



A systematic review and meta-analysis of the effect of cognitive interventions to prevent intrusive memories using the trauma film paradigm

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ABSTRACT

There is an unmet need for effective early interventions that can relieve initial trauma symptoms and reduce symptoms of posttraumatic stress disorder (PTSD). We evaluated the efficacy of cognitive interventions compared to control in reducing intrusion frequency and PTSD symptoms in healthy individuals using the trauma film paradigm, in which participants view a film with aversive content as an experimental analogue of trauma exposure. A systematic literature search identified 41 experiments of different cognitive interventions targeting intrusions. In the meta-analysis, the pooled effect size of 52 comparisons comparing cognitive interventions to no-intervention controls on intrusions was moderate ($g = -0.46$, 95% CI $[-0.61$ to $-0.32]$, $p < .001$). The pooled effect size of 16 comparisons on PTSD symptoms was also moderate ($g = -0.31$, 95% CI $[-0.46$ to $-0.17]$, $p < .001$). Both visuospatial interference and imagery rescripting tasks were associated with significantly fewer intrusions than controls, whereas verbal interference and meta-cognitive processing tasks showed nonsignificant effect sizes. Interventions administered after viewing the trauma film showed significantly fewer intrusions than controls, whereas interventions administered during film viewing did not. No experiments had low risk of bias (ROB), 37 experiments had some concerns of ROB, while the remaining four experiments had high ROB. To the best of our knowledge, this is the first meta-analysis investigating the efficacy of cognitive interventions targeting intrusions in non-clinical samples. Results seem to be in favour of visuospatial interference tasks rather than verbal tasks. More research is needed to develop an evidence base on the efficacy of various cognitive interventions and test their clinical translation to reduce intrusive memories of real trauma.

1. Introduction

Posttraumatic stress disorder (PTSD) is one of the most prevalent clinical disorders, with a lifetime prevalence of approximately 8.3% (Kilpatrick et al., 2013). In recent years, several treatments appear

effective for treating PTSD, including trauma-focused cognitive behavioural therapy (Bisson and Andrew, 2007; Bradley et al., 2005) (Bisson and Andrew, 2007; Bradley et al., 2005) and eye movement desensitization and reproprocessing (EMDR) (Cuijpers et al., 2020; Seidler and Wagner, 2006)(Cuijpers et al., 2020; Seidler and Wagner, 2006).

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Nevertheless, on the primary and secondary prevention side the advances have been less pronounced. In other words, preventive strategies aimed at preventing symptoms altogether (primary) or reducing the impact of PTSD symptoms (secondary). Most brief early psychological interventions have been ineffective and, in some cases, may even exacerbate PTSD symptoms (in particular, psychological debriefing; Adler et al., 2008; Mayou et al., 2000; McNally et al., 2003; Sijbrandij et al., 2006). To fill this prevention gap, researchers have searched for new ways to prevent the onset of PTSD, and one promising approach is to focus on reducing intrusions after a potentially traumatic experience (Iyadurai et al., 2019).

Intrusive memories are common after a traumatic experience and are a hallmark symptom of PTSD (American Psychiatric Association, 2013). Intrusions are recurring, unwanted sensory–perceptual impressions that are experienced involuntarily (Clark and Rhyno, 2005). They are predominantly visual (Hoppe et al., 2022), but may also contain sounds, smells, and tastes (Brewin et al., 2010). Usually, the frequency of intrusions subsides within the first weeks following the potentially traumatic event; but if intrusions prolong, they may be indicative of PTSD (Bryant et al., 2011; Carper et al., 2015). Studies have shown that the number of intrusions, their lack of context, their sense of “nowness,” and the associated distress predict the course of subsequent PTSD (Michael et al., 2005; O’Donnell et al., 2007). Intrusive memories are centrally associated with additional PTSD symptoms in the acute phase (Bryant et al., 2015). These findings suggest that intrusions may play a key role in the development of PTSD and as such represent a promising target for preventing its onset, as well as a potentially distressing symptom that could be targeted in its own right even if sub-clinical.

Researchers conceptualize intrusive memories as the result of faulty information processing (Brewin and Holmes, 2003). According to the dual representation theory (Brewin, 2001; Brewin et al., 1996, 2010), two different types of memory representation are encoded in parallel at the time of the traumatic event. Sensory-based representations contain lower-level sensory details such as sights, sounds or smells, and primary emotions experienced during the traumatic event. In contrast, contextual representations contain a subset of the sensory input integrated into an abstract structural narrative in conjunction with the personal and spatial context of the individual experiencing the trauma. The sensory-based and contextual representations are assumed to be tightly interconnected with everyday memories. The extremely distressing nature of experiencing trauma can significantly disrupt cognitive processing, causing strongly encoded sensory-based and weakly encoded contextual representations and a lack of interconnection between the two. Intrusions may be seen as overly strongly encoded sensory-based representations. The dual representation theory argues that, to inhibit the involuntary intrusions, the sensory-based representations need to be reassociated with the contextual representations so that the intrusive sensory and emotional representations are seen in a more realistic context (Brewin and Burgess, 2014).

Ehlers and Clark’s (2000) cognitive theory of PTSD explains intrusions’ development by including an appraisal component. The cognitive theory proposes that PTSD symptoms arise when individuals process the trauma in a way that violates previously held beliefs and produces a strong sense of current threat. Two major mechanisms that produce this sense of current threat involve negative appraisals of the trauma or its sequelae and the way the trauma is encoded into autobiographical memory. Negative appraisals of the trauma and its consequences may include over-generalizing threat (e.g., “the world is dangerous”), negative appraisal of symptoms (e.g., “having intrusions means I am crazy”), other people’s reactions (e.g., “Others think I am weak”) and the future (e.g., “No one will love me”). Furthermore, Ehlers and Clark (2000) distinguish between conceptual encoding (focusing on contextual meaning and integration into autobiographical memory) and data-driven encoding (predominantly focusing on sensory information). They argue that during a traumatic event, elevated stress levels may cause sensory and perceptual information about the event to be

processed in a data-driven manner, leading to poorly elaborated and fragmented memories which can be automatically reactivated by cues associated with the event (intrusions).

The aforementioned theories have directed efforts to develop a range of cognitive interventions that may interfere with the early development of intrusions. These interventions include imagery rescripting, a therapeutic technique that changes the meaning of the original traumatic event by imagining an alternative response or outcome (e.g., Dibbets and Arntz, 2016; Hageraars and Arntz, 2012) and meta-cognitive processing tasks that include people’s monitoring and control of their cognitions (e.g., Buck et al., 2009; Pile et al., 2015) (e.g. Buck et al., 2009; Pile et al., 2015). Other interventions are visuospatial interference tasks, that aim to interfere with (re-)consolidation of traumatic memories by mentally manipulating visual information (e.g., Holmes et al., 2009; James et al., 2015), and interventions concerning cognitive reappraisal about having intrusions, which involves changing the meaning of certain emotional events (e.g., Lang et al., 2009; Woud et al., 2012, 2013). Some studies also tested tasks predicted to have no effect or to worsen intrusions, such as verbal interference tasks (Deeprouse et al., 2012; Holmes et al., 2010).

A common way to test these potential interventions is to use experimental analogue designs in lab environments with healthy samples. Most studies utilized the trauma film paradigm to induce intrusive memories, whereby healthy volunteers are exposed to film scenes containing aversive traumatic content such as death and serious injury. Exposure to aversive film scenes has repeatedly been shown to elicit measurable analogue responses that can in some ways be comparable to symptoms experienced after actual trauma, such as physiological arousal, mood deterioration, and generating intrusive memories; (see Holmes and Bourne, 2008; James, Lau-Zhu, Clark et al., 2016 for an overview). Since intrusive memories provoked by exposure to trauma films tend to subside within a week (e.g., Butler et al., 1995; Holmes et al., 2004), the trauma film paradigm is regarded as an effective and experimental model to temporarily induce intrusions. Ethically, we want a paradigm with a temporary induction of intrusions and, by definition, a *model* of the disorder (such as PTSD), not the disorder itself. Other studies have used audio clips of a traumatic event (e.g., Dorahy et al., 2016. Krans et al., 2010a) or aversive or distressing pictures of the International Affective Picture Series (IAPS; Lang et al., 2008) to induce intrusions in healthy volunteers (e.g., Ehlers et al., 2006). However, these other analogue methods typically induce fewer intrusions than the trauma film paradigm (James et al., 2016a). Furthermore, from pragmatic reasoning, we are limiting to one paradigm to be able to compare studies.

Despite a growing body of research investigating ways to prevent the build-up of intrusions soon after trauma exposure and the possible implications this may have for primary and secondary prevention and treatment of symptoms of post-traumatic stress, the efficacy of cognitive interventions targeting intrusions in healthy volunteers has, to the best of our knowledge, not been quantified using a meta-analysis. Against this background, the current meta-analysis has two objectives. First, to identify and describe what cognitive interventions aimed at preventing and/or reducing intrusions, have been examined in healthy volunteers using the trauma film paradigm. Second, to quantitatively compute an overall estimate of the difference in the mean effect of such interventions compared to no-intervention controls in reducing intrusions and symptoms related to PTSD. Where possible, additional subgroup comparisons were explored, comparing the types of cognitive intervention administered and administration timing.

2. Method

2.1. Literature search strategy

Two reviewers performed a systematic review in four major databases for studies published on or before, June-06-2020: PubMed,

PsycInfo, Scopus and Web of science, and an update search was performed on January-28-2022. Titles and abstracts were identified by combining keywords, text words, and MeSH terms in which synonyms of involuntary (e.g., intrusive, spontaneous) were combined with words indicative of intrusive memories (e.g., intrusion, flashback, reexperience, memory, images) and trauma film (e.g., trauma, distressing, aversive, film, video, clip). The exact search-string for each database is given in [Supplementary Material A](#). In addition, we also checked the references of two previous reviews on the trauma film paradigm (Holmes and Bourne, 2008; James et al., 2016b; Singh et al., 2020).

2.2. Inclusion criteria

Inclusion criteria were: 1. studies had to be published in peer-reviewed journals; 2. studies had to use the trauma film paradigm (i.e., reported that intrusions were induced using a trauma film in a laboratory setting); 3. study samples had to be comprised of healthy adults; 4. studies had to include intrusion frequency as one of their outcome measures (e.g., intrusion diary or a scale that measured the number of intrusions); 5. studies had to have both an experimental intervention condition and a no-intervention or neutral-task control condition; 6. studies had to include a cognitive intervention that was predicted to *reduce* intrusions (excluding for example Dunn et al., 2009; Nixon et al., 2009) with the exception for cases with a concurrent verbal task. Verbal tasks were included even when hypothesized to *increase* intrusions by the original authors due to the possibility that verbal tasks may reduce intrusions according to a non-modality specific general working memory account (Gunter and Bodner, 2008; van den Hout and Engelhard, 2012); and 7. cognitive interventions had to be given either during or, up until one week, after watching the trauma film. The first (JA) and last author (MS) determined study inclusion independently and discussed any disagreements.

2.3. Primary outcome measure: intrusion diary

Most studies used an ‘Intrusion diary’ as outcome measure (Singh et al., 2022a). In this pen-and-paper or electronic diary, participants were asked to record and describe each intrusion from the video clips viewed in the Trauma Film Paradigm each day during seven days. They also reported whether the intrusion was an image, a thought, or a combination of both (e.g. Holmes et al., 2009). For each diary, the intrusion frequency was calculated by counting all intrusion entries (total intrusions) and number of image-based intrusions (total image-based intrusions) per participant (e.g., Holmes et al., 2004, 2009). In most studies, it was also checked whether diary entries matched with actual video clips viewed (e.g., James et al., 2015; Brennen et al., 2021).

2.4. Coding of studies

The following study, participant, and intervention characteristics were coded and extracted from each eligible experiment by the first (JA) and last author (MS): year of publication, country, population type, number of participants, average age, type of procedure, the timing of the intervention and the outcome measures used to capture intrusion frequency and PTSD symptoms.

2.5. Quality assessment

The methodological quality of the selected studies was assessed according to the Cochrane Collaboration’s Risk of Bias 2 tool (ROB 2) (Sterne et al., 2019). ROB 2 includes five key indicators to assess bias: randomization process, intended interventions, missing outcome data, the measurement of the outcome, and the selection of reported results. Two independent raters assessed the methodological quality of the studies and disagreements were resolved through discussion.

2.6. Statistical analyses

The primary outcome measure of the meta-analysis was intrusion frequency post-intervention measured via an intrusion diary or any other assessment tool that measured intrusion frequency (e.g., visual analogue scale). The secondary outcome measure was intrusion/PTSD symptoms measured by the intrusions subscale of the Impact of Events Scale (IES) or the Impact of Events Scale-Revised (IES-R).

The experiments in this meta-analysis had relatively small sample sizes; therefore, we used Hedges’ *g* to measure for effect size. Hedges’ *g* tends to calculate effect sizes more accurately in small samples compared to the more common Cohen’s *d*, which has a bias to overestimate effect sizes when the sample sizes are small (Hedges and Olkin, 1985). Values of Hedges’ *g* are considered small when $g = 0.20$, medium when $g = 0.50$, and large when $g = 0.80$ (Higgins and Green, 2011). Since the primary outcome measure was intrusion frequency and the secondary outcome measure was PTSD symptoms, higher values on these measures would denote more intrusions and more PTSD symptoms, respectively. As such, positive effect sizes indicate that the cognitive intervention group experienced more intrusions/more analogue PTSD symptoms than the control group. In contrast, negative effect sizes indicate that the cognitive intervention group had fewer intrusions/fewer PTSD symptoms than the control group.

When experiments compared two or more similar interventions against one control condition, a single pair-wise comparison was created by combining the means and standard deviations of the intervention condition. Furthermore, in studies where more than one experimental condition was compared to a control condition, the number of participants in the control condition were divided evenly over the experimental conditions so that each participant was used only once in the meta-analysis. Both approaches are advocated in Section 23.3.4 of the Cochrane Handbook for Systematic Reviews of Interventions (Higgins et al., 2022).

Effect sizes were calculated either directly from means, standard deviations, and group sample sizes or via group sample sizes and the reported values from independent *t*-tests. Using a random effects pooling model, pooled mean effect sizes were computed with the Comprehensive Meta-Analysis software (Version 3.3.070; Borenstein et al., 2015). Theoretically, one cannot expect, as is done in the fixed-effect model, that the true effect size for all studies is identical and only varies between studies due to sampling error (Borenstein et al., 2010). In addition, the random-effects model balances the weight assigned to each study more evenly across studies, preventing the summary effect from being overly influenced by studies with relatively large sample sizes. The heterogeneity I^2 statistic, including 95% confidence intervals, was computed to quantify inconsistency across comparisons. A value of 0% indicates no observed heterogeneity, with larger values representing increased heterogeneity. I^2 values of 25%, 50%, and 75% can represent low, moderate, and high heterogeneity (Higgins et al., 2003). Sensitivity analyses were conducted with only studies including risk of bias scored as some concerns (i.e., removing studies with a high risk of bias).

Moreover, subgroup analyses were conducted using the mixed-effects model, which pools studies within subgroups with the random-effects model, and tests for significant differences between subgroups with the fixed-effects model. Subgroup analyses for cognitive intervention type and timing of administration (during vs. after film viewing) were considered. Because of the relatively small sample sizes, subgroup analyses were only conducted for subgroups where at least three comparisons were available.

Publication bias was assessed by visually inspecting the funnel plot and by Duval and Tweedie’s (2000) trim and fill procedure, which provides an estimate of the effect size after the publication bias has been considered (as implemented in Comprehensive Meta-Analysis, version 3.3.070). We also conducted Egger’s test of the intercept (Egger et al., 1997) to quantify the bias captured by the funnel plot and test whether it was significant.

3. Results

The database search identified 1314 unique articles. After title and abstract screening, 115 articles were identified as potentially relevant. In total, 84 articles were excluded due to the following reasons: the intervention was not a cognitive procedure, had no control group, the study did not use the trauma film paradigm, the study was not peer-reviewed, the study used a clinical sample instead of healthy volunteers, or the intervention was given prior to the film. Thirty-one articles met the inclusion criteria (Fig. 1).

3.1. Description of included studies

This meta-analysis included 31 articles describing 41 experiments. In 11 experiments, two types of cognitive intervention were examined against one control condition, resulting in a total of 52 separate intervention versus no-intervention control comparisons. Table 1 presents the characteristics of the included experiments, and a description of each intervention can be found in Table S1 in Supplementary Material.

In total, 28 comparisons examined visuospatial interference tasks, 14 examined verbal interference tasks (note that in some studies verbal tasks were predicted to worsen intrusions, such as from the Holmes lab, but were included due to criteria specified earlier), five examined imagery rescripting, four examined meta-cognitive processing, and one comparison examined attention bias modification (Verwoerd et al., 2012) as a type of intervention. Cognitive reappraisal intervention studies were excluded from this meta-analysis as none of the potential studies (Lang et al., 2009; Woud et al., 2012, 2013) met the inclusion criteria.

Furthermore, 50 comparisons had a no-intervention or neutral-task control condition (i.e., were given a neutral task such as recall only or no-task such as sitting quietly), and two comparisons had an active control condition, namely: positive imagery (which did not involve trauma-processing as it was unrelated to the film; Hageraars and Arntz, 2012)(which did not involve trauma-processing as it was unrelated to

the film; Hageraars and Arntz, 2012), and a control attention bias modification training (Verwoerd et al., 2012).

All comparisons involved healthy (non-clinical) participants (see Table S1 for an overview). Participants were recruited from student populations in 30 comparisons and the general population in 22 comparisons. In 40 comparisons the cognitive intervention was administered after film viewing, while 12 comparisons had the cognitive intervention administered during film viewing.

In total, 50 comparisons used an intrusion diary to measure intrusion frequency; one comparison used a visual analogue scale (Buck et al., 2009) and one categorized the number of intrusions using a six-point Likert scale (Wells and Roussis, 2014). PTSD symptoms were not measured in 36 comparisons. Two comparisons assessed these using the intrusion subscale of the IES, and 14 comparisons used the IES-R.

3.2. Quality assessment

None of the 41 experiments had low ROB (Fig. 2); 37 experiments had some concerns due to risk of bias in measurement of the outcome, while the remaining four experiments had high ROB (see Table 2).

3.3. Cognitive interventions versus control

The pooled effect size of all cognitive interventions compared to control (52 comparisons, 2426 participants) on the primary outcome measure, intrusion frequency, was $g = -0.46$ (95% CI = -0.61 to -0.32 , $p < .001$), with moderate heterogeneity ($I^2 = 65$; 95% CI: 57–74), see Fig. 2 for the forest plot. This overall effect indicates that participants who received a cognitive intervention reported fewer intrusions than participants in the no-intervention/neutral task control group (see Table 3).

The overall effect of cognitive interventions versus control (16 comparisons, 826 subjects) on the secondary outcome, PTSD symptoms, was $g = -0.31$ (95% CI = -0.46 to -0.17 , $p < .001$), with high heterogeneity ($I^2 = 80$; 95% CI: 0–22), see Fig. 3 for the forest plot. This

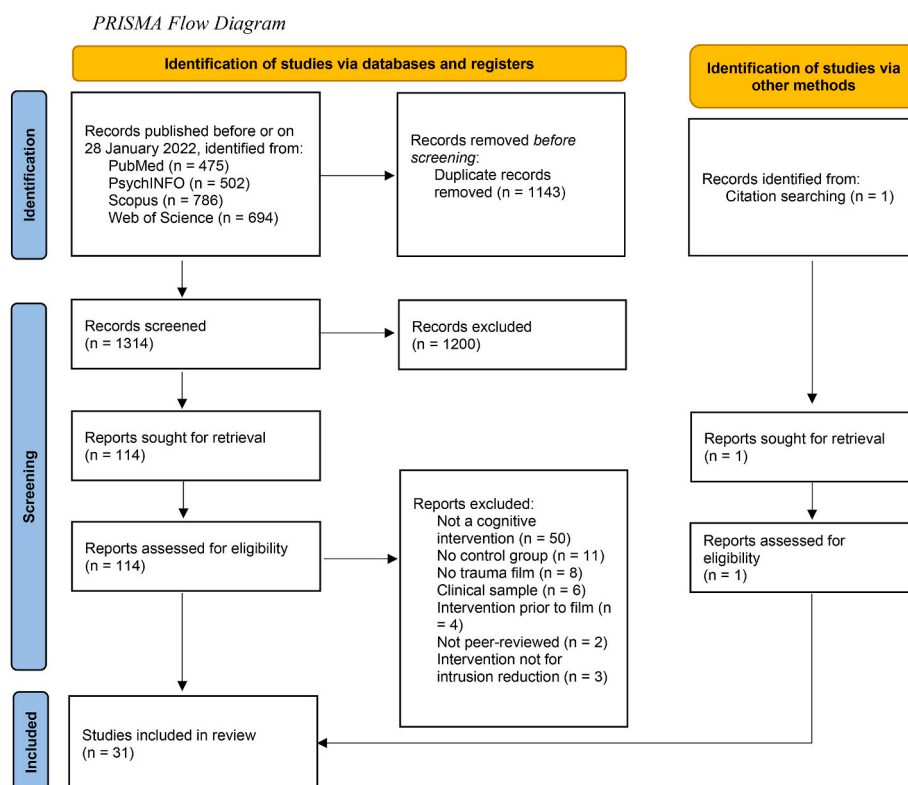


Fig. 1. PRISMA Flow Diagram.

Table 1
Selected Characteristics of Included Studies.

Study	Population ^a	Intervention	Control procedure	Timing of intervention	Outcome measures ^b	Duration between intervention and outcome assessment ^d
Asselbergs et al. (2018), Netherlands						
Expt 1	Students	TGP1 (n = 31)	No task (n = 30)	After film	Intrusion diary	Diary for 7 days
Expt 2	Students	TGP1 dual task (n = 31) TGP2 (n = 30) TGP2 dual task (n = 29)	No task (n = 30)	After film	Intrusion diary	Diary for 7 days
Badawi et al. (2020), Australia	General pop.	Tetris (n = 32)	No task (n = 34)	After film	Intrusion diary IES-R	Diary for 7 days
Bourne et al. (2010), UK		D-Corsi (n = 34)				
Expt 1	General pop.	Visuospatial tapping (n = 11) Counting backwards (n = 15)	No task (n = 14)	During film	Intrusion diary	One-week intrusion diary
Expt 2	General pop.	Counting backwards (n = 19)	No task (n = 19)	During film	Intrusion diary	One-week intrusion diary
Brennen et al. (2021), Norway	General pop.	Reactivation + Tetris after 10 min (n = 33) Reactivation + Tetris after 1.5 h (n = 40) Tetris + reactivation (n = 39)	No task (n = 37)	After film	Intrusion diary, IES	Diary days 2–8
Brewin and Saunders (2001), UK	Students	Visuospatial tapping (n = 20)	No task (n = 19)	During film	Intrusion diary, IES	Two-week intrusion diary
Brühl et al. (2019), Germany	General pop.	Tetris (n = 24)	No task (n = 23)	After film	Intrusion diary	One-week intrusion diary
Buck et al. (2009), Netherlands	Students	Conceptual processing (n = 31)	No task (n = 30)	After film	VAS (intrusions)	After 4 h
Deepröse et al. (2012), UK						
Expt 1	General pop.	Visuospatial tapping (n = 20) Counting backwards (n = 20)	No task (n = 20)	After film	Intrusion diary	Diary for 7 days
Expt 2	General pop.	Visuospatial tapping (n = 25) Counting backwards (n = 25)	No task (n = 25)	After film	Intrusion diary	Diary for 7 days
Dibbets & Arntz. (2016), Netherlands	Students	Early imagery rescripting (n = 25) Late imagery rescripting (n = 25)	Read neutral magazine (n = 25)	After film	Intrusion diary	Diary for 7 days
Hagenaars and Arntz (2012), Netherlands	Students	Imagery rescripting (n = 24)	positive imagery (n = 27)	After film	Intrusion diary	Hand in on Day 7
Hagenaars et al. (2017), Netherlands	Students	Tetris (n = 18) Word games (n = 18)	Reactivation only (n = 18)	After film	Intrusion diary	One-week intrusion diary
Holmes et al. (2009), UK	Students	Tetris (n = 20)	No task (n = 20)	After film	Intrusion diary, IES	One-week intrusion diary
Holmes et al. (2004), UK						
Expt 1	Students	Visuospatial tapping (n = 17)	No task (n = 17)	During film	Intrusion diary	One-week intrusion diary
Expt 2	Students	Visuospatial tapping (n = 20)	No task (n = 20)	During film	Intrusion diary	One-week intrusion diary
Expt 3	Students	Counting backwards (n = 20)	No task (n = 20)	During film	Intrusion diary	One-week intrusion diary
Holmes et al. (2010), UK						
Expt 1	General pop.	Tetris (n = 20) Pub Quiz (N = 20)	No task (n = 20)	After film	Intrusion diary	One-week intrusion diary
Expt 2	General pop.	Tetris (n = 26) Pub Quiz (N = 26)	No task (n = 26)	After film	Intrusion diary	One-week intrusion diary
James et al. (2015), UK						
Expt 1	General pop.	Tetris (n = 26)	No task (n = 26)	After film	Intrusion diary	Diary for 7 days
Expt 2	General pop.	Tetris (n = 18)	No task (n = 18)	After film	Intrusion diary	Diary for 7 days
Kessler et al. (2020), Germany	Students	Tetris (n = 28) QUIZPro IV (n = 30)	No task (n = 28)	After film	Intrusion diary IES-R	Hand in on Day 7
Krans et al. (2010b), Netherlands	Students	Visuospatial tapping (n = 17)	No task (n = 19)	During film	Intrusion diary	One-week intrusion diary
Krans et al. (2009), Netherlands	Students	Counting backwards (n = 29)	No task (n = 23)	During film	Intrusion diary	One-week intrusion diary
Lau-Zhu et al. (2019), UK						
Expt 1	General pop.	Tetris (n = 23)	No task (n = 23)	After film	Intrusion diary	Hand in on Day 8
Expt 2	General pop.	Tetris (n = 18)	No task (n = 18)	After film	Intrusion diary	Hand in on Day 8

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Table 1 (continued)

Study	Population ^a	Intervention	Control procedure	Timing of intervention	Outcome measures ^b	Duration between intervention and outcome assessment ^d
Lau-Zhu et al. (2021), UK	General pop.	Tetris (n = 18)	No task (n = 18)	After film	Intrusion diary	One-week intrusion diary
Logan & O'Kearney (2012), Australia	Students	Mould plasticine (n = 35) Counting backwards (n = 35)	No task (n = 35)	During film	Intrusion diary	Hand in on Day 7
Page and Coxon (2017), UK	General pop.	BlockOut rift (n = 10) Tetris (n = 10)	No task (n = 10)	After film	Intrusion diary	Diary for 7 days
Pile et al. (2015), UK	General pop.	Updating (n = 37)	Viewing a non-traumatic film (n = 37)	After film	Intrusion diary IES-R	Diary for 7 days
Rijkeboer et al. (2020), Netherlands	Students	Imagery rescripting (n = 29)	No task (n = 31)	After film	Intrusion diary	Diary for 6 days
Siegesleitner et al. (2019), Germany	Students	Late imagery rescripting (n = 29)	Read neutral magazine (n = 30)	After film	Intrusion diary	Diary days 2–8
Siegesleitner et al. (2020), Germany	Students	Active imagery rescripting (n = 24) Passive imagery rescripting (n = 20)	No task (n = 20)	After film	Intrusion diary	One-week intrusion diary
Stuart et al. (2006), UK	Students	Modeling plasticine (n = 20)	No task (n = 20)	During film	Intrusion diary	Diary for 7 days
Takarangi et al. (2014), US	General pop.	Downplayed reactions (n = 24)	No task (n = 24)	After film	Intrusion diary, IES	Diary for 7 days
Van Schie et al. (2019), Netherlands	Students	Recall + eye movement (n = 25) Recall + counting backwards (n = 26)	No task (n = 25)	After film	Intrusion diary	Hand in on Day 8
Expt 2	Students	Recall + eye movement (n = 25) Recall + counting backwards (n = 24)	No task (n = 25)	After film	Intrusion diary	Hand in on Day 8
Expt 3	Students	Recall + eye movement (n = 25) Recall + counting backwards (n = 25)	No task (n = 25)	After film	Intrusion diary	Hand in on Day 8
Verwoerd et al. (2012), Netherlands	Students	Attention bias modification task (n = 22)	Control training task (n = 23)	After film	Intrusion diary IMS	After 3 days
Wells and Roussis (2014), UK	Students	Detached mindfulness (n = 14)	No task (n = 14)	After film	6-points Likert scale	After Intervention

Note. pop. = population; TGP = TraumaGameplay; TGP2 = second iteration of TraumaGameplay; VAS = Visual Analogue Scale; IES = Impact of Events Scale; IES-R = Impact of Events Scale-Revised; IMS = Impact of Movie Scale.

c The description of duration of the intrusion diary as described by the original authors, this varied between studies.

^a Note that the samples of all included studies were comprised of healthy participants.

^b The assessment method for the primary outcome (intrusion frequency) is mentioned first, followed by the assessment method of the secondary outcome (PTSD symptoms).

Standardized Effect Sizes of Cognitive Interventions to Reduce Intrusions Compared to Control Groups

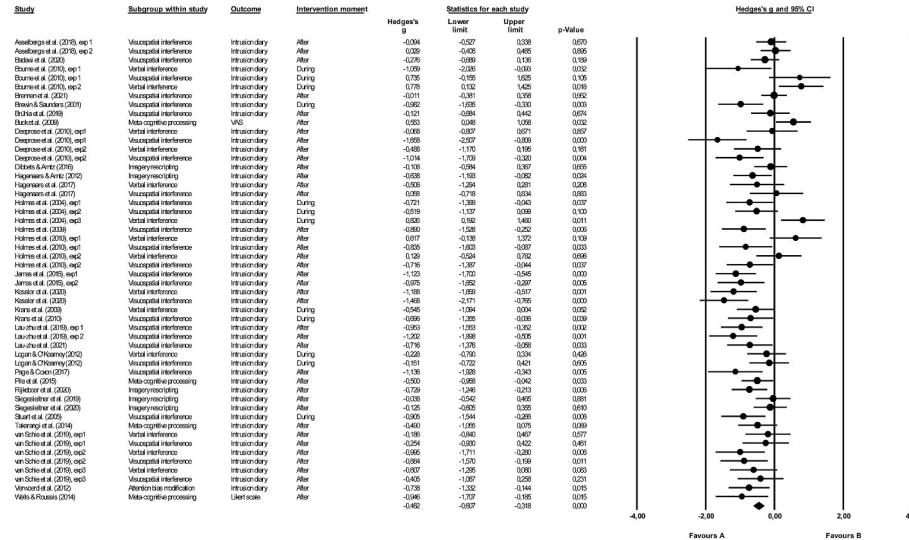


Fig. 2. Standardized effect sizes of cognitive interventions to reduce intrusions compared to control groups.

Table 2

Risks of Bias 2.0 within the included studies.

Study	Bias arising from the randomization process	Bias due to deviations from the intended interventions	Bias due to missing outcome data	Bias in measurement of the outcome	Bias in selection of the reported result	Overall risk of bias ^a
Asselbergs et al. (2018) , Netherlands						
Expt 1	H	H	L	C	L	H
Expt 2	C	L	L	C	L	C
Badawi et al. (2020) , Australia	C	L	L	C	H	H
Bourne et al. (2010) , UK						
Expt 1	C	L	L	C	L	C
Expt 2	C	L	L	C	L	C
Brennen et al. (2021) , Norway	C	C	C	L	L	C
Brewin and Saunders (2001) , UK	C	C	L	C	L	C
Brühl et al. (2019) , Germany	C	L	L	C	L	C
Buck et al. (2009) , Netherlands	C	L	L	C	L	C
Deepröse et al. (2012) , UK						
Expt 1	C	L	L	C	L	C
Expt 2	C	L	L	C	L	C
Dibbets & Arntz. (2016) , Netherlands	L	L	L	C	L	C
Hagenaars and Arntz (2012) , Netherlands	C	L	L	C	L	C
Hagenaars et al. (2017) , Netherlands	C	L	L	C	H	H
Holmes et al. (2009) , UK	C	L	L	C	L	C
Holmes et al. (2004) , UK						
Expt 1	C	L	L	C	L	C
Expt 2	C	L	L	C	L	C
Expt 3	C	L	L	C	L	C
Holmes et al. (2010) , UK						
Expt 1	C	L	L	C	L	C
Expt 2	C	L	L	C	L	C
James et al. (2015) , UK						
Expt 1	C	L	L	C	L	C
Expt 2	C	L	L	C	L	C
Kessler et al. (2020) , Germany	C	L	L	C	L	C
Krans et al. (2010b) , Netherlands	C	L	L	C	L	C
Krans et al. (2009) , Netherlands	C	L	L	C	L	C
Lau-Zhu et al. (2019) , UK						
Expt 1	C	L	L	C	L	C
Expt 2	C	L	L	C	L	C

(continued on next page)

Table 2 (continued)

Study	Bias arising from the randomization process	Bias due to deviations from the intended interventions	Bias due to missing outcome data	Bias in measurement of the outcome	Bias in selection of the reported result	Overall risk of bias ^a
Lau-Zhu et al. (2021), UK	C	C	L	C	L	C
Logan & O'Kearney (2012), Australia	C	L	L	C	L	C
Page and Coxon (2017), UK	C	L	L	C	L	C
Pile et al. (2015), UK	C	L	L	C	L	C
Rijkeboer et al. (2020), Netherlands	C	L	L	C	L	C
Siegesleitner et al. (2019), Germany	C	L	L	C	L	C
Siegesleitner et al. (2020), Germany	C	L	L	C	C	C
Stuart et al. (2006), UK	C	L	L	C	L	C
Takarangi et al. (2014), US	C	L	L	L	L	C
Van Schie et al. (2019), Netherlands	C	L	L	C	L	C
Expt 1	C	L	L	C	L	C
Expt 2	C	L	L	C	L	C
Expt 3	C	L	L	C	L	C
Verwoerd et al. (2012), Netherlands	C	L	L	L	L	C
Wells and Roussis (2014), UK	C	L	L	C	L	C

Note. Green circle and 'L', low risk; red circle and 'H', high risk; yellow circle and 'C', some concerns.

^a The criteria used are for clinical trials (The Risk of Bias 2 (RoB 2) tool) but studies coded here were lab experiments.

Table 3

Effects of cognitive interventions to reduce intrusions compared to control.

	Ncomp	Hedge's g (95% CI)	Q	I ²	df	95%CI ^a	p-value ^b
Overall outcomes							
All experiments	52	−0.46 (−0.61 to −0.32)	147.53	65	51	54–74	<0.001
Subgroup analyses							
Cognitive intervention							0.14
Visuospatial interference	28	−0.61 (−0.80 to −0.42)	69.78	61	27	42–74	
Verbal interference	14	−0.24 (−0.57 to 0.10)	44.69	71	13	50–83	
Imagery rescripting	5	−0.31 (−0.59 to −0.030)	6.25	36	4	0–76	
Meta-cognitive processing	4	−0.32 (−0.94 to 0.30)	14.75	80	3	46–92	
Time of administration							0.28
During film	12	−0.29 (−0.66 to 0.08)	42.44	74	11	54–85	
After film	40	−0.51 (−0.66 to −0.35)	103.15	62	39	47–73	

^a 95% confidence intervals around I².

^b The p-values indicate the significance of differences between the effect sizes in the subgroups.

overall effect indicates that participants who received a cognitive intervention reported lower levels of analogue PTSD symptoms than control participants. Sensitivity analyses removing studies with a high risk of bias (4 comparisons removed) showed no significant changes compared to the original analyses ($g = -0.49$ [95% CI = -0.64 to -0.33 , $p < .001$]). Furthermore, sensitivity analyses removing studies with high risk of bias (1 comparison removed; Badawi et al., 2020) also showed no significant changes in PTSD symptoms ($g = -0.33$ [95% CI = -0.48 to -0.18 , $p < .001$]).

3.4. Subgroup analyses

Subgroup analyses regarding intrusion frequency were conducted for

intervention type (visuospatial interference, verbal interference, imagery rescripting, and meta-cognitive processing) and timing of administration (during vs. after film viewing). Only one study examined the effect of attention bias modification training; hence that intervention type was excluded from the relevant subgroup analysis. The subgroup analyses (Table 3) showed that visuospatial interference tasks ($g = -0.61$, 95% CI = -0.80 to -0.42 , $p < .001$) and imagery rescripting ($g = -0.31$, 95% CI = -0.59 to -0.03 , $p = .03$) resulted in significantly fewer intrusions compared to controls. In contrast, the overall pooled effect sizes of verbal interference tasks and meta-cognitive processing tasks compared to controls were nonsignificant, $p = .17$ and $p = .32$, respectively (Fig. 4). However, no subgroup effects were found, as the difference between the intervention types was not significant in terms of

Standardized Effect Sizes of Cognitive Interventions to Reduce PTSD Symptoms Compared to Control Groups

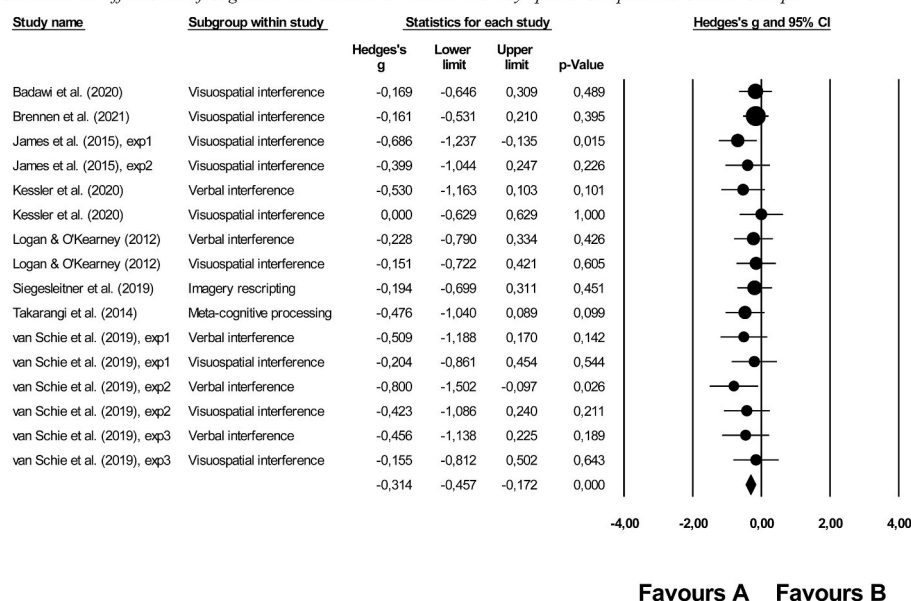


Fig. 3. Standardized effect sizes of cognitive interventions to reduce PTSD symptoms compared to control groups.

Standardized Effect Sizes of the Various Intervention Types for Intrusions Compared to Control Groups

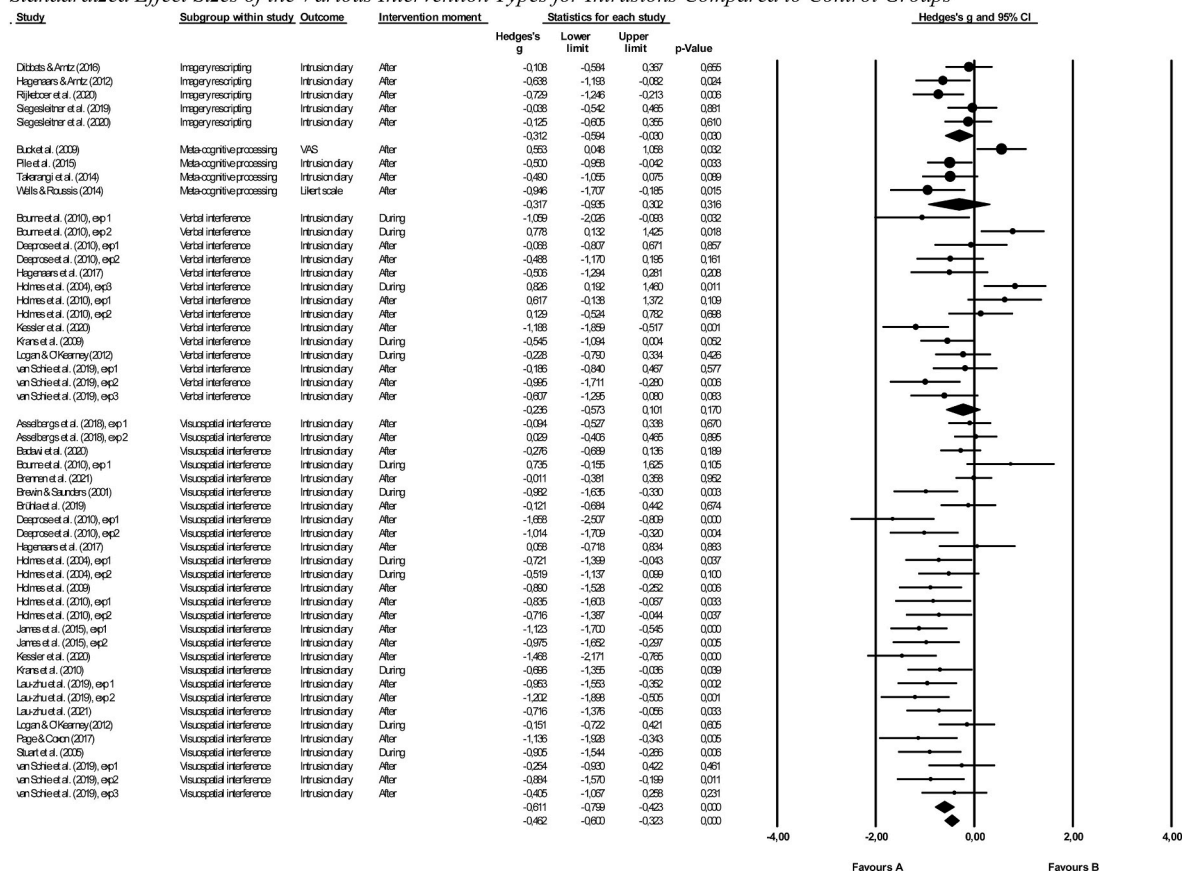


Fig. 4. Standardized Effect Sizes of the Various Intervention Types for Intrusions Compared to Control Groups.

their effect sizes for intrusion frequency ($p = .14$).

Subgroup analysis examining the timing of the intervention (Fig. 5) showed that interventions administered after viewing the trauma film resulted in significantly fewer intrusions compared to controls ($g = -0.51$, 95% CI = -0.66 to -0.35 , $p < .001$), whereas the effect for

interventions administered during film viewing was not significant ($p = .13$). Again, the pooled effect sizes did not differ significantly between interventions that were administered during or after film viewing ($p = .28$), indicating no significant subgroup effects.

Visual inspection of the funnel plot (Fig. 6) did show signs of

Standardized Effect Sizes regarding the Timing of the Intervention for Intrusions Compared to Control Groups

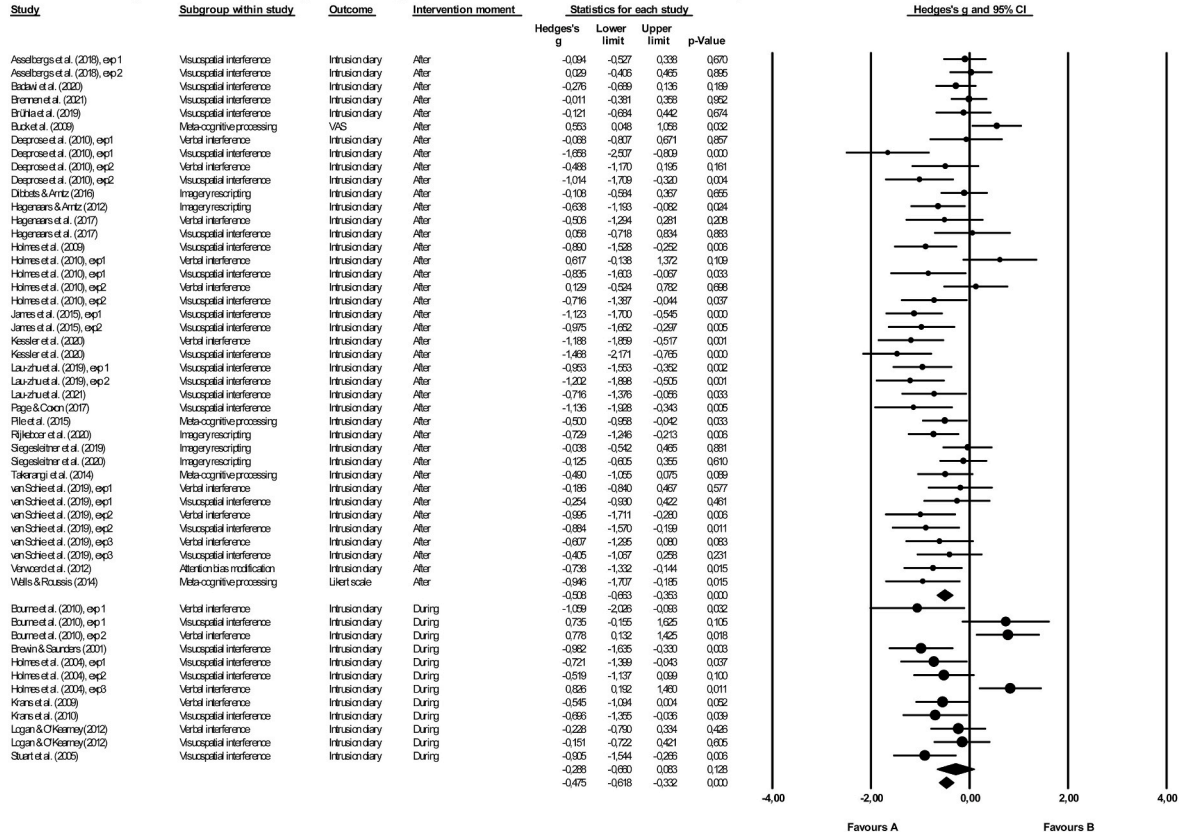
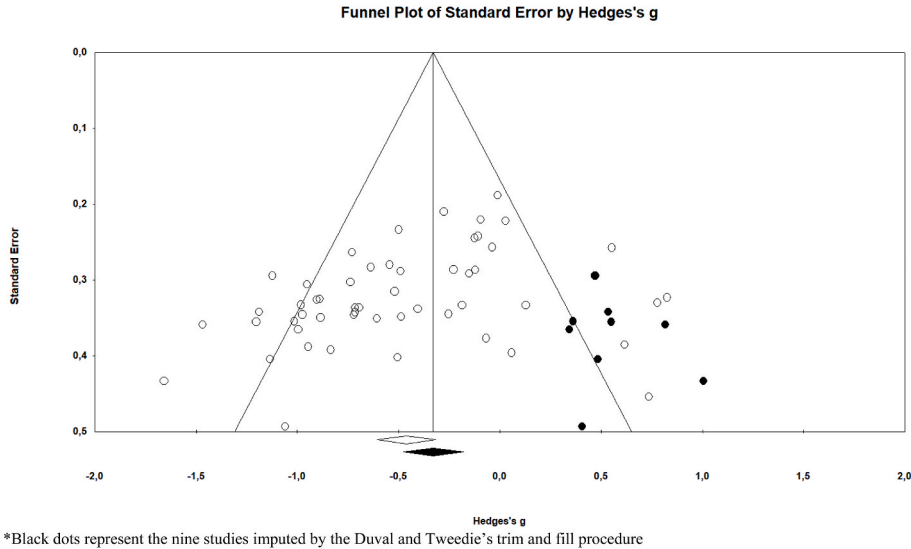


Fig. 5. Standardized Effect Sizes regarding the Timing of the Intervention for Intrusions Compared to Control Groups.

Funnel Plot of Effect Sizes from Included Studies



*Black dots represent the nine studies imputed by the Duval and Tweedie's trim and fill procedure

Fig. 6. Funnel Plot of Effect Sizes from Included Studies.

*Black dots represent the nine studies imputed by the Duval and Tweedie's trim and fill procedure.

asymmetry. Egger's test revealed a significant bias, $t(50) = 2.81, p = .007$. The Duval and Tweedie's trim and fill procedure imputed nine studies (right of the mean), and after adjustment for publication bias, the mean effect size changed from $g = -0.46, 95\% \text{ CI} = -0.61 \text{ to } -0.32$ to $g = -0.33, 95\% \text{ CI} = -0.48 \text{ to } -0.18$. For PTSD symptoms, the Duval and Tweedie's trim and fill procedure imputed four studies (right of the mean), and after adjustment for publication bias, the mean effect size

changed from $g = -0.31, 95\% \text{ CI} = -0.46 \text{ to } -0.17$ to $g = -0.23, 95\% \text{ CI} = -0.36 \text{ to } -0.10$.

4. Discussion

In this meta-analysis, we identified and evaluated controlled experiments testing the effects of cognitive interventions aimed at reducing

intrusions (i.e., visuospatial interfering tasks, imagery rescripting, meta-cognitive processing, and attention bias modification) in healthy volunteers who were exposed to the trauma film paradigm in laboratory settings. In addition, we included verbal interference tasks in our analysis, despite the original authors (e.g., [Deepröse et al., 2012](#); [Holmes et al., 2010](#)) predicting them to worsen rather than reduce intrusions since it is of interest to check whether this was the case as an alternative theory of general working memory suggesting that all modalities of cognitive tasks might be helpful ([van den Hout and Engelhard, 2012](#)). The included studies comprised 52 intervention versus no-intervention control comparisons in all but 2426 cases.

The meta-analysis results showed a moderate overall effect in favour of cognitive interventions over controls in reducing the number of intrusions (primary outcome) and a low to moderate overall effect in favour of cognitive interventions over controls regarding PTSD symptoms (secondary outcome). Subgroup analyses showed that visuospatial interference tasks and imagery rescripting resulted in significantly fewer intrusions compared to controls. Critically, the overall pooled effect sizes of verbal interference tasks and meta-cognitive processing tasks compared to controls were nonsignificant.

Although we found no significant overall difference between the different intervention types, visuospatial interference tasks resulted in significantly fewer intrusions compared to control conditions, whereas this effect was not significant for verbal interference tasks. This difference between broadly visual and verbal tasks is in line with the original authors' predictions of modality-specific interference effects and does not support a general working memory account. This result is interesting as it suggests that task modality plays a relevant role in reducing intrusions (see for example: [Baddeley and Andrade, 2000](#); [Baddeley and Hitch, 1994](#); [Conway and Pleydell-Pearce, 2000](#)); over and above interpretations of more general theories of memory and attention, which have argued that any interference task reduces intrusions through mere distraction ([Engelhard et al., 2011](#); [Gunter and Bodner, 2008](#); [van den Hout and Engelhard, 2012](#)). Since intrusive memories after trauma are predominantly visual ([Hackmann et al., 2004](#)) ([Hackmann et al., 2004](#)), proponents of task modality argue that visuospatial cognitive tasks perform better than verbal tasks because they interfere with visual memory storage ([Brewin, 2014](#); [Holmes et al., 2010](#)). This finding is in line with those earlier studies that showed that visuospatial tasks resulted in significantly fewer intrusions compared to verbal tasks (e.g., [Bourne et al., 2010](#); [Deepröse et al., 2012](#); [Holmes et al., 2004, 2010](#); [Kessler et al., 2020](#)), although other studies have not found this to be the case (e.g., [Hagenaars et al., 2017](#)). It is also in line with studies showing that the content of intrusive memories of trauma is predominantly sensory (not verbal) in both lab and clinic ([Hoppe et al., 2022](#); [Singh et al., 2022b](#)). Even though the results of this meta-analysis hint in favour of visuospatial interference tasks, further inquiries are needed to understand better the role task modality plays in reducing intrusive memories. We did not find an effect for verbal interference tasks. It is premature to conclude whether they increase or decrease intrusions because that can only be determined by comparing them to control tasks. In our study, there is no evidence that verbal interference tasks are effective, but in the long run would be helpful to choose tasks that are more reliably effective.

Despite low power (only 5 available comparisons), our study also found effects for imagery rescripting compared to control. By asking people to imagine different and more positive outcomes related to the trauma or trauma film, imagery rescripting is assumed to change the (emotional) meaning of the memories ([Hagenaars and Arntz, 2012](#)). This is in line with clinical studies, suggesting imagery rescripting may have some benefits in the treatment of PTSD as a stand-alone treatment ([Raabe et al., 2022](#)) or add-on treatment ([Arntz et al., 2007](#)). However, given the few comparisons available and the low quality of most trials, our results should be interpreted with caution.

Furthermore, subgroup analysis on the timing of the intervention showed that cognitive interventions administered *after* viewing the

trauma film resulted in significantly fewer intrusions compared to control, whereas this effect was not significant for cognitive interventions administered *during* the trauma film. Again, pooled effect sizes did not differ significantly for interventions that were administered during or after film viewing. Unfortunately, no causal implications from these subgroup analyses can be drawn; thus, further research is needed to explore the timing of the cognitive interventions. Thus far, initial steps at clinical translation of the timing of cognitive interventions have shown that playing Tetris within 6 h following a motor vehicle accident ([Iyadurai et al., 2018](#)) or an emergency caesarean section ([Horsch et al., 2017](#)) reduced subsequent intrusions (for a recent conceptual replication study, see: [Kanstrup et al., 2021b](#)). There is also early-stage evidence that even intrusive memories that are years old can be reduced by competing task techniques ([Iyadurai et al., 2020](#); [Kanstrup et al., 2021a](#); [Kessler et al., 2020](#); [Thorarinsdottir et al., 2021](#)). This may indicate that the timeframe post-trauma in which an intervention could be administered may be expanded, but further research is warranted.

One of the strengths of this meta-analysis is the broad range of interventions that were included. However, the study also has some limitations. First, our initial attempt for this meta-analysis dates back a few years ago, and we had not pre-registered the study beforehand. Since then, we have updated our literature search multiple times and kept the search up to date. Second, for several intervention types the number of available experiments was small (i.e., attention bias modification $n = 1$, imagery rescripting $n = 5$, meta-cognitive processing $n = 4$), reducing the power and increases the margin of error. Third, all the included studies had at least some concern for bias according to the ROB 2 assessment tool ([Sterne et al., 2019](#)). Fourth, signs of publication bias suggest that the pooled effect sizes might be an overestimation of the actual effect sizes and as such should be interpreted with caution. Publication bias, whereby authors are more likely to submit and journal editors are more likely to publish 'positive' results compared to null findings or unsupportive results ([DeVito and Goldacre, 2018](#)), could lead to unpublished studies (with unsupportive results or null findings) not being included into our meta-analysis. This results in results being overestimated, but stayed significant. Fifth, for our secondary outcome only a small sample of the studies reported PTSD symptoms ($n = 8$ studies: 16 comparisons). Thus, the associated pooled effect size should be interpreted with caution. Sixth, eleven experiments compared two different cognitive interventions against the same control condition. Even though we took adequate steps to protect against double-counting the participants in the shared control condition, it is still possible that including these experiments might have resulted in an artificial reduction of heterogeneity, potentially leading to an oversized pooled effect size ([Higgins et al., 2003](#)). Seventh, this meta-analysis only examined cognitive interventions tested in controlled experiments with healthy volunteers exposed to the trauma film paradigm. Hence, no conclusions may be drawn about the effectiveness of early cognitive interventions in actual trauma survivors. However, studies in (sub-)clinical samples have shown benefits for the use of for example Tetris in reducing intrusions related to real-life negative or potentially traumatic events ([Horsch et al., 2017](#); [Iyadurai et al., 2018](#); [Kanstrup et al., 2021b](#)). Eighth, in this review we chose to only do sensitivity analyses with removing studies with high risk of bias. Our results showed that all included studies have at least some concerns with bias, most notably the bias arising from the randomization process and bias in the measurement of the outcome. In other words, the methodological bias (such as biases from the randomization process, due to deviations from intended interventions, or measurement of the outcome) of included studies lead to uncertain results for example due to an over-estimation of the effect found. Lastly, the current meta-analysis did not assess long-term effects of these cognitive interventions. We note the Trauma Film paradigm extends beyond an initial first day in the lab for 7 days in everyday life. Some work indicates that intrusions on day 1 are associated with those over seven days ([Lau-Zhu et al., 2021a](#)) suggesting the relevance of early effects over time. Most studies did not report longer term follow-up

assessments, thus we could not evaluate beyond a week. Moreover, the Trauma Film Paradigm is designed to temporarily induce intrusions, thus making it challenging to evaluate the effects of the cognitive interventions in the longer run.

In sum, to the best of our knowledge, this is the first systematic review and meta-analysis that investigates the efficacy of cognitive interventions targeting intrusions in healthy volunteers. The results showed that cognitive interventions, especially visuospatial interference tasks and imagery rescripting, hold future promise as part of an early strategy to reduce intrusions and potentially prevent the development of PTSD symptoms. While several studies showed strong effects, more research and particularly replication studies (Lau-Zhu et al., 2021b) are needed to develop an evidence base that can more decisively provide estimates of the efficacy of the various types of cognitive interventions. To do so, careful training in the use of the materials and methods involved is likely to be important (Badawi et al., 2020). In addition, conducting more experiments that directly compare two or more cognitive interventions with each other may help determine which interventions are the most promising in reducing intrusions. Finally, this study also highlights the need for further research on cognitive interventions aimed at reducing intrusions in real trauma patients in clinical settings. While a meta-analysis included studies with clinical samples (Astill-Wright et al., 2021), there is still a high unmet need for an effective early intervention that can relieve initial trauma symptoms (i.e., intrusions) and may even reduce longer term intrusive memories or reduce associated disorder such as PTSD rates. Providing an intervention after a traumatic event is more practical (than one during) as it is easier to administer, offers a larger time window and applies to a broader range of trauma survivors. Thus, future research is needed to evaluate the efficacy of various types of cognitive interventions with most promise in clinical populations. Particularly, the current work suggests that an increased focus on visuospatial interference tasks and imagery rescripting with both years-old or newer intrusive memories is warranted.

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Contributors

Joost Asselbergs designed the study, wrote the protocol, and conducted literature searches and statistical analyses. Jael van Bentum conducted an update search, performed additional statistical analyses, and edited the first draft of the manuscript. Marit Sijbrandij and Joost Asselbergs reviewed full texts, determined inclusions and coded all included studies for risk of bias. Heleen Riper, Pim Cuijpers, and Emily Holmes were involved in critically revising the manuscript. Joost Asselbergs wrote the first draft of the manuscript and all authors contributed to and have approved the final manuscript.

Ethical standards

The authors assert that all procedures contributing to this work comply with the ethical standards of the relevant national and institutional committees on human experimentation and with the Helsinki Declaration of 1975, as revised in 2008.

Availability of data and materials

The authors confirm that the data supporting the findings of this study are available within the article and its supplementary materials.

Declaration of competing interest

All other authors declare that they have no conflicts of interest.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.jpsychires.2023.01.028>.

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