]

Conclusions are constrained by the paucity of available data. Existing evidence mainly focused on symptom severity and only one study compared incidence rates of mental disorders[27] according to prevention programme. Subgroup analyses revealed a small but significant intervention effect when focusing on adult depression only. Yet, more high-quality research is needed to draw firm conclusions on effective prevention strategies for different settings and receivers. It is essential to reduce the risk of bias in future studies by applying rigorous randomisation procedures and intention-to-treat analyses and to include follow-up data to gain more knowledge on the long-term effectiveness of preventive interventions. Future trials should furthermore explore the efficacy of specific contents and components in preventive interventions. None of the included moderators in our meta-analysis explained the large heterogeneity between studies, but potential influences may have been masked by the limited power of analyses.

According to current guidelines,[45] early interventions to prevent PTSD should be implemented within the first 3 months of a traumatic event. This criterion was only met by two of the included studies (one focusing on children and one on first responders). The lack of studies prohibited subgroup analyses for early preventive interventions, and it is possible that the delay of interventions concealed effects that such interventions could have when distributed earlier. The influence of delivery time as a potential source of heterogeneity should be investigated in future studies. The mean delivery time of the preventive interventions across studies was a year after the natural hazard, which could be significantly accelerated using digital interventions. In light of the necessity to reach affected people as soon as possible,[46] the advantages of digital interventions,[20] and their overall efficacy,[22 23] it appears reasonable to prioritise research in this area.

A limitation of this meta-analysis is the focus on PTSD, depression and anxiety, whereas exposure to natural hazards may also promote the development of additional mental health complaints such as insomnia,[47] suicidality[48] or substance use.[49] Future studies may therefore also include additional outcomes. We included studies based on the absence of current mental health disorder diagnoses irrespective of the elapsed time since exposure to the natural hazard. The extended timeframe until intervention delivery creates the possibility that some interventions may have functioned as tertiary prevention for survivors who did not present with a clinical diagnosis at study entry yet experienced clinically relevant symptoms prior to this point. It furthermore contributed to the extended heterogeneity between studies.

CONCLUSION

Findings from this meta-analysis suggest that survivors do not seem to benefit from mental health prevention programmes in

the aftermath of natural hazards, while two studies found significant effects for disaster responders. Yet, insufficient evidence is available to derive any recommendations regarding prevention strategies. The risk of bias in included studies was high, limiting the generalisability of findings. The findings are a call for action to increase investments in the development of effective prevention programmes for mental health in the aftermath of natural hazards. It is essential to intensify research efforts to adapt effective preventive interventions to the context of natural hazards and to accumulate evidence on requirements for effective mental health prevention in such settings.

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Contributors AK conducted the literature search, final statistical analyses, drafted the manuscript and is the guarantor. SV and LW conducted title and abstract screening, full-text review, data extraction and risk of bias assessment. LW and LS drafted the initial study protocol. LBS designed the study and provided supervision. PC and MD contributed to data interpretation and discussion. All authors reviewed and approved the final manuscript. The corresponding author attests that all listed authors meet authorship criteria and that no others meeting the criteria have been omitted.

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Patient consent for publication Not applicable.

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Provenance and peer review Part of a topic collection; not commissioned; internally peer-reviewed.

Data availability statement Data are available in a public, open access repository. The dataset is available on osf: https://osf.io/58mhd/

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