Abstract

Background

Healthcare workers worldwide are at increased risk of a range of adverse mental health outcomes including post-traumatic stress disorder (PTSD) following the unprecedented demand placed on them from the COVID-19 pandemic. Psychosocial interventions offered to mitigate these risks need to be based on best available evidence. There is limited information for the comparative effectiveness of interventions available.

Method

We undertook a systematic review of psychosocial interventions delivered to healthcare workers before, during and after disasters. We searched eight databases, including the Cochrane Central Register of Controlled Trials, PubMed/MEDLINE, EMBASE, and PsycINFO. Our primary outcomes were changes in symptoms of PTSD, anxiety, depression and sleep. We calculated effect sizes, where unreported, and reliable change indices to appraise intervention effectiveness. We registered our review with [removed for blind review].

Findings

We screened 12,198 papers of which 14 were included. Interventions based on evidence-based protocols, including individual and group-based cognitive-behavioural therapy (CBT) for PTSD, anxiety and depression led to reliable changes in PTSD and anxiety. Single-session debriefing and psychological first aid workshops showed limited efficacy.

Interpretation

There is a limited evidence base for psychosocial interventions for healthcare workers before, during and after disasters, with the strongest evidence base for CBT-based approaches. Future research should include controlled evaluations of interventions and target identified risk factors.

Introduction

The COVID-19 pandemic, caused by the severe acute respiratory syndrome coronavirus 2 (SARS-CoV-2), has placed an unprecedented demand on healthcare workers around the world. Medical, nursing and support staff involved in the crisis face increased workloads, prolonged separation from family and friends, insufficient or inconsistent infection control measures, ethical dilemmas, concern for their own well-being and reduced access to public services and other support structures (Kisely et al., 2020). Elevated levels of fatalities and suffering among patients and colleagues increase the risk for post-traumatic stress responses (Maunder et al., 2003, 2004). This is compounded by the broader psychological impact of quarantine and infection control measures that interfere with existing social support networks and coping strategies (Brooks et al., 2020).

Research on the psychological impact of disaster on healthcare staff has documented a range of adverse mental health outcomes. Frontline staff involved in the 2014 Ebola and 2002 SARS responses identified job stress (Maunder et al., 2004; Wong et al., 2005), fear of contagion (Ho, Kwong-Lo, Mak, & Wong, 2005; Maunder, 2004; Maunder et al., 2004; Wong et al., 2005), concern for family health (Maunder, 2004; Nickell et al., 2004; Wong et al., 2005), interpersonal isolation (Maunder et al., 2004; Wong et al., 2005), quarantine (Bai et al., 2004), perceived stigma (Bai et al., 2004; Maunder et al., 2004; Verma et al., 2004) and caring for healthcare workers as patients and colleagues (Maunder et al., 2003) as main sources of stress. Longitudinal studies of healthcare workers working daily with patients in health crises suggest that the longer-term effects of occupational and personal pressures can include PTSD, depression and suicide (Maunder et al., 2006; Stanley, Hom, & Joiner, 2016; Wild et al., 2016). Emerging evidence from the COVID-19 pandemic suggests that frontline staff worldwide are presenting with acute and post-traumatic stress disorders, depression, anxiety, insomnia and increased risk of suicide (Amerio et al., 2020; Chew et al., 2020; Choudhury et al., 2020; Felice, Di Tanna, Zanus, & Grossi, 2020; Huang, Han, Luo, Ren, & Zhou, 2020; Kang et al., 2020; Kisely et al., 2020; Lai et al., 2020; Pappa et al., 2020; Romero et al., 2020; Tan et

al., 2020; Thakur & Jain, 2020). In addition to the strong moral imperative to prevent unnecessary suffering amongst healthcare workers, there is an important public health argument for mitigating adverse mental health outcomes in healthcare staff, as healthy workers are required to meet the extraordinary demands placed upon them as a result of the pandemic.

Why is this systematic review needed?

Studies documenting the high prevalence of psychological problems among health workers have called for urgent psychosocial support to mitigate this risk (Kisely et al., 2020; Lai et al., 2020). However, there is limited evidence regarding what kind of interventions might be successful in reducing or preventing symptoms of mental health difficulties, or promoting resilience during or after these types of disasters in this occupational group, with prior literature on disaster-responses drawing primarily on military personnel (Iversen et al., 2008; Wessely, 2005). Robust evaluations are urgently needed to inform policy making and avoid the dissemination of interventions that have proven to be ineffective at best and harmful at worst (Van Emmerik, Kamphuis, Hulsbosch, & Emmelkamp, 2002). A recent Position Paper on multidisciplinary research priorities for the COVID-19 pandemic called for immediate research on how frontline health staff can be supported to optimise coping and mitigate symptoms of stress (Holmes et al., 2020). Here, we present a systematic review of psychosocial interventions used before, during and after a disaster to prevent or reduce adverse mental health outcomes in healthcare workers and compare their relative effectiveness. Our aims were (1) to summarise the evidence for the interventions in preventing or reducing adverse mental health outcomes in healthcare staff, in particular, PTSD, anxiety, depression, and sleep difficulties, (2) appraise the effectiveness of the interventions on psychological outcomes by calculating indices of reliable change for primary outcome measures and (3) provide clinical recommendations.

Method

Protocol and registration

We followed PRISMA guidelines (Moher et al., 2009) and pre-registered the systematic review with [removed for blind review]. See Supplementary Material for PRISMA checklist.

Search strategy and selection criteria

Studies were eligible for inclusion if they (a) presented findings from psychosocial interventions designed to prevent or reduce adverse mental health outcomes and/or promote resilience in health care staff; (b) presented results from peer-reviewed research or a doctoral dissertation; and (c) reported mental health outcomes. If studies included health care staff as part of a broader sample, data on staff must have been reported separately. We placed no restrictions on language or country.

Search strategy

We undertook electronic searches of eight databases indexing peer-reviewed academic literature including the Cochrane Central Register of Controlled Trials, PubMed/MEDLINE, EMBASE, and PsycINFO (see Supplementary Material for full list). We used a combination of terms relating to healthcare workers (e.g. "doctor" and "clinician"), pandemics and disasters (e.g. "outbreak" and "epidemic" and "disaster*"), interventions (e.g. "treat$" and "training") and mental health outcomes (e.g. "psych$" and "PTSD") (see Supplementary Material). The date limits for searches were 1st January 1995 until 15th February 2021. We supplemented electronic searches by reference list screening and citation tracking using Google Scholar. We asked fourteen experts to nominate additional relevant papers and received responses from seven.

We removed duplicate results prior to title and abstract screening [author initials removed for blind review]. Two reviewers [author initials removed for blind review] independently assessed full texts for

eligibility; disagreements were resolved by a third reviewer [author initials removed for blind review]. If it was not clear whether healthcare workers were included in a study sample, information was requested from the authors. One reviewer [author initials removed for blind review] extracted data outlining study design; participant characteristics; details of the disaster, intervention, and outcomes of interest.

Quality appraisal

The quality of included studies was independently appraised by two reviewers [author initials removed for blind review] using a Quality Appraisal (QA) tool (Hawker, Payne, Kerr, Hardey, & Powell, 2002). See Supplementary Material.

Data Analysis

To compare outcomes between studies, we calculated Cohen's d effect sizes (Van Etten & Taylor, 1998), 95% Confidence Intervals (CI) (Freeman, Hedges, & Olkin, 1986) and reliable change indices (RCI) (Jacobson & Truax, 1991) for all outcomes of interest. Effect sizes (ES) were calculated to provide a standard metric for comparison with other studies. However, effect sizes can be misleading in that they do not always translate to reliable change in clinical outcomes. Reliable change indicates whether a change in symptom severity scores can be attributed to the intervention of interest or is more likely explained by measurement error, even if a statistical difference is indicated (Jacobson & Truax, 1991). This approach is favoured in studies of psychosocial interventions to determine meaningful clinical change (de Souza Costa & de Paula, 2015; Evans, Margison, & Barkham, 1998; McElroy, Napoleone, Wolpert, & Patalay, 2019; Wise, 2004). If the RCI is greater than 1.96, then the change in pre- to post-treatment scores is assumed to be *reliable* at the 95% confidence level ($p = 0.05$) and not due to measurement error. See Supplemental Material for RCI criteria for each measure. Thus, whereas we report effect sizes where available, our criteria for evaluating the efficacy of interventions focused primarily on indices of reliable change.

Strength of evidence

The Oxford 2011 Levels of Evidence framework from the Oxford Centre for Evidence-based Medicine (Howick et al., 2009; 2011) was used to assign levels of evidence to each study to support the clinical recommendations.

Results

Search results

Details of the study selection process are described in the PRISMA flowchart in Figure 1. Fourteen papers were included in the systematic review.

<< INSERT FIGURE 1 >>

Study characteristics

Key characteristics of included studies are summarised in Table 1. Studies were published between 1997 and 2021 and reported on interventions offered to health workers in response to the coronavirus (SARS-CoV-2) outbreak (n = 3) (Dincer & Inangil, 2020; Liu et al., 2021; Tarquinio et al., 2020), the Ebola virus disease (EVD) outbreak (n = 3) (Cole et al., 2020; Sijbrandij et al., 2020; Waterman et al., 2018), hurricanes (n = 3) (Chemtob, Tomas, Law, & Cremniter, 1997; Powell & Yuma-Guerrero, 2016; Waelde et al., 2008), and one study on the Severe Adult Respiratory Syndrome (SARS) outbreak (Chen et al., 2006), an earthquake (Ke et al., 2017), a typhoon (Waelde, Hechanova, Ramos, Macia, & Moschetto, 2017), ongoing conflict and terrorist violence(Berger & Gelkopf, 2011), and the 9/11 terrorist attacks in New York City (Difede et al., 2007). Four studies were carried out in the United States (Chemtob et al., 1997; Difede et al., 2007; Powell & Yuma-Guerrero, 2016; Waelde et al., 2008), three in Sierra Leone (Cole et al., 2020; Sijbrandij et al., 2020; Waterman et al., 2018), two in Taiwan (Chen et al., 2006; Ke et al., 2017), and one each in Israel (Berger & Gelkopf, 2011), Turkey (Dincer & Inangil, 2020), France (Tarquinio et al., 2020), China (Liu et al., 2021), and in the Philippines (Waelde et al., 2017). Two studies (Cole et al., 2020; Waterman et al., 2018) related to one multi-phase study but reported on different outcomes and were therefore both included. No studies evaluated

pre-disaster interventions, six studies (Berger & Gelkopf, 2011; Chen et al., 2006; Dincer & Inangil, 2020; Ke et al., 2017; Liu et al., 2021; Tarquinio et al., 2020) evaluated interventions during the acute phases of the disaster response, and eight studies evaluated interventions delivered 10 weeks to 24 months after the disaster (Chemtob et al., 1997; Cole et al., 2020; Difede et al., 2007; Powell & Yuma-Guerrero, 2016; Sijbrandij et al., 2020; Waelde et al., 2017, 2008; Waterman et al., 2018). The timing of interventions in relation to the disaster is presented in Figure 2 and Table 1.

<< INSERT FIGURE 2 >>

>> INSERT TABLE 1 <<

We identified only two randomised controlled trials (Difede et al., 2007; Dincer & Inangil, 2020), one cluster randomised controlled trial (Sijbrandij et al., 2020), one quasi-randomised controlled trial (Berger & Gelkopf, 2011), one partially controlled case series (Chemtob et al., 1997), and nine case series (Chen et al., 2006; Cole et al., 2020; Ke et al., 2017; Liu et al., 2021; Powell & Yuma-Guerrero, 2016; Tarquinio et al., 2020; Waelde et al., 2017, 2008; Waterman et al., 2018). Four studies reported on interventions that targeted treatment to staff screening positively on measures of psychological distress (Cole et al., 2020; Difede et al., 2007; Ke et al., 2017; Waterman et al., 2018). One study offered a single-session intervention to nurses already receiving therapy prior to the pandemic (Tarquinio et al., 2020). The remaining twelve studies reported on interventions offered to all healthcare staff in a setting regardless of pre-treatment scores. Given large heterogeneity in study populations, interventions and outcome measures, it was not possible to include the results in a meta-analytic synthesis. The effect sizes, CIs and reliable change indices (RCIs) are provided in Table 1.

Appraisal of Studies Included

Overall quality was mixed (see Table 2) with high risk of bias (11 studies (79%) rated poor and 3 studies (21%) rated very poor) (see Supplementary Material for full details of ratings).

>> INSERT TABLE 2 <<

Psychological Outcomes

<u>PTSD</u>

Six studies investigated the effectiveness of a psychosocial intervention on healthcare workers' PTSD symptoms; four were delivered after a disaster or pandemic (Chemtob et al., 1997; Difede et al., 2007; Powell & Yuma-Guerrero, 2016; Waelde et al., 2017, 2008; Waterman et al., 2018) and one was delivered during a disaster (Ke et al., 2017).

An RCT (Difede et al., 2007) of healthcare workers who had been involved in the World Trade Center response demonstrated that 12 weekly 75-minute sessions of cognitive-behavioural therapy (CBT) for acute stress disorder (ASD) led to reliable improvement in PTSD, with authors reporting a large effect size both in the completers' and intention-to-treat analyses, as measured by diagnostic interview (Clinician-Administered PTSD Scale; CAPS). No adverse effects were reported in the CBT arm such as increase in PTSD symptom severity, whereas 21.3% of those in treatment-as-usual (TAU) control group showed a clinically significant deterioration. However, the study lacked placebo or active treatment controls. Further, the intervention had a high rate of drop-out, with only 8/15 participants in the CBT intervention group as compared to 14/16 in the TAU group completing treatment. The dropout analyses suggested that less formal education and lower income may have resulted in less flexibility to attend sessions due to work commitments. Participants with higher alcohol consumption and more severe PTSD symptoms were also less likely to complete treatment.

One partially-controlled trial of debriefing delivered to healthcare workers after a hurricane reported statistically significant reductions. Our analyses further indicated medium effect sizes, but no reliable change in PTSD symptoms (Chemtob et al., 1997).The four remaining studies did not have a control group and were therefore considered case studies (Ke et al., 2017; Waelde et al., 2017, 2008; Waterman et al., 2018). The first case series study (Waterman et al., 2018) was a stepped-care, peer-delivered intervention delivered after a pandemic with ex-Ebola Treatment Centre (ETC) workers in Sierra Leone also reliably reduced PTSD. The phased intervention consisted of a wellbeing workshop (Phase I), a 2-hour psychoeducational workshop (Phase II) and a 6-session low intensity CBT group covering behavioural activation, minimising avoidance, problem solving and coping with anxiety. Our analysis indicated that participants experienced reliable improvement on validated measures of PTSD, however they

remained in the clinical range. The second study was a workshop-based meditation intervention (Waelde et al., 2008) with 20 mental health staff involved in the response to Hurricane Katrina was associated with a statistically significant but not reliable change in self-report measures of PTSD, and again reported a moderate effect size. No adverse outcomes were reported, and 93% reported feeling somewhat or much better as a result of the intervention. More than 60% reported improvements in stress coping, frustration tolerance, activity levels, and depression, and 40% reported improvements in physical pain, sleep problems and fatigue.

The third study consisted of a comprehensive intervention (Ke et al., 2017) delivered during the response to an earthquake in Taiwan. It integrated psychology and psychiatry provision consisting of onsite emotional support, 'debriefing', mini-lectures to raise awareness about mental health, and a 1-year follow up programme to monitor post-traumatic disorders, with occupational safety measures, personal protective equipment, and deployment of additional personnel and resources. The study reported a decrease in distressing intrusive memories from 13.4% at baseline to 0.0% at 1-month follow up. We were unable to calculate effect sizes. In the absence of a control group and use of validated outcome measures, it is not clear if these findings are generalizable to other settings.

The fourth study (Waelde et al., 2017) was a meditation intervention delivered after a disaster and found that 22.1% (n = 15) vs 4.3% (n = 1) participants reported typhoon-related stress pre- vs post-intervention. However, the study used a single-item PTSD screener question from the Structured Clinical Interview for DSM-IV Axis I Disorders (SCID-I) rather than a validated measure of PTSD and suffered from a high attrition rate (45.6%), so we were unable to calculate effect sizes. Dropout analyses found that participants exposed to more typhoon-related stressors were less likely to complete the intervention.

Anxiety

One RCT and five non-controlled studies reported the impact of psychosocial interventions on healthcare workers' anxiety (Chen et al., 2006; Cole et al., 2020; Dincer & Inangil, 2020; Waelde et al., 2017, 2008; Waterman et al., 2018).

A study (Dincer & Inangil, 2020) that randomised nurses in a Covid-19 department in Turkey into a single session emotional freedom techniques (EFT) intervention delivered online reported statistically significant improvements in

anxiety symptoms before and after the 20-minute intervention, with our analyses indicating reliable improvement and a medium effect size.

Two studies (Cole et al., 2020; Waterman et al., 2018) drawing on the same sample of 253 ex-ETC staff in Sierra Leone found that a six week 3-hour group CBT intervention for anxiety and depression was associated. Authors reported both reliable improvement in anxiety symptoms, as well as medium effect sizes.

A hospital-based comprehensive prevention programme during the SARS outbreak in Taiwan (Chen et al., 2006) did not result in reliable improvement in anxiety among staff at one month follow up, although the authors did report statistically significant change and large effect sizes.

A report of a remote single-session eye-movement desensitisation and reprocessing (EMDR) intervention to effects of Covid-19 with a group of nurses already receiving 6-9 months of EMDR therapy for non-Covid-19 related reasons found that it significantly reduced anxiety symptoms one week after the session (Tarquinio et al., 2020), with our analysis indicating reliable improvement. However, as this sample was deliberately drawn from a group that were already receiving ongoing EMDR treatment with one of the study therapists and were chosen on account of having benefited from their previous EMDR sessions, it is not clear how generalisable this finding would be to other healthcare workers.

Two studies reported the positive effects of a 4-hour meditation workshop followed by an 8-week self-study programme on disaster workers' anxiety (Waelde et al., 2017, 2008). The first programme (Waelde et al., 2008) was associated with reliable improvement in anxiety, with a moderate effect size reported among mental health workers following the Hurricane Katrina response. However, participants scored only just above the suggested cut-off for clinically significant anxiety prior to the intervention, and thus may have constituted a generally well group. A second study from the same research group (Waelde et al., 2017) with counsellors and psychologists affected by Typhoon Haiyan did not find improvements in anxiety and reported a small effect size.

A case series of a diaphragmatic breathing relaxation training (DBRT) among hospital-based nursing staff caring for Covid-19 patients reported that 4 weeks of daily 30 minutes of self-practice with MP3 audio recordings significantly but not reliably improved anxiety symptoms (Liu et al., 2021).

<u>Depression</u>

Eight studies examined changes in depressive symptoms. An RCT (Difede et al., 2007) investigating the effectiveness of 12 weekly 75-minute sessions of CBT for ASD in disaster responders with PTSD reported no statistically significant or reliable changes in depressive symptoms, alongside a large effect size. Two studies (Cole et al., 2020; Waterman et al., 2018) reported on different components of an uncontrolled phase-based intervention with ex-ETC staff in Sierra Leone which targeted symptoms of depression. Phase I consisted of a 2-hour Psychological First Aid (PFA) workshop offered to all staff in six ETCs (n = 1553). Phase II comprised of 2-hour workshops on different common mental health problems and incorporated psychoeducation and simple coping strategies based on behavioural and cognitive approaches. Participants were referred into the Phase II depression workshop if they screened positive on a single-item question asking about 'sadness' in the last two weeks. However, participants were able to attend any other workshops if they so wished. Our analyses revealed that the depression workshop was associated with a large effect size and reliable improvement, alongside statistically significant improvement in depression scores, although reported scores remained in the moderately severe range (Waterman et al., 2018). It is unclear from this study if individuals who attended the depression intervention also attended any of the other CBT workshops, and hence may have benefited from a multi-session approach. Phase III of the intervention (Cole et al., 2020), six weeks of 3-hour group CBT for anxiety and depression, was associated with a further reduction in depression though not reliable improvement and only a small reported effect size, with scores remaining the moderate range.

A case series of a comprehensive SARS prevention programme (Chen et al., 2006) focusing on in-service infection control training, provision of PPE and additional medical personnel, and provision of mental health support for patients and staff was implemented at the start of the outbreak in Taiwan's designated SARS treatment hospital. This intervention demonstrated a reliable improvement in depression at one-month follow-up with our analysis revealing a large effect size. A study reporting on a single session of remote-delivered EMDR to Covid-19 stress (Tarquinio et al., 2020) with nurses involved in the pandemic response reported statistically significant reductions, with our analysis indicating reliable improvements in depressive symptoms and a medium effect size. As this study was conducted with nurses who had already been benefitting from 6-9 months of EMDR treatment for non-Covid-19 related reasons it is unclear how generalisable this finding would be to other health workers.

Two case series of mindfulness meditation found that interventions did not statistically or reliably improve depressive symptoms in mental health workers responding to Hurricane Katrina (Waelde et al., 2008) or counsellors and psychologists affected by Typhoon Haiyan (Waelde et al., 2017), with small effect sized reported by authors in both studies. A case series of a 4-week intervention of daily 30 minutes DBRT self-practice did not statistically or reliably improve depressive symptoms in nurses involved in caring for hospitalised Covid-19 patients.

Sleep Quality

Three case series examined the impact of interventions on sleep quality. A stepped-care intervention for ex-ETC staff in Sierra Leone (Waterman et al., 2018) was associated with statistically significant improvements in sleep quality. While a moderate effect size was reported, there was no reliable improvement. A hospital-based comprehensive SARS prevention programme (Chen et al., 2006) implemented during the SARS outbreak in Taiwan resulted in statistically significant improvements in sleep quality and reported a large effect size, but again this finding did not constitute reliable change, and staff remained symptomatic above the clinical cut off. A study of hospital nurses involved in caring for Covid-19 patients in China found that four weeks of daily 30-minute DBRT self-practice resulted in statistically significant improvements in sleep quality, but no reliable change, and staff in this study also remained symptomatic above the clinical cut off.

Quality of Life and other outcomes

Three studies examined the impact of psychosocial interventions on professional quality of life as measured on the Professional Quality of Life (ProQOL), including subscales of Burnout, Compassion Satisfaction and Compassion Fatigue (Berger & Gelkopf, 2011; Powell & Yuma-Guerrero, 2016; Sijbrandij et al., 2020). One quasi-randomised controlled trial of 12 weekly 6-hour sessions with Well Baby clinic nurses during a period of ongoing conflict (Berger & Gelkopf, 2011) reliably improved compassion satisfaction and also made statistically significant reductions in compassion fatigue, burnout and improvements in professional self-efficacy, reporting large effect sizes for compassion satisfaction and professional self-efficacy, and a small to moderate effect size for compassion fatigue. Two separate studies (Powell & Yuma-Guerrero, 2016; Sijbrandij et al., 2020) examined the impact of one-off PFA and Resilience and Coping workshops on improving compassion fatigue and burnout, and found these were not

associated with significant changes with only small effect sizes reported, if at all. Taken together, there is no evidence that single session interventions focused on building resilience or coping skills are effective in significantly and reliably improving staff mental health. Finally, one-session workshops (Powell & Yuma-Guerrero, 2016; Waterman et al., 2018) and 6 sessions of group CBT for anxiety and depression (Waterman et al., 2018) showed no reliable change in perceived stress levels (Cohen, 1988), alcohol use (Difede et al., 2007), work and social adjustment (Cole et al., 2020; Difede et al., 2007) or perceived social support (Powell & Yuma-Guerrero, 2016).

Discussion

This is, to our knowledge, the first systematic review examining the effectiveness of psychosocial interventions specifically targeting healthcare staff before, during and after a disaster or pandemic. Recent reviews have reported heightened prevalence rates of adverse mental health outcomes in health care staff during pandemics (Kisely et al., 2020), and our review provides an important synthesis of the evidence-base for psychosocial interventions aimed at mitigating these adverse outcomes. Consistent with literature for other occupational groups (Brooks, Amlôt, Rubin, & Greenberg, 2018; Brooks, Dunn, Amlôt, Rubin, & Greenberg, 2018; Stanley et al., 2016; Wild, El-Salahi, & Degli Esposti, 2020; Wild et al., 2018), such as police officers, firefighters or paramedics, our review found that more effective interventions targeted specific skills and processes, such as in-service skills training (Berger & Gelkopf, 2011; Chen et al., 2006), behavioural activation, minimising avoidance, problem solving, coping with anxiety (Cole et al., 2020) and mindfulness (Waelde et al., 2008). Interventions based on evidence-based protocols, including CBT for PTSD (Difede et al., 2007) and group-based CBT for anxiety and depression (Cole et al., 2020; Waterman et al., 2018), led to reliable changes in PTSD and anxiety. In contrast, we found that single-session workshops did not result in improvements in PTSD, compassion fatigue or burnout (Chemtob et al., 1997; Ke et al., 2017; Powell & Yuma-Guerrero, 2016; Sijbrandij et al., 2020, Tarquinio et al., 2020). Our review identified no studies that evaluated pre-disaster interventions and a very limited number of studies that evaluated interventions offered peri-disaster.

This review suggests that psychosocial interventions can be helpful in reducing the negative impact on health workers' wellbeing in the acute and subsequent phases of disaster response. However, because of the scarcity

and low quality of evidence, including insufficient knowledge on tolerability, it is not possible to determine the comparative effectiveness of interventions in this review. As such, our findings are based on a limited number of studies without adequate controls, and recommendations will need to be followed with high quality research.

Preliminary evidence supports comprehensive programmes that integrate visible and accessible mental health support with in-service training, enhanced team and institutional support, infection control measures, protective equipment, and increased provision of personnel and resources (Chen et al., 2006; Ke et al., 2017). This is consistent with findings that staff responding to pandemics greatly value social support, team cohesion or organisational support (Bell & Wade, 2020). We recommend providing a comprehensive package of care that deploys additional personnel and resources, allocates adequate protective equipment and infection control, and promotes team cohesion and social support through effective communication and leadership, alongside mental health support.

Healthcare workers with subthreshold symptoms may benefit from lower-intensity interventions during and after a disaster, such as low intensity groups for anxiety, PTSD symptoms, and depression. Group interventions may offer the added benefit of promoting social support and team cohesion. In terms of post-disaster interventions, attention must be focused on eliminating barriers to treatment and making interventions accessible around logistical constraints. In the only RCT of PTSD treatment included in our review, there was a high dropout rate amongst staff linked to fewer years of formal education and lower income. Authors attributed the high dropout rate to clients' difficulties attending sessions due to shift work and long hours.

Interventions of greater frequency, intensity and duration were more effective at reliably improving clinically significant distress as compared to single-session interventions such as debriefing, PFA, and resilience and coping workshops. Evaluations of interventions to build resilience to stress-related psychopathology suggest the importance of targeting known modifiable risk factors of poor mental health (Wild, El-Salahi, et al., 2020), as well as other potential mediators of distress, including job stress, interpersonal isolation, perceived mistrust and fear by others, fear of contagion, concern for family health, and treating ill colleagues, at the organisational level.

Treatment based on current best practice using evidence-based interventions was most effective, with individual trauma-focused CBT leading to reliable change on structured clinical interviews (Difede et al., 2007).

Therefore, CBT for PTSD and CBT approaches to anxiety and depression are the recommended treatment approaches. There is evidence that staff with the highest burden of mental health difficulties were the least likely to request or receive support (Bell & Wade, 2020), failed to access care when offered only a diagnosis of PTSD and signposting to treatment (Difede et al., 2007); and were at risk of treatment dropout as a function of increased exposure to disaster-related stressors (Waelde et al., 2017). As such, we recommend embedding a Screen-and-Treat approach within the comprehensive organisational package of care described above, alongside assertive outreach. A similar approach was implemented by Brewin et al. (2008; 2010) to support the general population following several major incidents in London, UK. In the current context, this approach includes the provision of on-site or directly accessible wellbeing support for staff with trained in-house or independent practitioners, as well as training managers in monitoring risk among their staff and facilitating early intervention for those with the highest burden of mental health difficulties who may be at risk of not accessing treatment.

The scale and nature of the Covid-19 outbreak may result in healthcare workers being exposed to long-term and enduring levels of stress. Most interventions identified in our review were implemented only once the acute threat of the disaster had been resolved. Interventions delivered at these later stages allowed for the more targeted identification and treatment of severely exposed or symptomatic healthcare workers and were frequently delivered in group-based settings. Given the ongoing need for social distancing measures, care planners will need to carefully consider how these types of interventions may be delivered in the current context, as well as planning and preparing staff to cope with possible future surges. Digital transformation may be one such approach to responding effectively to disease outbreaks, and many NICE-recommended treatments, including cognitive therapy for PTSD (Wild, Warnock-Parkes, et al., 2020), Cognitive Processing Therapy (Moring et al., 2020), and Prolonged Exposure (Wells et al., 2020) have already been adapted to be delivered remotely.

This review raises several questions that merit further investigation. For example, research is needed to determine the optimal timing and focus of an intervention for healthcare staff; whether the approach should be targeted or universal; the potential long-term benefits, if any; whether or not the intervention leads to greater reduction of symptoms than would be expected with the passage of time; whether or not the intervention is cost-

effective for the health care system and society; and what types of intervention to offer *before* as opposed to *during* or *after* a disaster, to help build resilience and mitigate future impact.

Our review evaluated interventions delivered to frontline healthcare staff of diverse cultures and ethnicities. However, it is unclear what cultural adaptations, if any, were made. Whilst the success of the CBT interventions in Sierra Leone identified in this review warrant further evaluation, the results suggest the importance of building on existing support networks and structures when delivering evidence-based care. We need to determine why some individuals experiencing threat and high levels of distress do not attend interventions and how to tailor supports to specific groups to make them acceptable and accessible. Ethnic minorities make up a large percentage of the healthcare staff workforce and are at greater risk of developing posttraumatic stress disorder as well as, relevant to this pandemic, dying from COVID-19 (Public Health England, 2020). Interventions should thus target those identified at highest risk, including staff with greater level of trauma exposure, greater levels of proximity with infected patients, those with pre-existing psychological and physical health problems, staff who have been forcibly redeployed, and those in more junior staff roles (Bell & Wade, 2020; Brooks et al., 2018; Kisely et al., 2020). However, there is an urgent need to determine not only what works for whom, but also how and when to offer and deliver evidence-based psychosocial interventions to frontline healthcare staff, who may face psychological and/or practical barriers to accessing support due to perceived stigma (Bell & Wade, 2020), work demands or lack of organisational support (Chen et al., 2006; Ke et al., 2017). In addition, staff will continue to require support around their basic needs, as well as providing adequate staffing and access to infection control measures to limit the risk of exposure (Billings et al., 2020; Brooks et al., 2018).

There are several limitations of this review worth noting. First, the recommendations made apply primarily to frontline medical staff, and the needs of other workers on the frontlines of disasters, including social care staff, porters, administrators, and cleaning staff, need to be considered and addressed (Tan et al., 2020). Although we anticipate that they share many of the risk factors associated with poor psychological outcomes, such as disaster exposure and increased work demands (Tan et al., 2020), there are likely to be distinct differences in their needs that merit further investigation, and recommendations from this review should be generalised to other groups cautiously. Second, the reviewed literature, and the quality of some of the included studies was low and there was

heterogeneity among included studies. Calculating effect sizes and reliable indices of change thus served as a metric to standardise the results and draw meaningful conclusions. Indexing reliable change also allowed for clinically useful analysis of symptom change and assessment of intervention efficacy, given that psychometric measures are inherently subject to measurement error (Jacobson & Truax, 1991). Third, it should be noted that effect size (Cohen's d) calculations for within-group comparisons require a measure of correlation. Because this was not available, we derived within-group effect sizes by assuming statistical independence between pre- and post-intervention scores. This assumption is inaccurate and may result in over-estimating the true effects, which could explain the discrepancy between the reported effect sizes and the indices of reliable change. Fourth, many studies did not include follow-up, and most did not report which of the many outcome measures was their primary outcome. For the purposes of our review, we selected diagnostic or symptom severity outcomes as the primary measure, although we acknowledge that for some studies, this was not their primary outcome. Fifth, in studies investigating PTSD symptomatology, authors did not consistently establish exposure to a Criterion A traumatic event or confirm that symptoms self-reported by participants on screening measures were attributable to the disaster or pandemic-related traumas. As exposure to a life-threatening or severely stressful event is a necessary requirement for ASD and PTSD diagnoses, the true levels of traumatic stress symptomatology – and most effective interventions for addressing them – need to be much more clearly established.

Conclusion

Despite these limitations, our review indicates promising psychosocial interventions that could mitigate the effects of prolonged crisis working in disasters and disease pandemics. Our findings may help to shape the longer-term response to COVID, including future research directions examining which interventions work for whom and what timings are most beneficial for their delivery. This review highlights the urgent need for methodologically sound, high-quality research to guide decision making to ensure healthcare staff on the frontlines are provided optimal and evidence-based care.

Declaration of interests

We declare no competing interests.

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