

The effectiveness of dual-task interventions for modulating emotional memories in the laboratory: A meta-analysis[☆]

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ABSTRACT

Dual-tasks (e.g., making horizontal eye-movements) while recollecting a memory are often used both in the lab and the clinic (such as in EMDR therapy) to attenuate emotional memories and intrusive mental images. According to working memory theory, dual-task interventions are effective because they limit cognitive resources available for the processing of emotional memories. However, there is still ongoing debate about the extent to which and under what conditions dual-task interventions are effective to interfere with emotional memories. In this meta-analysis, we assessed $k = 53$ laboratory studies investigating the effects of dual-task interventions on negative and positive memories. The effects were measured with the raw mean reduction in vividness and emotionality self-report ratings of emotional memories before compared to after the intervention on 100-point rating scales. Results showed that the dual-task interventions made both negative and positive memories less vivid (mean reduction negative images = 9.18, 95% CI [7.06, 11.29]; mean reduction positive images = 11.73, 95% CI [8.59, 14.86]) and less emotional (mean reduction negative images = 6.22, 95% CI [4.50, 7.94]; mean reduction positive images = 6.71, 95% CI [2.21, 11.20]). Several moderators were tested and are discussed in the light of working memory theory.

1. Introduction

1.1. Emotional memories and mental images and their role in psychopathology

Memory provides us with a highly adaptive ability: we can remember relevant experiences from the past and use those to adaptively adjust our behavior to the current situation (Baddeley, 2010). However, highly emotional and intrusive autobiographical memories can cause distress and interfere with everyday functioning, as is the case in posttraumatic stress disorder (PTSD) and other psychiatric disorders (e.g., obsessive-compulsive disorder) (Brewin et al., 2010). As such, emotional autobiographical memories are an important target for optimizing current available treatments (e.g., Beckers & Kindt, 2017; Engelhard et al., 2019).

A closely related phenomenon is mental imagery. Mental imagery has been described as “seeing with the mind’s eye” (Cattaneo & Silvanto, 2015) and can be defined as a quasi-perceptual experience occurring in

the absence of perceptual input (Rinck & Denis, 2004). Mental images can be derived from long-term memory or from newly viewed audiovisual stimuli (Conway & Pleydell-Pearce, 2000). Research suggests that mental imagery is involved in cravings (Tiggemann & Kemp, 2005), overeating (McManus & Waller, 1995), and relapse after drug abstinence (Shiffman et al., 1997). Hence, not only negatively valenced autobiographical memories, but also such positive mental images can lead to maladaptive behavior and may thus require consideration to achieve treatment success (May et al., 2004; McClelland et al., 2006). It is worthwhile to note, however, that positive autobiographical memories are typically associated with mental well-being (e.g., Walker et al., 2003).

1.2. Changing emotional memories and images: a dual-task approach

One way in which emotional mental images and memory may be changed is by execute a demanding dual-task. According to Working Memory (WM) theory, keeping a memory or mental images in mind

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requires WM resources (Andrade et al., 1997; Baddeley & Hitch, 1974). These resources can be limited by simultaneously executing a demanding dual-task, such as making horizontal eye-movements, counting backwards, solving mental arithmetic, or even playing the computer game Tetris (van den Hout & Engelhard, 2012). As a result of the competition for resources between the mental image and the dual-task, it is hypothesized that the mental vividness of the mental image will decrease (Andrade et al., 1997). Presumably, the memory can then be reappraised and/or restored in long-term memory in a degraded fashion (e.g., Engelhard et al., 2019; van den Hout et al., 2014).

The evidence for this theory comes from both studies on Eye-Movement and Desensitization Processing (EMDR) therapy, an evidence-based therapy for the treatment of Post-Traumatic Stress Disorder (National Collaborating Centre for Mental Health, 2005; Shapiro & Forrest, 2016), and laboratory studies investigating dual-task interventions. Most often, the working mechanisms of dual-task interventions are investigated in a laboratory model using healthy subjects for safety and feasibility reasons (Andrade et al., 1997; Gunter & Bodner, 2008; van den Hout et al., 2001). Typically, such a laboratory model involves three phases: First, healthy participants are asked to recall a negative autobiographical memory and to rate its vividness and emotionality. Second, one group of participants (i.e. the dual-task group) is asked to recall the memory again while engaging in a demanding dual-task, while the control group is asked to simply recall the memory. Third, the participants are asked to recall the memory again and to rate its vividness and emotionality again. The typical finding from these studies is that, at the post-test, participants in the dual-task condition give lower ratings for the vividness and emotionality of their emotional memories and mental images compared to the control group.

It is worthwhile to note that many different theories have been proposed to explain the effectiveness of EMDR therapy (e.g., increased inter-hemispheric connectivity; for a recent overview see Landin-Romero et al., 2018). However, in the current paper, we focus specifically on the WM explanation for EMDR therapy. Thus, our aim is not compare different theories for explaining the effectiveness of EMDR therapy, but rather to evaluate predictions from the WM theory using meta-analytic tools. As such, we will focus specifically on dual-task intervention studies conducted within the laboratory to interfere with emotional memories and mental images.

1.3. Effects of dual-tasks on emotional memories: prior meta-analyses

Several meta-analyses have been conducted to establish the overall effect of dual-task interventions on emotional autobiographical memories and mental images. However, the results of these meta-analyses are not consistent. In a relatively early meta-analysis, Davidson and Parker (2001) analyzed the results of 13 dismantling studies on EMDR therapy looking specifically at the contribution of eye-movements and found no difference compared to eyes-fixed control conditions. In another meta-analysis, Lee and Cuijpers (2013) examined the effect of executing eye-movements on emotional memories in 14 clinical trials focusing on EMDR therapy and 10 laboratory studies focused on dual-task interventions in the lab. They found an effect of eye-movements, which was particularly pronounced in laboratory studies (Cohen's $d = 0.74$) but was also present in EMDR treatment studies (Cohen's $d = 0.41$). However, in a more recent meta-analysis focusing on EMDR therapy in 10 clinical dismantling studies, no differences were found for EMDR with or without eye-movements (Hedges $g = -0.04$) (Cuijpers et al., 2020). Finally, in a meta-analysis of 15 laboratory studies, Houben et al. (2020) found that eye-movements and other dual-tasks are effective to reduce the self-reported vividness and emotionality of negative autobiographical memories compared to no dual-task control conditions (Cohen's d 's = 0.28-0.59).

Hence, the evidence from these different meta-analyses is inconsistent (i.e., effect sizes ranging from no effects of the dual-task

interventions to medium-to-large effect sizes; Cohen, 1992). This is likely due to differences in the included studies with regard to methodological aspects (e.g., the type of control condition), type of studies included (i.e., laboratory studies or clinical trials), the target populations (clinical, subclinical or healthy), and the used outcome measures (i.e., process-related or symptoms-based) (Cuijpers et al., 2020). These differences complicate any direct comparison between these previous meta-analyses.

So it remains unclear whether and to what extent dual-task interventions are effective, and whether WM theory indeed provides a strong explanation of the effects of the dual-task component of EMDR (Landin-Romero et al., 2018). Furthermore, these prior meta-analyses have placed little emphasis on theoretically important moderators. For instance, WM theory predicts that the dual-task intervention should also be effective for positive emotional memories, but these meta-analyses only focused on negative emotional memories. In addition, WM theory predicts that higher load of the dual-tasks, longer interventions, and weaker memories should produce stronger effects of the dual-task. Also, dual-tasks are expected to change long-term memory and objective memory performance. However, none of these issues were taken into account in these prior meta-analyses. Yet these hypotheses have been tested in extended laboratory-based studies in recent years that will briefly be summarized below.

1.4. Extensions of the dual-task approach

1.4.1. Impact of the type of memory

Most dual-task intervention studies have focused on negative autobiographical memories. However, more recently laboratory studies have exposed participants to aversive pictures, film clips, or virtual reality scenes and asked participants to recall the memory of these stimuli during the dual-task intervention (e.g., Cuperus et al., 2016). Although both procedures use emotional memories, there might be a difference in how malleable these memories are. Specifically, autobiographical memories are older, more complex, and personally relevant, and may therefore be more resistant to change (Conway & Pleydell-Pearce, 2000; Neil Macrae & Roseveare, 2002). Furthermore, memories of high emotional valence are more strongly stored in long-term memory (McGaugh, 2000). Therefore, emotional autobiographical memories may be less malleable by dual-task interventions than memories created in the lab.

1.4.2. Effects on positive mental images

Given that mental imagery is believed to also require WM resources and that positive imagery is related to cravings (e.g., May et al., 2004), laboratory studies have employed the dual-task procedure to investigate whether also positive memories and mental images can be adjusted with this procedure. Indeed, several studies have indicated that dual-tasks also reduce the vividness and emotional intensity of positive mental images (Bartels et al., 2018; Engelhard, van Uijen, et al., 2010b; Hornsveld et al., 2011; Littel et al., 2016).

1.4.3. Variations of the dual-task load

According to WM theory, more demanding dual-tasks should be more effective to modulate emotional memories (i.e., dose-response relationship). Several studies have investigated this issue by increasing the WM load of their dual-tasks and indeed found support for this hypothesis (Maxfield et al., 2008; van Veen et al., 2015), though this was not the case in all studies (Engelhard, van den Hout, & Smeets, 2011b; Mertens et al., 2019).

Another prediction of the WM theory is that matching the modality of the dual-task (most commonly: visuo-spatial or auditory) to the modality of the emotional memory enhances intervention effects. Specifically, because WM is often conceptualized as involving two modality-specific slave-systems (e.g., Baddeley & Hitch, 1974), matching the modality of the dual-task to the modality of the emotional memory

should induce more WM competition. Again, some studies found support for this hypothesis (Kemps & Tiggemann, 2007; Kristjánssdóttir & Lee, 2011; Lilley et al., 2009), but other studies did not (Matthijssen et al., 2019; Mertens, Bouwman, et al., 2020a).

1.4.4. Follow-up effects

Another question is whether the memory effects persist beyond the experimental session. If dual-task interventions do indeed change the long-term memory due to WM competition, as argued by the WM theory, reductions in memory vividness and emotionality, should be maintained over longer time intervals. The results regarding this are somewhat inconsistent. Some studies did not show an effect at follow-up (Kavanagh et al., 2001; Lee & Drummond, 2008), and other studies did (e.g., Asselbergs et al., 2018; Gunter & Bodner, 2008; Leer et al., 2014). Thus, the dual-task interventions can change emotional memory over longer time spans (i.e., beyond immediately after the dual-task intervention) remains unclear.

1.4.5. Longer intervention duration

The lab intervention using the dual-task approach is usually short (typically about 96 s), whereas EMDR therapy usually consist of multiple sessions of at least one hour. Based on WM theory, it is expected that more prolonged competition between the emotional memories and images should produce stronger reductions in the intensity of these memories and images (i.e., dose-response relationship). Some studies have looked into this. One study found that effects a day after the intervention were only present after a longer intervention duration compared to a short intervention (Leer et al., 2014). Another study found that although immediate effects were found after a short intervention, the effect became stronger with longer interventions (van Veen et al., 2019). Nevertheless, few studies have directly compared different durations of WM intervention. Thus, it is unclear whether longer and/or more repeated WM intervention durations affect memory more strongly.

1.4.6. Effects on objective memory performance

According to WM theory, dual-task interventions and EMDR therapy work because the emotional memory becomes less detailed and vivid (Maxfield et al., 2008; van den Hout et al., 2013). While changing the subjective (self-reported) vividness and emotionality of images is crucial from a clinical perspective, as this is what patients report suffering from (Holmes & Mathews, 2010), these measures cannot clarify whether a memory has indeed changed because they are sensitive spurious factors such as socially desirable responding and experimental demand (Orne, 1962). Rather, investigating this hypothesis requires behavioral measures of memory performance (e.g., reaction times or memory accuracy). If the long-term memory indeed becomes less detailed and vivid due to the dual-task intervention, it should be expected that memory performance is affected.

1.5. Goals of the current meta-analysis

The aim of the current meta-analysis was to investigate the above-mentioned research questions based on laboratory research examining the effects of on dual-task interventions in healthy participants. Like the earlier meta-analyses (Cuijpers et al., 2020; Davidson & Parker, 2001; Houben et al., 2020; Lee & Cuijpers, 2013), we aimed to establish the effect of dual-tasks on negative emotional memories. However, we included not just studies focusing on negative autobiographical memories, but also studies using novel aversive stimuli and positive mental images. As a crucial addition, our meta-analysis also focused on theoretically relevant moderators in laboratory analogues of the dual-task paradigm. In particular, in this meta-analysis we examined: (1) whether dual-tasks affect positive mental images; (2) whether more cognitively demanding dual-tasks are more effective; (3) whether longer intervention duration strengthens the effects; (4) whether effects persist beyond the experimental session (i.e., follow-up effects); and (5)

whether dual-task interventions impede objective memory performance. Such analyses establish the conditions under which the effect is most outspoken and test whether theoretical relevant factors indeed moderate the effect (e.g., cognitive load, intervention duration, modality-specificity). This is an important addition to the literature because none of the previous meta-analyses on dual-task interventions focused on these theoretically important moderators.

2. Method

2.1. Pre-registration

The meta-analysis was pre-registered on the Open Science Framework (<https://osf.io/q3xdu/>). We followed the pre-registration with several deviations. First, modality-specificity of dual-tasks was not included as a moderator due to the lack of sufficient available studies investigating this. Second, due to a small number of studies and a lot of variability in procedures between these studies, we conducted a systematic review instead of meta-analysis on objective memory performance. Third, we pre-registered that we aimed to include follow-up tests as a moderator, but instead we investigated the effect at follow-up using separate meta-analyses due to a limited number of studies. Fourth, the analyses regarding the control condition were removed through the revision process, but are still available in the preprint of this paper (<https://psyarxiv.com/3nqt5/>). Finally, instead of using Comprehensive Meta-Analysis software, the Metafor package (Viechtbauer, 2010) in R studio was used, because it could perform the analyses and was free of charge.

2.2. Literature search

Search strategy, screening, and selection criteria adhered to the PRISMA guidelines (Moher et al., 2009). Papers were identified by searching PubMed, PsychInfo, EMBASE and Web of Science. The full search strategy can be found in Fig. 1. The search terms (syntax from PsychInfo) were as follow: eye movement desensitization therapy/ or eye movements/ or ("eye movement*" or "secondary" or "working memory" or "taxation" or "dual" or "counting" or "finger tapping" or "tetris" or "attentional breathing" or "EMDR").ti,ab,id.) and (memory/ or short term memory/ or ("memor*" or "autobiographical").ti,ab,id.) and (emotion* or "distress" or "vivid*" or "intensity").ti,ab,id.). The search was conducted on November 7th, 2018 and an updated search was conducted on May 18th, 2020. In addition to identifying relevant papers through data base search, several researchers were contacted to obtain missing information and unpublished data (i.e., Marcel van den Hout, Marianne Littel, David Kavanagh, Eva Kemps, and Raymond Gunter).

2.3. Screening procedure and inclusion criteria

Two researchers (GM and ML) screened the papers independently using the screening program Rayyan (<https://rayyan.qcri.org>) by reading the title and abstract. When the abstract did not provide sufficient information to make the decision, the full text was screened. Both researchers were blind to each other's decisions during the screening process. After the screening was finished, any discrepancies in the inclusion of papers were discussed and resolved. Before the screening started, the following inclusion criteria were set (PICOS-criteria; Liberati et al., 2009):

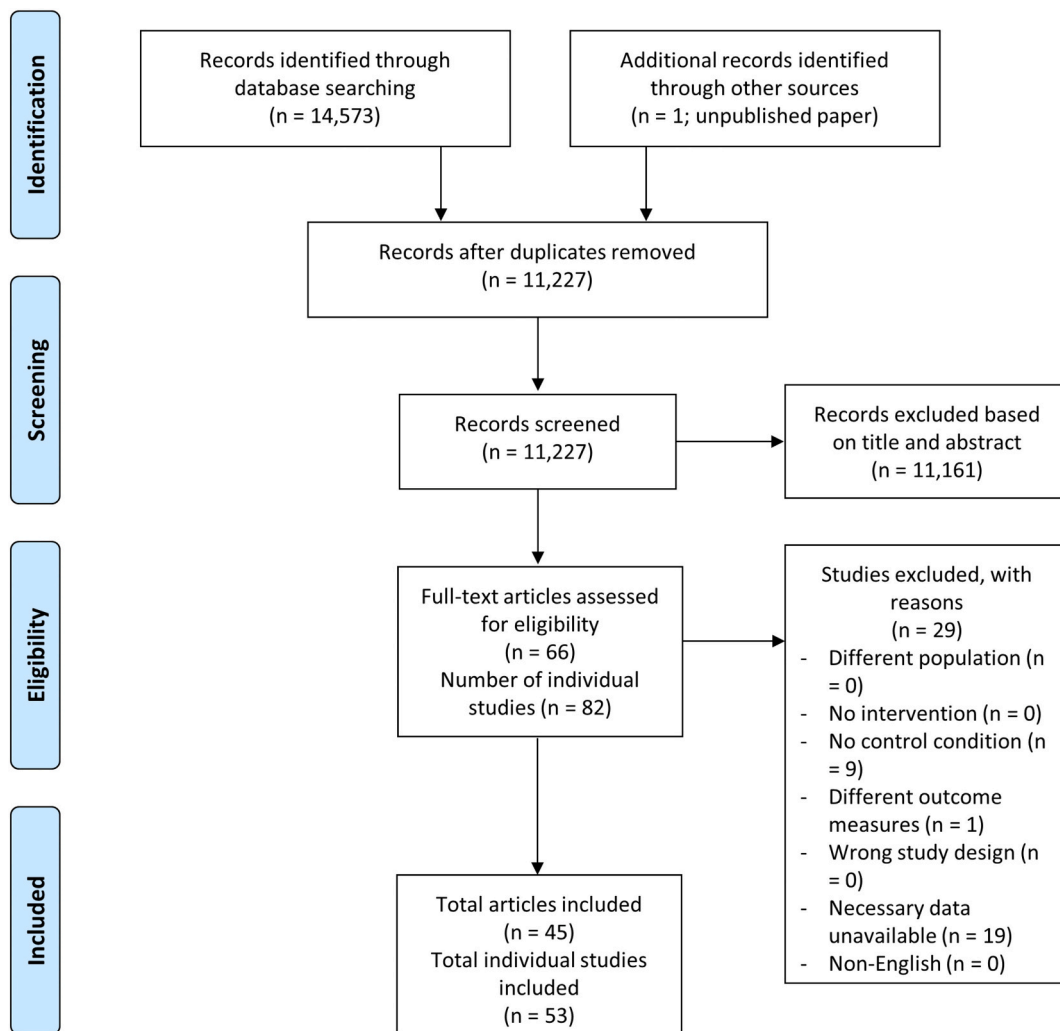


Fig. 1. Flow diagram illustrating the search strategy and exclusion criteria used.

1. Population: The study tested a non-clinical (i.e., healthy or subclinical) sample of adults older than 18 years (i.e., not patients diagnosed with a psychiatric disorder).
2. Interventions: Participants in the dual-task condition were instructed to perform a dual-task (e.g., making eye movements, counting, listening to tones, etc.) while retrieving a positive or negative memory.¹
3. Comparator: Control participants were instructed to retrieve the memory without performing a dual-task.
4. Outcomes: The outcomes were (1) subjective ratings of vividness and emotionality of the memory using rating scales such as Visual Analogue Scales (VASs) or Subjective Units of Distress (SUD) scales before and after performing the dual-task and (2) objective memory performance (i.e., reaction time, memory accuracy and skin conductance response) after the WM intervention.
5. Study design: We selected experimental laboratory studies that investigated the effect of a dual-task on a) self-report measures on vividness and emotionality of emotional memories and/or b) behavioral measures investigating alterations in memory

6. Data availability: Sufficient data had to be available to compute mean difference score and standard deviation. If the means and standard deviation were only reported in a graph, means and standard deviations were extracted using Engauge Digitizer software (<http://digitizer.sourceforge.net>). When the mean and standard deviation for pre-testing and post-testing (or difference scores with SD) was missing from the graph or table, the authors were contacted. If the data could not be obtained, the study was excluded.
7. Publication year: There was no time restriction in terms of publication year.
8. Language: Only papers written in English were included.

¹ This procedural aspect (i.e., explicit instructions to actively recall the memory while performing the dual-task) differentiates the procedure under investigation here from other procedure that focused on the effects of dual-task interventions after spontaneous retrieval of memories (e.g., [Iyadurai et al., 2018](#)).

2.4. Coding

The data extraction sheet was a modified version of the template developed by the Cochrane collaboration ([Higgins & Green, 2008](#)) and is available on the Open Science Framework (<https://osf.io/q3xdu/>). The

coding categories were developed a priori although some were added/deleted during coding when this was deemed necessary/unnecessary. For study characteristics, the following information was extracted: title, authors, publication year, publication type, study aim, and intervention aim. For population characteristics, the following information was extracted: total sample size, sample size for dual-task group and control group, mean age, number of males and females, and healthy or sub-clinical sample. For interventions characteristics, the following information was extracted: intervention duration, number of blocks, number of dual-task groups, type of dual-task(s), time between intervention and assessment of memory effects (“post-test”), and time at follow-up. For comparator characteristics, we only extracted the type of control condition. For outcome characteristics, we extracted: reported memory vividness, reported memory emotionality, subjective units of distress, memory valence, memory modality, type of memory (autobiographical or not), type of scale, and behavioral measures. For study design characteristics, the following information was extracted: independent variable, dependent variable, design, and analytical framework.

2.5. Publication Bias

Publication bias was assessed using a funnel plot asymmetry test (Egger et al., 1997) and a *p*-curve analysis (Simonsohn, Nelson, Simonsohn, 2014a, 2014b). The principle behind the funnel plot is that if all relevant studies are included, the distribution of the effects sizes is symmetrical. The x-axis represents the outcome variable and the y-axis the standard error. An asymmetrically funnel plot indicates publication bias and usually introduces an overestimation of the effect size (Borenstein et al., 2009). When the funnel plot asymmetry test (Egger's regression test) was significant, the Trim-and-Fill method was used (Duval & Tweedie, 2000) to assess the number of missing studies and estimated the average effect when missing studies are included.

Potential publication bias and selective reporting were also examined with a *p*-curve analysis (Simonsohn, Nelson, Simonsohn, 2014b). The *p*-curve entails the distribution of statistically significant *p*-values for a specific sample of studies ($\alpha < 0.05$). If the results reflect true effects, the *p*-curve is right-skewed, showing more “low” ($p < .025$) than “high” ($p > .025$) significant *p*-values. Alternatively, the curve can be uniformly distributed (i.e., an equal proportion for all different *p*-value levels). This shape suggests that there is no population effect. A left skewed *p*-curve (i.e., a higher proportion of *p*-values between 0.025 and 0.05) may indicate selective reporting or *p*-hacking. We used the interaction between time (pre and post-intervention test scores) and condition (dual-task and control task) as the crucial test statistic in the *p*-curve analysis, given that this was the crucial test of interest in most of the included studies.^{2,3}

2.6. Analytical framework

All analysis were conducted in R using the Metafor package (Viechtbauer, 2010). Due to variations in the methods to investigate the effectiveness of WM interventions, the random effects model was used (Thompson & Higgins, 2002). Additionally, differences in the procedure

² Some papers did not provide an interaction term. In these cases, the simple main effects were used. Furthermore, some interactions included an extra dual-task in addition to dual-task and the control task. For these cases, the test statistics for the interaction was still used, because typically the results showed that the crucial dual-task was driving the effect.

³ The rationale behind choosing the interaction term for the *p*-curve instead of the pre-post difference, which was used for the meta-analysis, was that in the meta-analysis we specifically wanted to look at the effect of the control task separately. Given that on average the control task did not really change vividness and emotionality ratings, *p*-curves for the interaction terms and the simple pre-post main effects for the dual-task are near-equivalent.

employed by the studies were assessed as moderator variables in order to investigate whether they accounted for heterogeneity between studies.

When various outcome measures are used, a standardized metric of the effect for each study in a meta-analysis is necessary, such as Cohen's *d* or Hedges' *g* (Lakens, 2013). However, in the present analysis, all studies used the same outcome variable: VAS and/or SUD self-report scales ranging from 0 to 10 or 0-100. We decided to use the difference between post-score and pre-score means in the dual-task condition minus the post-score and pre-score difference in the control condition as the effect size, because raw change scores on the actual used scales are more straightforward to interpret than standardized effect sizes (Bond et al., 2003). Most studies used scales ranging from 0 to 100, so the 0-10 scales were transformed to 0-100 scales by multiplying means and standard deviations with 10. The variance for our effect size measure was calculated by taking the average standard deviation from the pre and post-ratings in both the dual-task condition and the control condition, given by: $((SD_{pre_DT} + SD_{post_DT} / 2) + (SD_{pre_CTRL} + SD_{post_CTRL} / 2) / 2$ (Lakens, 2013).⁴

In order to test homogeneity of the difference scores, the I^2 statistic was calculated, which determines the magnitude of variation due to heterogeneity instead of chance (Higgins et al., 2003). I^2 ranges from 0%-100% and, based on guidelines from Cochrane (Ryan, 2016), $I^2 < 40\%$ was defined as low heterogeneity, I^2 between 40% and 65% was defined as moderate heterogeneity, I^2 from 65% to 90% was defined as substantial heterogeneity, and $I^2 > 90\%$ was defined as considerable heterogeneity.

2.7. Main analysis

It was necessary to conduct separate meta-analyses for vividness and emotionality ratings, because participants reported both (i.e., these data are not independent). Consequently, for negative memories two meta-analyses were conducted for vividness ratings and two were conducted for emotionality ratings (pretest-posttest mean differences and pretest-follow-up test mean differences for each outcome measure).

With regard to positive memories, there were fewer studies and only one included follow up measures. Therefore, one meta-analysis was conducted for vividness ratings and one was conducted for emotionality ratings pre-post mean differences.

2.8. Moderator variables

Based on various methods used in the studies, we decided to include four moderator variables:

1. Type of memory: Most studies focused the intervention on autobiographical memories, but some used memory of emotional stimuli presented earlier in the session instead. The type of memory was grouped as autobiographical or other.

⁴ We acknowledge that using the standard deviation from the difference score would be more appropriate for the design of the included studies (i.e., within-subjects change in pre- to post-test scores) and it would be expected that the standard deviation from the difference score would be slightly smaller than the average standard deviation. Thus, using the average standard deviation is somewhat more conservative (Lakens, 2013). However, around 40% of the studies reported raw means for pre-score and post-score rather than difference scores with standard deviation. Hence, rather than excluding the papers that failed to report difference scores, we decided to use the average standard deviation. An additional advantage of calculating the effect size this way is that it allows us to estimate an average effect size appropriate for both within-subjects studies (pre-post reduction) and between-subjects studies (post-test comparison between a control and a dual-task group), because it does not assume that the variances of the pre- and post-test are related (Lakens, 2013).

- WM demand of the dual-task: The studies used various dual-tasks, and it was not possible to include each of them in the moderator analysis. Instead, the dual-tasks were grouped into low difficulty, medium difficulty and high difficulty. When possible, the grouping was based on reaction time tasks, which several studies used to establish cognitive load of the different tasks (following the initial work by Engelhard, van Uijen et al., 2010b, and van den Hout et al., 2010). If this information was not available, we based the grouping on what we considered to be more or less taxing.
- Intervention duration: Likewise, given variations in the intervention duration we grouped this variable into short, medium, and long duration, using the standard laboratory procedure by van den Hout et al. (2001) (4 blocks of 24 s recollection of a memory while performing the dual-task) as a reference for 'medium', given that it is the most common duration for the dual-task interventions in the reviewed studies (see Table 1). Studies using a shorter duration were grouped as 'short' and those using a longer duration were grouped as 'long'.
- Lab and publication year: The lab and publication year were included as moderators, which is common in meta-analyses. There were no predictions for these moderator variables. The lab was grouped based on the university where the study was conducted and publication year was grouped by decade.

Table 1
Study characteristics of the included studies.

Author(s) and Year	Type of memory	Dual-task	Control condition	Intervention duration	Follow-up	Sample size	Design
Andrade et al. (2012) Exp. 2	Memory of chocolate	Clay modeling & counting	NA	10 min	NA	87	BS
Asselbergs et al. (2018) Exp. 2	Memory of film	Computer game	Recall only	20 block of 24 s	1 week	120	BS
Barrowcliff et al. (2004)	Autobiographical	EM	Eyes fixed	2 blocks of 25 s	NA	80	WS
Bartels et al. (2018)	Autobiographical	EM	Eyes fixed	4 blocks of 24 s	NA	80	Mixed
Brandtner et al. (2020)	Image of gaming	EM	Eyes fixed	4 blocks of 24 s	NA	77	BS
Calvillo and Emami (2019)	Memory of film	EM	Eyes fixed	4 blocks of 24 s	NA	120	BS
Cuperus et al. (2016) Exp. 2	Memory of VR game	Shape-sorter task	Blindfolded	4 blocks of 24 s	NA	34	BS
Devilly and Brown (2011)	Recollection of words	EM	Eyes closed	3 blocks of 20-30s	NA	48	BS
Engelhard et al. (2010a)	Autobiographical	EM	Eyes fixed	4 blocks of 24 s	NA	28	WS
Engelhard et al. (2010b)	Autobiographical	EM and Tetris	Eyes fixed	4 blocks of 24 s	NA	60	WS
Engelhard et al. (2011a)	Autobiographical	EM	Eyes fixed	8 blocks of 24 s	NA	37	WS
Engelhard et al. (2011b)	Autobiographical	Subtraction	Eyes fixed	4 blocks of 24 s	NA	80	BS
Homer and Deeproose (2018)	Autobiographical	EM	Eyes fixed	3 blocks of 60s	NA	26	BS
Hornsveld et al. (2011)	Autobiographical	EM	Eyes fixed	5 blocks of 10s	NA	53	WS
Houben et al. (2018)	Autobiographical	EM	Eyes fixed	4 blocks of 24 s	NA	82	BS
Kearns and Engelhard (2015)	Memory of script	EM	Eyes fixed	4 blocks of 24 s	NA	34	BS
Kristjánssdóttir and Lee (2011)	Autobiographical	EM and counting	Eyes closed	1 block of 60s	NA	36	WS
Lee and Drummond (2008)	Autobiographical	EM	Eyes fixed	45 min	1 week	48	BS
Leer et al. (2013)	Memory of film	EM	Eyes fixed	4 blocks of 24 s	NA	63	BS
Leer et al. (2014)	Autobiographical	Short EM and long EM	Eyes fixed	4 or 8 blocks of 24 s	24 h	73	Mixed
Leer et al. (2017) Exp. 1	Memory of picture	EM	Eyes fixed	1 block of 24 s	NA	26	WS
Leer et al. (2017) Exp. 2	Autobiographical	EM	Eyes fixed	1 block of 24 s	NA	52	BS
Littel & van Schie et al. (2019)	Autobiographical	Counting	Recall only	8 blocks of 24 s	24 h	44	WS
Littel et al. (2017)	Autobiographical	EM	Recall only	6 blocks of 24 s	24 h	56	BS
Markus et al. (2016)	Autobiographical	EM	Eyes fixed	12 blocks of 30s	1 week	47	BS
Matthijssen et al. (2019) Exp. 1	Autobiographical	EM	Relax	8 min	NA	30	WS
Matthijssen et al. (2019) Exp. 2	Autobiographical	EM	Relax	8 min	1 week	75	BS
Maxfield et al. (2008) Exp. 1	Autobiographical	EM	Eyes fixed	10 blocks of 10s	NA	25	WS
Maxfield et al. (2008) Exp. 2	Autobiographical	Fast EM and slow EM	Eyes fixed	10 blocks of 10s	1 week	36	BS
Mertens et al. (2019)	Autobiographical	EM & letter identification	Eyes fixed	4 blocks of 24 s	NA	96	Mixed
Mertens et al. (2020a) Exp. 1 & 2	Memory of picture	Letter identification	Recall only	4 blocks of 24 s	NA	96	WS
Mertens et al. (2020b) Exp. 1	Autobiographical	EM	Recall only	6 blocks of 24 s	24 h	96	Mixed
Onderdonk and van den Hout (2016)	Autobiographical	EM and CVI	Eyes fixed	3 blocks of 24 s	NA	39	WS
Patel and McDowall (2016) Exp. 1	Autobiographical	EM	Eyes fixed	4 blocks 24 s	NA	31	WS
Patel and McDowall (2016) Exp. 2	Autobiographical	Fast EM and slow EM	Eyes fixed	4 blocks 24 s	NA	30	WS
Phaf (2017)	Memory of words	EM	Eyes fixed	1 block of 30s	NA	40	BS
Schubert et al. (2011)	Autobiographical	Fixed EM and varied EM	Eyes closed	45 min	NA	62	BS
Slofstra et al. (2016) Exp. 1	Autobiographical	Attentional breathing	Recall only	4 blocks of 24 s	NA	48	WS
Smeets et al. (2012)	Autobiographical	EM	Eyes fixed	4 blocks of 24 s	NA	61	BS
Tsai and McNally (2014)	Memory of film	Matching-to-sample task	Recall only	NA	NA	80	BS
van den Hout et al. (2013)	Memory of picture	EM	Eyes fixed	13 blocks of 40s	NA	32	BS
van den Hout et al. (2010)	Autobiographical	Counting	Recall only	1 block of 90s	NA	41	WS
van den Hout et al. (2001)	Autobiographical	EM and finger tapping	Recall only	4 blocks of 24 s	NA	60	Mixed
van den Hout et al. (2011a) Exp. 1	Autobiographical	EM & attentional breathing	Recall only	4 blocks of 24 s	NA	36	WS
van den Hout et al. (2011a) Exp. 2	Autobiographical	EM & attentional breathing	Eyes fixed	4 blocks of 24 s	NA	33	WS
van den Hout et al. (2011b) Exp. 4	Autobiographical	EM and tones	Recall only	4 blocks of 24 s	NA	54	WS
van Schie and Leer (2019)	Memory of film	EM	Eyes fixed	4 blocks of 24 s	NA	206	BS
van Schie et al. (2016)	Autobiographical	Fast EM and slow EM	Recall only	4 blocks of 24 s	NA	66	Mixed
van Schie et al. (2019) Exp. 1	Memory of film	EM	Relax	6 blocks of 24 s	NA	76	BS
van Schie et al. (2019) Exp. 2	Memory of film	EM	Relax	16 blocks of 24 s	NA	74	BS
van Schie et al. (2019) Exp. 3	Memory of film	EM	Recall only	16 blocks of 24 s	NA	100	BS
van Veen et al. (2016)	Autobiographical	EM	Eyes fixed	8 blocks of 24 s	NA	108	BS
van Veen et al. (2019)	Autobiographical	EM	Recall only	32 blocks of 24 s	24 h	100	BS

Notes: VR = virtual reality; EM = eye-movements; CVI = changing visual input; BS = Between-Subjects; WS = Within-Subjects; Mixed = typically involved a within-subjects manipulation of the dual-task vs. control condition and an additional between-subjects factor such as the type of dual-task.

3. Results

3.1. Description of studies

The final sample consisted of 53 studies with a combined number of 3328 participants. Table 1 depicts an overview of relevant experimental characteristics. Nine out of the 53 studies investigated objective memory performance. Seven of the included studies focused on positive mental images. Most studies tested a healthy sample of undergraduate students and four out of 53 studies tested sub-clinical analogue samples reporting anxiety symptoms. In most studies, the intervention focused on an autobiographical memory that had some emotional significance, but in 14 out of 53 studies, it focused on memory of an emotional film clip or picture that had been shown in the lab.

Forty-four studies used the horizontal eye-movements dual-task and 17 studies used another dual-task (of which eight studies combined using eye-movements and another dual-task). The control condition was always the same: participants were asked to merely retrieve the memory (without doing a dual-task). However, in 30 studies, participants were asked to fixate their eyes, and in 23 studies no particular instruction to fixate eyes were used (in some of these, participants were blindfolded or closed their eyes, see Table 1). Regarding intervention duration, 23 studies used the standard four blocks of 24 s (separated by 10s breaks); duration was shorter for eight studies and longer for 22 studies. Only ten of 53 studies included a follow-up assessment, whereas the other studies only used a post-test shortly after the intervention.

3.2. Main analyses

3.2.1. Effects of dual-task interventions on negative memories

3.2.1.1. Short-term effects. The difference score between the experimental condition and control condition for vividness ratings was 9.18 (95% CI [7.06, 11.29]) with substantial heterogeneity ($I^2 = 71.92\%$, $p < .001$), indicating that taxing working memory while keeping an emotional memory in mind, on average, reduces the vividness of that memory with 9.18 points on a 0-100 VAS compared to the control condition (see Fig. 2).

Likewise, the difference score between the experimental condition and control condition for emotionality ratings was 6.22 (95% CI [4.50, 7.94]) with moderate heterogeneity ($I^2 = 58.99\%$, $p < .001$), indicating that taxing working memory while keeping an emotional memory in mind, on average reduces the emotionality of that memory with 6.22 points on a 0-100 VAS/SUD⁵ scale compared to the control condition (see Fig. 3).

3.2.1.2. Follow-up effects. Twelve studies tested whether effects of the dual-task intervention persist beyond the laboratory session (i.e., one day or one week later; see Table 1). The difference score between experimental condition and control condition on follow-up vividness ratings of negative memories was 2.65 (95% CI [-0.53, 5.83]) with moderate heterogeneity ($I^2 = 50.67\%$, $p < .02$), indicating that the dual-task intervention did not reliably reduce vividness scores in delayed tests compared to the control condition (i.e., because zero falls within the confidence interval). However, for the difference score between experimental condition and control condition on follow-up-score for emotionality of negative memories, a reliable reduction of 4.36 points (95% CI [0.18, 8.54]) was observed, with substantial heterogeneity ($I^2 = 74.45\%$, $p < .001$). Supplementary Figs. 3 and 4 provide forest plots with the difference scores for vividness and emotionality at follow-up test together with the 95% confidence intervals for each of the

⁵ For emotionality ratings, VAS and SUD were used interchangeably because some studies investigated emotionality/distress using either a VAS scale or SUD scale, whereas for vividness only VAS ratings were used.

included studies (see Supplementary Materials).

3.2.2. Effects of dual-task interventions on positive mental images

The difference score between the experimental condition and control condition for vividness ratings of positive images was 11.73 (95% CI [8.59, 14.86]) with low heterogeneity ($I^2 = 11.59\%$, $p = .35$), indicating that the dual-task intervention, on average, reduces vividness of positive mental images with 11.73 points on a 0-100 VAS compared to the control condition. The difference score between experimental condition and control condition for emotionality ratings of positive images was 6.71 (95% CI [2.21, 11.20]) with moderate heterogeneity ($I^2 = 52.92\%$, $p = .049$). Supplementary Figs. 4 and 5 provide forest plot with the difference scores for vividness and emotionality for positive memories with the 95% confidence intervals for each of the included studies (see Supplementary Materials).

3.3. Moderator analyses

As indicated above, we tested the following three moderators: WM demand of the dual-task, duration of the intervention, and type of memory. The results of these moderator analyses are summarized in Table 2. Only one significant moderator was found: reductions in memory emotionality and vividness were more pronounced for autobiographical memories (Vividness: $M = 10.86$, 95% CI [8.86, 12.85], Emotionality $M = 7.50$, 95% CI [5.66, 9.32]) compared to newly acquired memory of a film or pictures (Vividness: $M = 3.63$, 95% CI [-1.49, 8.75], Emotionality: $M = 2.13$, 95% CI [-1.38, 5.63]). The other factors did not significantly moderate the effect of the experimental condition (see Table 2).⁶

3.4. Quality of included studies

The quality of the studies was screened based on a "Risk of Bias" evaluation tool developed by Cochrane Collaboration (Higgins & Green, 2008). These criteria are developed for meta-analyses on studies using randomized controlled trials, thus the criteria were modified to fit the studies included in the current study. Three criteria were used: (1) Whether prior knowledge of EMDR or prior participation in EMDR studies were used as excluded criteria. Most studies (40 out of 53) did not exclude participants with knowledge about how EMDR works, even though such knowledge may facilitate expectation effects. Yet, so far only three studies have examined the role of expectations in EMDR (Gosselin & Matthews, 1995; Littel et al., 2017; Mertens et al., 2020b). Though all these studies indicated only small and non-significant effects of treatment expectations, more laboratory and clinical studies are needed to assess the role of participants' expectations on the effects of dual-task and EMDR-related interventions. (2) Selective reporting was investigated by inspecting whether all outcome variables in the method section were also reported in the results section. Furthermore, we checked whether the studies were pre-registered. We found no instances of selective reporting, but it can be hard to detect selective reporting and *p*-hacking in published articles (Simmons et al., 2011). Only six of the included studies were pre-registered. (3) We assessed whether the experimenter was blinded to the experimental condition. The assessments in the typical EM laboratory procedure are computerized to reduce experimenter effects (i.e., the experimenter's expectation of the results of the manipulation and subconscious influences on participants'

⁶ Additionally, we included lab and publication year as moderator in the analyses. The lab was a significant moderator for both emotionality, but not vividness. When the studies by Lee and Drummond (2008) and Schubert et al. (2011), were excluded, which were from the same lab, the moderator was not significant anymore. Publication year was not when the two studies were excluded. For reasons of parsimony, these additional moderator analyses can be found in Table 1 in the Supplementary Materials (S.2).

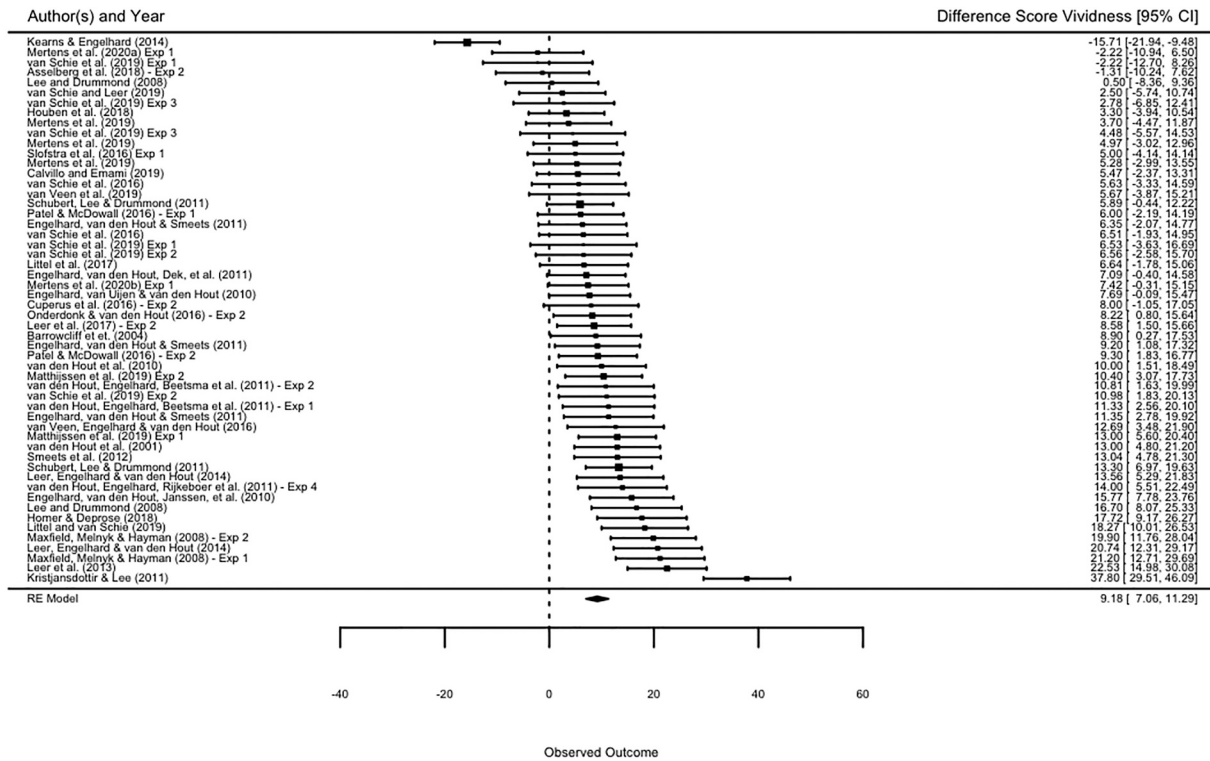


Fig. 2. Forrest plot for studies comparing difference scores for experimental condition and control condition vividness ratings of negative memories.

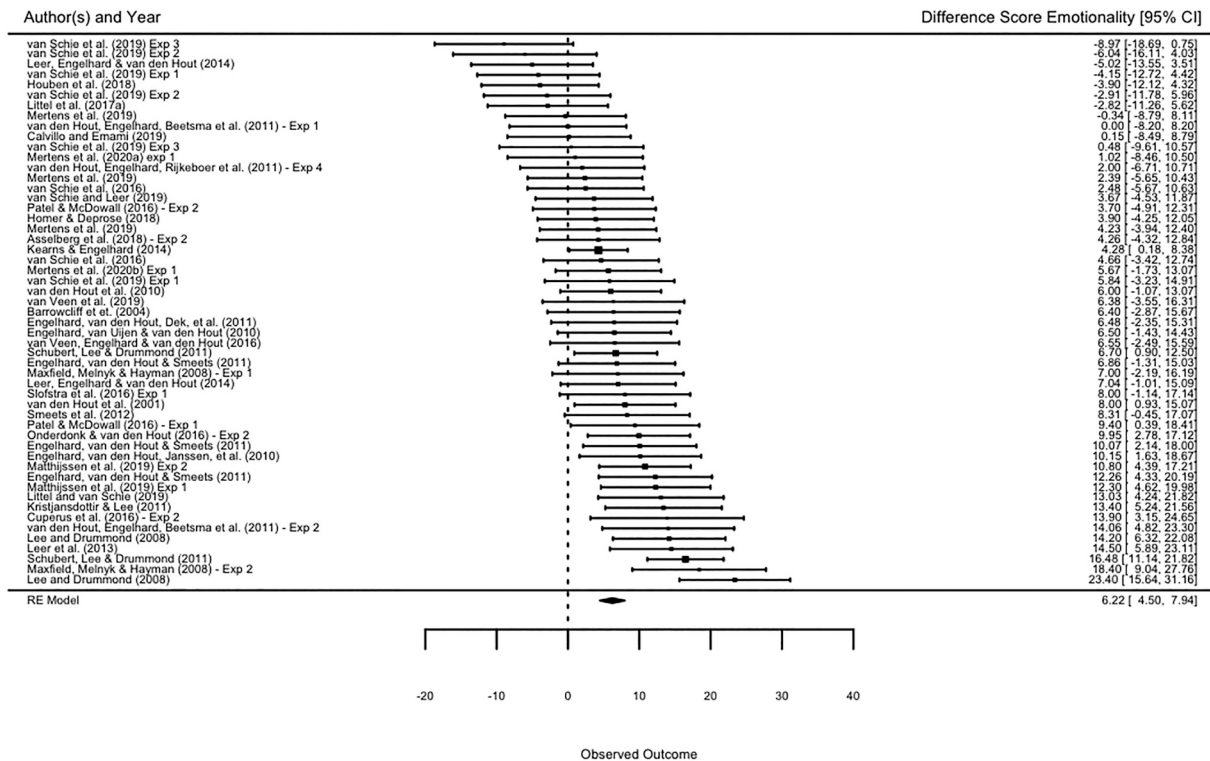


Fig. 3. Forrest plot for studies comparing difference scores for experimental condition and control condition emotionality ratings of negative memories.

behavior; e.g., Doyen et al., 2012). Still, the best way to eliminate such effects is by using assessors who are blind to the condition, but this only occurred in one of the 53 studies (van Schie et al., 2016).

3.5. Publication bias

3.5.1. Funnel plot

The rule of thumb is that funnel-plot asymmetry should be tested when there are more than 10 studies (Higgins & Green, 2008).

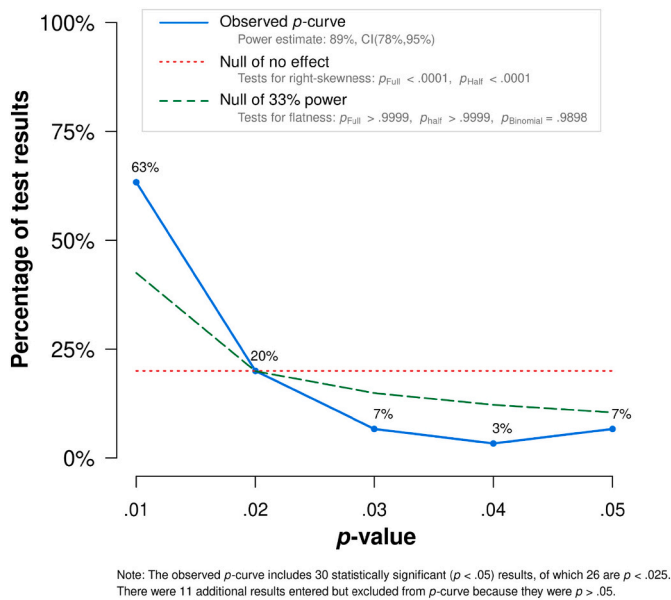


Fig. 4. P -curve distribution of the studies investigating the effect of dual-task on *vividness* of negative and positive images (blue solid line), compared to the expected distribution when the null hypothesis is true (red dotted line) or the alternative hypothesis is true and studies were powered at 33% (green striped line). (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

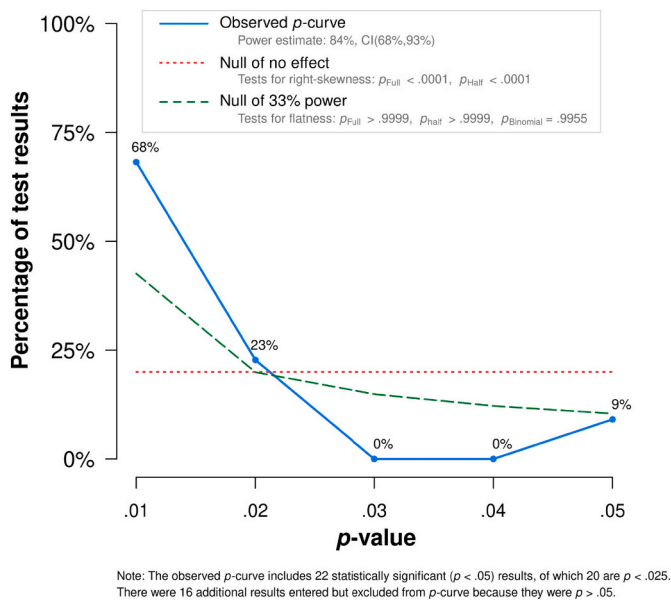


Fig. 5. P -curve distribution of the studies investigating the effect of dual-task interventions on *emotionality* of negative and positive images (blue solid line), compared to the expected distribution when the null hypothesis is true (red dotted line) or the alternative hypothesis is true and studies were powered at 33% (green striped line). (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Therefore, we did not conduct funnel plot asymmetry tests for the follow-up test and positive memories. The Egger's regression test for funnel plot asymmetry was not significant for either vividness or emotionality (see Table 3). This indicated that there is little evidence for publication based on the asymmetry of the funnel plots.

Table 2

Statistical details relating to the moderator analyses.

Factor	Df	QM (estimate)	p
Vividness			
Type of memory	1	9.39	0.002
Difficulty of dual-task	2	1.35	0.717
Intervention duration	2	2.66	0.264
Emotionality			
Type of memory	1	7.45	0.006
Difficulty of dual-task	2	1.62	0.559
Intervention duration	2	1.22	0.542

Note: Bold text indicates a significant ($p < .05$) p -value.

Table 3

Egger's regression test for funnel plot asymmetry for the meta-analyses of the pre-post difference scores between the dual-task and control condition.

Factor	z	P
Vividness	-0.01	0.995
Emotionality	-1.56	0.756

3.5.2. P -curve analyses

To further investigate potential publication bias, two p -curves analyses were conducted: one for vividness ratings and one for emotionality ratings of negative and positive memories. We included 30 significant (< 0.05) p -values for vividness and 22 for emotionality. The p -curve results showed clear evidential value for both vividness (right skew test: $Z = -9.52, p < .0001$; estimated power of the studies = 89%; 90% CI [78%, 95%]) and emotionality (right skew test: $Z = -7.43, p < .0001$; estimated power of the studies = 84%; 90% CI [69%, 93%]). Hence, both p -curves showed strong evidence for the effects of the dual-task interventions on memory emotionality and vividness ratings. Figs. 4 and 5 show the p -curves. The associated disclosure table (see Simonsohn, Nelson, Simmons, 2014a) is included in the Supplementary Materials (S.5).

3.6. Objective memory performance

Due to the large variety in methods and limited number of studies, the effect of the dual-task intervention on objective memory performance was reviewed rather than analyzed. Table 4 shows the study characteristics and conclusions. Nine studies were included: five of them showed memory impairment in the dual-task condition, compared to the control condition, as predicted by WM theory; three studies did not find a difference between the dual-task condition and the control condition; and one study found the opposite (memory enhancement in the dual-task condition). Hence, the evidence for the effect of dual-task interventions on objective memory performance is mixed. It can be further noted that there was quite some variation in the procedures between these studies. Three studies made use of a false-information procedure about a film, two studies used a word-list learning procedure, two other studies used an old-new judgement task, and finally two other studies used yet two other procedures (i.e., generalization in a fear conditioning procedure and a matching-to-sample task). As such, it is quite difficult to directly compare these different studies with one another. However, two of the included studies (i.e., Calvillo & Emami, 2019; van Schie & Leer, 2019) were direct replication of another study (Houben et al., 2018), but could not replicate the results. This may suggest that some of the findings regarding effects of dual-tasks on objective memory performance are weak and unreliable. Given the limited number of studies and variability in the procedures, strong conclusions are difficult to draw. Clearly, more research is needed to come to more definitive conclusions about the effects of dual-task interventions on memory performance.

Table 4
Summary of studies investigating objective memory performance after a dual-task intervention.

Author(s) and Year	Procedure	Outcome measure(s)	Conclusion	Sample Size
Calvillo & Emami, 2019	Misinformation about a film of a car crash. Participants had to recall the film while making eye-movements or not.	Memory accuracy, endorsement of misinformation, and robust false memories.	No difference between dual-task and control condition	120
Devilley and Brown (2011)	Participants rehearsed a list of words while performing EM or merely rehearsing the words.	Number of words accurately recognized and recalled.	No difference between the dual-task condition and control condition.	48
Houben et al. (2018)	Misinformation about a film of a car crash. Participants had to recall the film while making eye-movements or not.	Memory accuracy and endorsement of misinformation	Less correct memory and more endorsement of misinformation in the dual-task condition	82
van den Hout et al. (2013)	Old-new judgement of pictures. Pictures were recalled while making eye-movements or not.	The time it took to decide whether the fragment was previously seen or not.	Slower judgement of new-old status in the dual-task condition.	32
Leer et al. (2017) Exp. 1	Old-new judgement of pictures. Pictures were recalled while making eye-movements or not.	The time it took to decide whether the fragment was previously seen or not.	Slower judgement of new-old status in the dual-task condition.	27
Leer et al. (2017) Exp. 2	Recall an image of a male face previously paired with an electric shock while performing eye-movements or not.	Generalization of shock expectancy ratings and skin conductance to similar faces.	More generalization of expectancy ratings and skin conductance in the dual-task condition.	52
Phaf (2017)	Perform eye-movements while recalling a list of words or merely recall the words.	Number of words correctly recalled.	Better performance in the dual-task condition.	40
Tsai and McNally (2014)	Matching-to-sample task with positive, neutral or negative pictures while recalling an aversive film clip.	Details remembered from the film clip.	Reduced memory detail in the positive matching-to-sample task, but not the neutral or negative variant.	80
van Schie and Leer (2019)	Misinformation about a film of a car crash. Participants had to recall the film while making eye-movements or not.	Memory accuracy and endorsement of misinformation	No difference between dual-task and control condition	206

4. Discussion

To date, extensive research has been conducted with the purpose of examining whether dual-task interventions can effectively decrease vividness and emotionality of emotional memories and images. Furthermore, efforts have been made to disentangle the mechanisms underlying the effectiveness of dual-task interventions such as the eye-movement component of EMDR therapy. The current meta-analysis demonstrated that taxing WM by performing a dual-task while keeping an emotional memory in mind results in reduced vividness and emotionality when the person recalls that memory again after the intervention. Furthermore, the results show that the effects of WM interventions are not restricted to negative memories but also render positive memories less vivid and emotional. With regard to longer term effects (i.e., 24 h to one week after the intervention), the effects of the dual-task intervention were not outspoken (i.e., only about 2.5 to 4.5 points on the VAS ratings) and only reliably observed for emotionality ratings. These results are consistent with the findings of previous research demonstrating reliable effects of WM interventions on emotional memories and imagery (Landin-Romero et al., 2018; Lee & Cuijpers, 2013; van den Hout & Engelhard, 2012).

Concerning moderators of this effect, even though most of the studies showed an effect of dual-task interventions on emotional memories, results were heterogeneous, ranging from small to large decreases. Only one significant moderator of the effects of dual-task interventions on emotional memories was found: the type of memory. Particularly, effects of the dual-task were more outspoken for both memory vividness and emotionality when the memory was autobiographical compared to a novel memory based on a movie clip or an image. This is somewhat surprising, because it would be expected that autobiographical memories are more strongly consolidated into long-term memory and may therefore be more difficult to change (e.g., McGaugh, 2000). Additionally, typically participants in these studies are asked to select an unpleasant memory of at least one week old, suggesting that quite stable emotional memories are typically selected and these would be expected to be not very amendable. However, one difference between the autobiographical memories and the other memories is that the biographical memories are typically more emotional (e.g., death of a relative) than the other memories, the latter which are typically based on relatively weak

stimulus material (e.g., unpleasant pictures) due to ethical concerns. As such, there is a larger range for reductions in memory vividness and emotionality of autobiographical memories than other memories. Another explanation is that autobiographical memories are more emotional, which requires more WM capacity to recall compared to less emotional memories based on novel stimulus material (see van den Hout et al., 2014). As a result, there may be more WM competition when recalling autobiographical memories than when recalling memories of novel stimulus material. The exact explanation for this moderation based on the type of memory requires further research.

None of the other moderators (i.e., difficulty of the dual-task and duration of the intervention) significantly moderated the effects of the dual-task interventions. This is not consistent with WM theory of dual-task interventions, because based on the WM theory it is expected that more difficult dual-tasks and less strongly consolidated memories should be more effective/sensitive to change than their counterparts. However, it should be mentioned that there are alternative reasons why these moderators were not significant in the current meta-analysis. For instance, the variation in the difficulty of the dual-task intervention may have been insufficient to find differences (e.g., Littel and van Schie, 2019) or there was a lack of sufficient statistical power to detect differences due to the limited number of studies. Furthermore, the vast majority of studies used horizontal eye-movements as the dual-task intervention. Hence, there was fairly limited variation in the type of dual-task interventions that were used. These considerations and limitations are inherent to meta-analyses, which are necessarily limited to procedurally characteristics of the available studies. More studies which vary in procedurally important aspects of the interventions (e.g., duration, task difficulty, type of dual-task) are needed to establish whether such factors influence the outcomes.

Interestingly, the vividness and emotionality decrease at the follow-up test was weaker than at the immediate post-test. This suggests that the effects of dual-task interventions are not necessarily long lasting. One alternative explanation is that the pre-test phase of the reviewed studies leads to a short-lived inflation of the memory (i.e., participants are typically asked to vividly recall their memory and rate it), which is later counteracted by the intervention and time. Indeed, a similar pattern was observed in a recent study by van Veen et al. (2019). The dual-task intervention showed immediate effects in decreasing vividness

and emotionality, whereas the control conditions showed a greater effect at follow-up. The authors argued that this could be due to the delayed effect of the control condition which functioned as imaginal exposure or that the working mechanisms of the dual-task intervention became less active over time. Whatever the precise mechanism, it is clear that long-term effects of dual-task interventions require more research given that only a limited number of studies have looked into this (see Table 1). Furthermore, it can be noted that the follow-up interval was rather short in the included studies (one day up to one week). More research is required to establish whether the dual-task effects are maintained over longer periods (e.g., six months up to one year).

Regarding positive memories, the results demonstrated that dual-task interventions are effective regardless of the memory valence. Furthermore, the effect was more outspoken for vividness (11.73, 95% CI [8.59, 14.86]) than for emotionality (6.71, 95% CI [2.21, 11.20]). Additionally, the effect for emotionality was lower for positive mental images than for negative memories, but the effect of vividness was comparable across these types of memories. Previous research has demonstrated that positive memories are rated as less emotional compared to negative memories (Bohanek et al., 2005). This might explain the difference in reduced emotionality for positive compared to negative memories. Nevertheless, the results of the meta-analysis showed that dual-task interventions are effective in modulating positive memory and imagery.

Concerning the effects of dual-task interventions on objective memory performance, we found only a limited amount of studies that have tested this. Some studies examined susceptibility to misinformation, some examined response latency in a stimulus discrimination task, and some examined recall accuracy. The evidence for an effect of dual-task interventions on objective memory performance was mixed based on the included studies. Due to the lack of a sufficient number of studies and the wide variability in the used procedures, it was not possible to quantitatively examine the results. Clearly, further research should be conducted to test the effects of dual-tasks interventions on objective memory performance and susceptibility to misinformation, which is particularly relevant with respect to eye-witness testimony (see Houben et al., 2018; van Schie & Leer, 2019).

Finally, no evidence for publication bias was found using either Egger's regression test for funnel plot asymmetry or *p*-curve analyses. Rather, the results of the *p*-curve analysis showed a right skewed curve for both vividness and emotionality ratings. Thus, the results indicated strong evidential value for the effects of dual-tasks on vividness and emotionality ratings for memory and imagery. That is, it is unlikely that results from this meta-analysis were *p*-hacked or only reflect the selective publication of false positive results (Simonsohn, Nelson, Simmons, 2014b). Hence, the effects of dual-task interventions on memory vividness and emotionality are most likely robust and appear to be not or only minimally affected by selective reporting or publication bias. However, note that this conclusion only applies to the short-term effects of dual-tasks on negative emotional memories (see Section 3.5) and thus does not apply to positive mental images or follow-up effects of dual-tasks.

4.1. Implications

The results of the present meta-analysis provide several implications for research and clinical practice. First, the results confirm the beneficial effects of taxing WM while keeping a memory or image in mind, thereby providing additional evidence for the effectiveness of eye-movements in EMDR therapy (Lee & Cuijpers, 2013). However, it still unclear whether more demanding tasks are beneficial, and more research directly comparing tasks with different WM demand is needed. Considering the fact that PTSD patients suffer from intrusive memories of several modalities (Ehlers et al., 2002), it would be beneficial to establish whether modality specific task would be more effective in tackling multimodal memories. Recently, modality-specific tasks have been proposed as a potential way to improve the effectiveness of EMDR therapy (see

Hornsveld et al., 2018). Unfortunately, not enough studies exploring the effect of modality-specific tasks could be included to be investigated as a moderator and in the meta-analysis. Furthermore, only a few studies (Kemps & Tiggemann, 2007; Kristjánsdóttir & Lee, 2011; Lilley et al., 2009; Matthijssen et al., 2017; Mertens et al., 2020a) have assessed the effect of modality specific dual-tasks and more research is warranted to test whether this could improve the effectiveness EMDR therapy.

These results with respect to positive memories and images have implications for interventions aimed at decreasing, for instance, unhealthy eating behavior, obesity and addiction. The results suggest that dual-task interventions therapy might not only be useful for negative memories, but also for positive mental images such as those involved in eating-related or addictive disorders (Müller, 2013). Furthermore, employing modality-specific tasks in dual-task interventions might be particularly useful in treating addictive disorders given that cravings are often maintained by sensory imagery (i.e., imagining smell or taste: Andrade et al., 2012; Littel et al., 2016). Nonetheless, more research is needed to establish the efficacy of dual-task interventions in addictive disorders and health interventions.

4.2. Limitations

There are several limitations to this meta-analysis which require some further attention. First, we were not able to obtain unpublished studies despite contacting several researchers. Thus, the results of the meta-analysis might be affected by publication bias, even though this is unlikely given that the funnel plots and *p*-curve analysis showed little evidence of publication bias.

Second, we calculated the average standard deviation instead of using the standard deviation from the difference scores. The average standard deviation is somewhat larger than the standard deviation of the difference scores due to the correlation between pre- and post-test scores, and thus produces a more conservative test. One could therefore argue that our meta-analysis represents an underestimation of the effectiveness of dual-task interventions. We calculate the standard deviation this way, because many studies did not report the required statistics to be able to calculate the standard deviation of the difference scores (see Footnote 3). In addition, calculating the average standard deviation allowed us to compare within-subjects and between-subjects studies, which is relevant to compare treated groups to untreated groups. Finally, our way of calculating the standard deviation produces a more stringent test and decreases the risk of obtaining an over-estimation of the effect. Given these considerations, we think that using the average standard deviation rather than the standard deviation of the difference scores provided a good alternative.

A third limitation was that for some questions we examined, only a limited amount of studies were available. This was the case dual-tasks effects on positive mental images, follow-up effects, and objective memory performance. Hence, it is important for future research to focus on aspects of this meta-analysis that need more evidence.

Fourth, another limitation is that this meta-analysis focused on fundamental laboratory research that included healthy or sub-clinical participants, and did not include studies focusing on clinical interventions. It remains unknown whether the results from this meta-analysis can be generalized to clinical populations. There is more room for a reduction in memory unpleasantness for clinical groups, and interventions in the clinic are of course more extensive than a laboratory model. As such, it may be expected that the effects observed for dual-task interventions in the clinic could be more pronounced. However, this most likely also depends on the conditions with which the dual-task interventions are compared and outcome measures that are used (e.g., memory ratings or symptoms scales). So far, there is inconclusive evidence with regard to the specific contribution of the dual-task component (i.e., making horizontal eye-movements) in symptom reduction in EMDR therapy (see Cuijpers et al., 2020).

Finally, one issue that was not considered in this meta-analysis is the

correlation between the two dependent variables that were investigated, emotionality and vividness ratings of the memories or mental images. Most studies within this field typically include both dependent variables. As can be seen in the results, reductions due to a dual-task intervention are found on both variables, although descriptively the effect sizes for vividness ratings were slightly larger than for emotionality ratings for both negative and positive emotional memories and images. Theoretically, the two measures are taken as separate constructs. It is often believed that dual-task interventions reduce the vividness of the emotional memories and images, and this subsequently causes a reduction in the emotionality of the memories and images (e.g., Andrade et al., 1997; Gunter & Bodner, 2008). However, the correlations between the two measures are typically moderate to strong (i.e., between 0.4 and 0.8; see Kemps & Tiggemann, 2007; van den Hout et al., 2010) and the question could be asked whether they indeed do capture two independent constructs. For the purpose of this meta-analysis, we treated the two measurements as two independent outcome measures in line with the convention in the literature. However, whether these measures indeed capture two different constructs requires further consideration using appropriate psychometric techniques.

4.3. Conclusions

In the current meta-analysis we investigated the effects of dual-task interventions on emotional memories and images. Overall, we found a substantial effect (i.e., approximately 6 to 12 points on a 100-point scale in the short term). Furthermore, publication bias analyses indicated little evidence for systematic bias in the literature. However, moderation analyses did not unequivocally support a WM theory interpretation of the effects of dual-tasks. Taken together, the effects of dual-tasks on emotional memories in the lab seem robust, but more research is required to determine whether and which specific factors moderate the effect.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.actpsy.2021.103424>.

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