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The Effectiveness of Dual-task Interventions for Modulating Emotional Memories in the

Laboratory: A Meta-Analysis

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Abstract

Persistent emotional memories and intrusive mental images can be weakened by executing a demanding dual-task during recollection of those images and memories. According to working memory (WM) theories, such dual-task interventions are effective because they limit the amount of 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 the current meta-analysis, we assessed k = 44 laboratory experiments investigating the effects of dual-task interventions on negative and positive memories. The effect was 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 = 10.83, 95% CI = [8.15, 13.52]; mean reduction positive images = 12.48, 95% CI = [6.73, 15.39]) and less emotional (mean reduction negative images = 11.34, 95% CI = [7.38, 15.30]; mean reduction positive images = 5.35, 95% CI = [2.15, 8.54]). Several moderators were tested and are discussed in the light of WM theories.

Keywords: Memories, Working Memory, Emotion, EMDR, dual-task

1. Introduction

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 memories can cause distress and interfere with everyday functioning, as is the case in Post-Traumatic Stress Disorder (PTSD) and other psychiatric disorders (e.g., obsessive-compulsive disorder) (Brewin, Gregory, Lipton, & Burgess, 2010). As such, emotional memories are an important target for optimizing current available treatments (e.g., Beckers & Kindt, 2017; Engelhard, McNally, & van Schie, 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 & Kemps, 2005), overeating (McManus & Waller, 1995), and relapse after drug abstinence (Shiffman et al., 1997). Not only negatively valenced memories, but also such positive mental images can lead to maladaptive behavior and may thus require consideration to achieve treatment success (May, Andrade, Panabokke, & Kavanagh, 2004; McClelland, Kemps, & Tiggemann, 2006).

1.1. Treating intrusive memories and mental imagery: EMDR therapy

Eye-Movement and Desensitization Reprocessing (EMDR) therapy is an evidence-based treatment for PTSD (Shapiro & Forrest, 2016). The core procedural element of EMDR therapy consists of bilateral stimulation, usually induced by having patients track the fingers of the therapist with their eyes. Initially, the introduction of EMDR therapy was met with skepticism (Lohr, Lilienfeld, Tolin, & Herbert, 1999). However, nowadays it is considered to

be an evidenced-based treatment for PTSD (National Collaborating Centre for Mental Health, 2005), for which it has comparable effects as standard cognitive-behavioral therapy (Seidler & Wagner, 2006; Van Etten & Taylor, 1998). Effectiveness of EMDR therapy for other psychiatric disorders than PTSD, such as addiction (Littel, van den Hout, & Engelhard, 2016; Markus, de Weert-van Oene, Woud, Becker, & DeJong, 2016; Zweben & Yeary, 2006), is not or less firmly established (Cuijper, van Veen, Sijbrandij, Yoder, & Cristea, 2020).

1.2. Studying how EMDR works: A laboratory model

One of the most controversial issues concerning EMDR therapy is its working mechanism. Several theories have been put forward (e.g., Gunter & Bodner, 2008), but the theory based on working memory (WM) (Andrade, Kavanagh, & Baddeley, 1997) has received most support. Baddeley and Hitch (1974) introduced WM to explain the relationship between long-term memory and flexible behavior. It encompasses a modality aspecific central executive and two modality-specific "slave" systems, namely the visuospatial sketchpad and the phonological loop. According to this theory, keeping a long-term memory or image in mind requires WM capacity. Likewise, executing eye-movement (as in EMDR therapy) or other WM demanding dual-tasks (e.g., playing Tetris; Holmes, James, Coode-Bate, & Deeprose, 2009) requires WM resources. So when an emotional memory is recalled while executing a dual-task, memory retrieval and the dual-task compete for the limited WM capacity. Presumably, this will diminish the vividness and emotional intensity of the memory. As a consequence, the memory can be reappraised and/or restored in long-term memory in a degraded fashion (e.g., Engelhard, McNally, & van Schie, 2019; van den Hout, Eidhof, Verboom, Littel, & Engelhard, 2014). WM theory is currently the most widely accepted theory for explaining the effects of EMDR and other WM interventions on emotional memories and images (Landin-Romero, Moreno-Alcazar, Pagani, & Amann, 2018).

To investigate this theory, laboratory models of dual-task interventions have been developed (Andrade et al., 1997; van den Hout, Muris, Salemink, & Kindt, 2001; Gunter & Bodner, 2008). Typically, they involve three phases: First, healthy participants recall a negative autobiographical memory and rate its vividness and emotionality. Second, one group of participants (i.e. the experimental 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 rate its vividness and emotionality. The typical finding from these studies is that participants in the experimental condition give lower ratings for the vividness and emotionality of their emotional memories and mental images compared to the control group at the post-test. However, in some studies, the results are weak and inconsistent across different dual-tasks (e.g., Mertens, Krypotos, et al., 2019).

1.3. Dual-tasks and emotional memories: Prior meta-analyses

In order to provide more decisive evidence for whether dual-tasks decrease the intensity of emotional memories, meta-analyses have been conducted. Particularly, three previous meta-analyses have partly addressed this issue. These meta-analyses examined whether the eye-movements (EM) component of EMDR therapy does indeed reduce the vividness and emotionality of emotional memories (Davidson & Parker, 2001; Lee & Cuijpers, 2013). However, the results of this meta-analyses are not consistent. Davidson and Parker (2001) showed no beneficial effect of the eye-movement component in EMDR therapy, but there were problems with their meta-analytic methods: they did not weigh each study in relation to the number of participants (Field & Gillett, 2010). In another meta-analysis on the effectiveness of EM, Lee and Cuijpers (2013), examined the effect sizes of 14 clinical trial studies and 10 laboratory studies published until 2012. They found that EM was effective in reducing the self-reported intensity of unpleasant memories. However, in a recent

meta-analysis focusing on EMDR therapy in 10 clinical dismantling studies, no differences were found for EMDR with our without the eye-movement component (Cuijpers et al., 2020). Hence, the evidence for a specific contribution of the eye-movement component in EMDR therapy is, based on primarily clinical studies, quite weak.

1.4. Extensions of the dual-task approach

In recent years, the laboratory research on the effectiveness of the dual-task approach has been extended, such as by also using other dual-tasks than horizontal eye movements (e.g., vertical eye movements, playing Tetris, counting backwards, attentional breathing; see Engelhard et al., 2019). We summarize the major extensions of the dual-task approach to reduce emotional memories and mental images here.

1.4.1. Effects on positive mental images. As mentioned, positive mental images can also lead to maladaptive and pathological behavior. Given that memories and imagery are important in the context of cravings (May, Andrade, Panabokke, & Kavanagh, 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, studies have indicated that dual-tasks also reduce the vividness and emotional intensity of positive mental images (Engelhard, van Uijen, & van den Hout, 2010; Hornsveld et al., 2011; Littel et al., 2016; Bartels, Harkins, Harrison, Beard, & Beech, 2018).

1.4.2. Variations of the dual-task load. According to the above-mentioned WM theory, more demanding dual-tasks should be more effective to modulate emotional memories. Several studies have investigated this issue by increasing the WM load of their dual-tasks and indeed found support for this hypothesis (Maxfield, Melnyk, & Hayman, 2008; van Veen et al., 2015), though this was not the case in all studies (Engelhard, van den Hout, & Smeets, 2011a; Mertens, Krypotos, et al., 2019).

Another prediction of the WM theory is that matching the modality of the dual-task (most commonly: visuo-spatial or auditive) to the modality of the emotional memory enhances intervention effects. Again, some studies found support for this hypothesis (Kemps, & Tiggemann, 2007; Kristjánsdóttir & Lee, 2011; Lilley, Andrade, Turpin, Sabin-Farrell, & Holmes, 2009), but other studies did not (Matthijssen, van Schie, & van den Hout, 2019; Mertens, Bouwman, Asmervik, & Engelhard, 2019).

1.4.3. Long-term effects. Another question is whether the memory effects persist beyond the experimental session, as predicted by the WM theory. The results are somewhat inconsistent. Some studies did not show an effect at follow-up (Kavanagh, Freese, Andrade, & May, 2001; Lee & Drummond, 2008), and other studies did (e.g., Asselbergs et al., 2018; Gunter & Bodner, 2008; Leer, Engelhard, & van den Hout, 2014). Thus, the dual-task interventions can change emotional memory over longer time spans remains unclear.

1.4.4. Extended interventions. The lab interventions using the dual-task approach are usually short (ranging from 60 seconds to 45 minutes), whereas interventions in the clinic usually consist of multiple sessions of at least one hour. For instance, guidelines recommend 8-12 weekly 90 min sessions of EMDR therapy (Committee on Treatment of Posttraumatic Stress Disorder, & Institute of Medicine, 2008). As such, some studies have investigated whether a more extensive intervention produces stronger effects. One study found that long term effects (see above) were only present with a longer intervention duration compared to a short intervention (Leer et al., 2014). Another study found that although immediate effects were found with a shorter intervention, the effect became stronger with more dual-task interventions (van Veen, van Schie, van de Schoot, van den Hout, & Engelhard, 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.

1.4.5. Impact of the type of memory. Most dual-task intervention studies have focused on negative autobiographical memories. A drawback of this is that the content of the memory is uncontrolled. Some studies have therefore shown participants aversive pictures, film clips, or virtual reality scenes (e.g., Cuperus, Laken, van den Hout, & Engelhard, 2016) and later asked them to recall those stimuli while performing the dual-task. 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. Indeed, research has demonstrated that self-relevant memories are more resistant to the effect of retrieval induced forgetting compared to non-personal relevant memories (Conway & Pleydell-Pearce, 2000; Neil Macrae & Roseveare, 2002). Furthermore, memories of high emotional valence are more strongly stored (McGaugh, 2000). Therefore, emotional autobiographical memories may be less malleable by dual-task interventions than memories created in the lab.

1.4.6. Varying control conditions. Another important aspect is the control condition. In laboratory studies, participants are usually asked to recall the emotional memory or image without doing a dual-task. This functions as a type of "imaginal exposure", which may render the memory less vivid and emotional (van Veen, van Schie, van de Schoot, van den Hout, & Engelhard, 2019). However, it can also result in memory rehearsal, which may increase memory vividness and emotionality. Additionally, control participants are sometimes instructed to stare at a fixation point while keeping the memory in mind (e.g., Engelhard et al., 2012, 2010; Smeets et al., 2012). However, a recent study demonstrated that keeping eyes stationary induced WM taxation (Lenoble, Janssen, & El Haj, 2018). Therefore, it is important to explore the effects of the control condition on memory vividness and emotionality.

1.4.7. Effects on objective memory performance. It has been proposed that the reason EMDR is effective, and shows long-term effects, is because of a loss in memory detail (Maxfield et al., 2008; van den Hout, Bartelski, & Engelhard, 2013). This proposition cannot be investigated using only self-report data, which are sensitive to spurious factors such as experimental demand (Orne, 1962). Rather, investigating this hypothesis requires behavioral measures of memory performance (e.g., reaction times or memory accuracy), although this does not clarify whether an intervention updates the original emotional memory or creates a competing inhibitory representation (Beckers & Kindt, 2017).

1.5. Goals of the current meta-analysis

We aimed to address the above-mentioned issues by conducting an updated meta-analysis of laboratory research examining the effects of on dual-task interventions in healthy participants. Like the earlier meta-analyses (Cuijpers et a;., 2020; Davidson & Parker, 2001; Lee & Cuijpers, 2013), we aimed to establish the effect of dual-tasks on negative emotional memories. However, our meta-analysis is specifically focused on laboratory rather than clinical studies, which have studied the effects of (variations of) dual-tasks in more controlled settings. In addition, we extended these meta-analyses by examining (1) whether dual-tasks affect positive mental images; (2) whether more cognitively demanding dual-tasks and dual-tasks that match the modality of emotional memories are more effective; (3) whether longer intervention duration strengthens the effects; (4) whether effects persist beyond the experimental session; (5) whether the control conditions show effects; and (6) whether dual-task interventions impede objective memory performance.

2. Method

2.1. Pre-registration

The meta-analysis was pre-registered on Open Science Framework (https://osf.io/q3xdu/). We followed the pre-registration with several deviations. First,

modality-specificity was not included as a moderator due to the lack of available studies

investigating modality specific dual-tasks. Second, due to a small number of 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 in order to be able to

conduct moderation analyses for the control condition as well. 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, Liberati, Tetzlaff, Altman, & Group, 2009). Papers were identified by searching

PubMed, PsychInfo, EMBASE and Web of Science. The full search strategy can be found in

Figure 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 final search was conducted on November 7[th,] 2018 and the screening was conducted in

November and December of 2018. 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):

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 experimental 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 on Visual Analogue Scales (VASs) or Subjective Units of Distress

   (SUD) scale with a range of 0-10 or 0-100 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

   performance related to stimuli that were retrieved during the dual-task. Because we

   investigated pre-post changes within each condition, studies were included if they

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[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).

entailed an appropriate control condition or experimental condition or both. Studies using only a post-score measurement were excluded.

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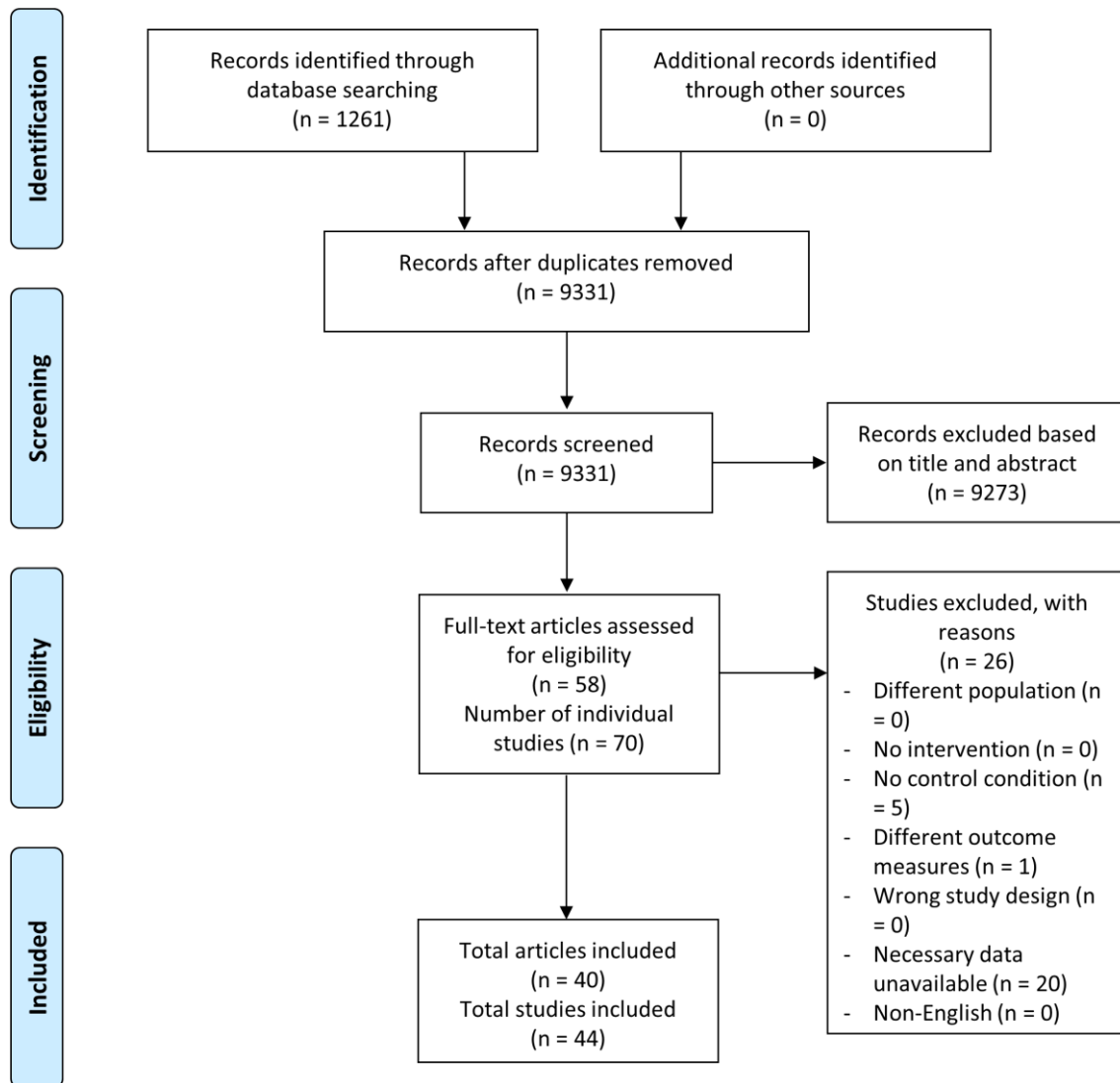
Figure 1. Flow diagram illustrating the search strategy and exclusion criteria used.

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 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 experimental group

and control group, mean age, number of males and females, and healthy or subclinical

sample. For interventions characteristics, the following information was extracted:

intervention duration, number of blocks, number of experimental 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, Smith,

Schneider, & Minder, 1997) and a *p*-curve analysis (Simonsohn, Nelson, & Simmons, 2014a).

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, Hedges, Higgins, & Rothstein,

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, & Simmons, 2014b). The *p*-curve entails the distribution of

statistically significant *p*-values for a specific sample of studies (alpha < .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*-vales between .025 and

.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 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 0-10 or 0-100. We decided to use the difference between

post-score and pre-score means as the effect size, because raw change scores are easier to

interpret than standardized metrics (Bond, Wiitala, & Dan Richard, 2003). Most studies used

scales ranging from 0-100, 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

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[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.

calculated by taking the average standard deviation from the pre and post-ratings, given by:

SD_pre + SD_post / 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, Thompson, Deeks, & Altman, 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 4 meta-analyses were conducted for vividness ratings and 4 were conducted for emotionality ratings (pretest-posttest mean differences and pretest-follow-up test mean differences in each group - experimental or control).

With regard to positive memories, there were fewer studies and only one included follow up measures. Therefore, 2 meta-analyses were conducted for vividness ratings and 2 were conducted for emotionality ratings pre-post mean difference in the experimental group and in the control group).

2.8. Moderator Variables

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[4] We acknowledge that using the standard deviation from the difference score would be more appropriate for the design of the included experiments (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).

Based on various methods used in the studies, we decided to include 6 moderator variables:

1. **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 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.

2. **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 Houtet al. (2001) (4 blocks of 24s) as a reference for 'medium'. Studies using a shorter duration were grouped as 'short' and those using a longer duration were grouped as 'long'.

3. **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. **Type of control condition:** The studies used slightly different conditions, which may differ in cognitive load. For example, participants were asked to retrieve the memory while staring at a stationary dot or without a fixation target (free gaze; though usually focused on a blank computer screen). Because the former is more taxing than the latter (Lenoble et al., 2018), we grouped stationary eyes conditions as high load and free gaze as low load.

5. **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.

3. Results

3.1. Description of Studies

The final sample consisted of 44 experiments with a combined number of 2364

participants. Table 2 depicts an overview of relevant experimental characteristics. Six out of

the 44 studies investigated objective memory performance, leaving 38 studies for the main

analysis. Most studies tested a healthy sample of undergraduate students and five out of 44

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 10 out of 44 studies, it focused on memory of an emotional film clip or picture that had

been shown in the lab.

Thirty-six studies used the EM dual-task and 18 studies (also) used 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 27 studies, participants were asked to fixate

their eyes, and in 17 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, 21 studies used the standard four blocks of 24 seconds (separated by 10s breaks);

duration was shorter for 8 studies and longer for 15 studies. Only six of 44 studies included a

follow-up assessment, whereas the other studies only used a post-test shortly after the

intervention.

Table 1. *Study characteristics of the included studies.*

Author(s) and Year	Type of memory	Dual-task	Control condition	Duration	Follow-up	Sample size
Andrade et al. (2012) Exp. 2	Memory of chocolate	Clay modeling & counting	NA	10 min	NA	87
Asselberg et al. (2018) Exp. 2	Memory of film	Computer game	Recall only	20 block of 24s	1 week	120
Bartels et al. (2018)	Autobiographical	EM	Eyes fixed	4 blocks of 24s	NA	80
Barrowcliff et al. (2004)	Autobiographical	EM	Eyes fixed	2 blocks of 25s	NA	80
Cuperus et al. (2016) Exp. 2	Memory of VR game	Shape-sorter task	Blindfolded	4 blocks of 24s	NA	34
Cuperus et al. (2019)	Memory of VR game	Shape-sorter task	NA	4 blocks of 24s	NA	84
Devilly & Brown (2011)	Recollection of words	EM	Eyes closed	3 blocks of 20-30s	NA	48
Engelhard et al. (2011a)	Autobiographical	EM	Eyes fixed	8 blocks of 24s	NA	37
Engelhard et al. (2011b)	Autobiographical	Subtraction	Eyes fixed	4 blocks of 24s	NA	80
Engelhard et al. (2010a)	Autobiographical	EM	Eyes fixed	4 blocks of 24s	NA	60
Engelhard et al. (2010b)	Autobiographical	EM and Tetris	Eyes fixed	4 blocks of 24s	NA	28
Homer and Deeprose (2018)	Autobiographical	EM	Eyes fixed	3 blocks of 60s	NA	26
Homer et al. (2016)	Autobiographical	EM and tones	NA	3 blocks of 60s	NA	36
Hornsveld et al. (2011)	Autobiographical	EM	Eyes fixed	5 blocks of 10s	NA	53
Houben et al. (2018)	Autobiographical	EM	Eyes fixed	4 blocks of 24s	NA	82
Kearns & Engelhard (2015)	Memory of script	EM	Eyes fixed	4 blocks of 24s	NA	34
Kristjansdottir & Lee (2011)	Autobiographical	EM and counting	Eyes closed	1 block of 60s	NA	36
Lee & Drummond (2008)	Autobiographical	EM	Eyes fixed	45 min	1 week	47
Leer et al. (2013)	Memory of film	EM	Eyes fixed	4 blocks of 24s	NA	63
Leer et al. (2014)	Autobiographical	Short EM and long EM	Eyes fixed	4 or 8 blocks of 24s	24h	73
Leer et al. (2017) Exp. 1	Memory of picture	EM	Eyes fixed	1 block of 24s	NA	26
Leer et al. (2017) Exp. 2	Autobiographical	EM	Eyes fixed	1 block of 24s	NA	52
Littel et al. (2017)	Autobiographical	EM	Recall only	6 blocks of 24s	24h	56
Markus et al. (2016)	Autobiographical	EM	Eyes fixed	12 blocks of 30s	1 week	47
Maxfield et al. (2008) Exp. 1	Autobiographical	EM	Eyes fixed	10 blocks of 10s	NA	25
Maxfield et al. (2008) Exp. 2	Autobiographical	Fast EM and slow EM	Eyes fixed	10 blocks of 10s	1 week	36
Merckelbach et al. (1994)	Memory of picture	EM & finder tapping	NA	4 blocks of 24s	NA	39
Mertens et al. (2019)	Autobiographical	EM & letter identification	Eyes fixed	4 blocks of 24s	NA	100
Onderdonk & van den Hout (2016)	Autobiographical	EM and CVI	Eyes fixed	3 blocks of 24s	NA	39
Patel & McDowall (2016) Exp. 1	Autobiographical	EM	Eyes fixed	4 blocks 24s	NA	31

Patel & McDowall (2016) Exp. 2	Autobiographical	Fast EM and slow EM	Eyes fixed	4 blocks 24s	NA	30
Phaf (2017)	Memory of words	EM	Eyes fixed	1 block of 30s	NA	40
Schubert et al. (2011)	Autobiographical	Fixed EM and varied EM	Eyes closed	45 min	NA	62
Slofstra et al. (2016) Exp. 1	Autobiographical	Attentional breathing	Recall only	4 blocks of 24s	NA	48
Smeets et al. (2012)	Autobiographical	EM	Eyes fixed	4 blocks of 24s	NA	61
Tsai & McNally (2014)	Memory of film	Matching-to-sample task	Recall only	NA	NA	80
van den Hout et al. (2013)	Memory of picture	EM	Eyes fixed	13 blocks of 40s	NA	32
van den Hout et al. (2010)	Autobiographical	Counting	Recall only	1 block of 90s	NA	41
van den Hout et al. (2001)	Autobiographical	EM and finfer tapping	Recall only	4 blocks of 24s	NA	60
van den Hout et al. (2011a) Exp. 4	Autobiographical	EM and tones	Recall only	4 blocks of 24s	NA	54
van den Hout et al. (2011b) Exp. 1	Autobiographical	EM & attentional breathing	Recall only	4 blocks of 24s	NA	36
van den Hout et al. (2011b) Exp. 2	Autobiographical	EM & attentional breathing	Eyes fixed	4 blocks of 24s	NA	33
van Schie et al. (2016)	Autobiographical	Fast EM and slow EM	Recall only	4 blocks of 24s	NA	66
van Veen et al. (2016)	Autobiographical	EM	Eyes fixed	8 blocks of 24s	NA	10

Notes: VR = virtual reality; EM = eye-movements; CVI = changing visual input.

3.2. Main Analysis

3.2.1. Short-term effects of dual-tasks on negative memories. The mean difference between the pre-intervention and post-intervention vividness ratings was 10.83 (95% CI: [8.15, 13.52]) with substantial heterogeneity (I^2 = 79.46%, p < .001), indicating that taxing working memory while keeping an emotional memory in mind, on average, reduces the vividness of that memory with 10.83 points on a 0-100 VAS. Figure 2 shows a forest plot with the mean difference scores.

Likewise, the mean difference between pre-intervention and post-intervention emotionality ratings was 11.34 (95% CI: [7.29, 15.39]) with considerable heterogeneity (I^2 = 91.06%, p < .001), indicating that taxing working memory while keeping an emotional memory in mind, on average reduces the emotionality of that memory with 11.34 points on a 0-100 VAS/SUD[5] scale. Figure 3 shows a forest plot with the mean emotionality difference scores.

3.2.2. Short-term effects of dual-tasks on positive mental images. The mean difference between pre and post-intervention vividness ratings of positive images was 12.48 (95% CI: [7.26, 17.74]) with substantial heterogeneity (I^2 = 68.49%, p < .01), indicating that the dual-task intervention, on average, reduces vividness of positive mental images with 12.48 points on a 0-100 VAS. The mean difference between pre-score and post-score for emotionality of emotional images was 5.34 (95% CI: [2.15, 8.54]) with no heterogeneity The mean vividness and emotionality difference scores for positive memories with 95% confidence intervals are included in the Supplementary Materials (S.3).

3.2.3. Long-term effects of dual-tasks on negative memories. Six 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 mean difference between pre-intervention and

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[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.

follow-up vividness ratings of negative memories was 21.01 (95% CI: [12.16, 29.86]) with

considerable heterogeneity ($I^2 = 90.47\%$, $p < .001$), indicating that the dual-task intervention,

on average, reduces the vividness of negative memories with 21.01 points on a 0-100 VAS.

Likewise, the mean difference between pre-score and follow-up-score for emotionality of

negative memories was 29.20 (95% CI: [15.88, 42.52]) with considerable heterogeneity ($I^2 =$

96.83%, $p < .001$). The difference scores for vividness and emotionality with 95% confidence

intervals are included in the Supplementary Materials (S.4).

 3.2.4. Short-term effects of the control condition on negative memories. The mean

difference between pre and post-intervention vividness ratings was 0.41 (95% CI: [-2.00,

2.81]) with substantial heterogeneity ($I^2 = 70.04\%$, $p < .001$), indicating no change in memory

vividness in the control condition (Figure 4 shows the mean vividness difference scores).

 The mean difference for emotionality ratings was 3.66 (95% CI: [0.28, 7.03]) with

substantial heterogeneity ($I^2 = 84.14\%$, $p < .001$), indicating that there is a small but

significant decrease in emotionality scores from before to after the intervention (Figure 5

shows the mean emotionality difference scores).

 3.2.5. Short-term effect of the control condition on positive mental images. The

mean difference between pre-intervention and post-intervention for vividness was -1.6 (95%

CI: [-4.65, 1.45]) with low heterogeneity ($I^2 = 3.98\%$), indicating no difference in vividness

scores. Likewise, the mean difference for emotionality was -0.47 (95% CI: [-3.61, 2.68]) with

no heterogeneity, indicating no difference over time. The mean vividness and emotionality

difference scores with 95% confidence intervals are included in the Supplementary Materials

(S.3).

 3.2.6. Long-term effects of the control condition. The mean difference between

vividness ratings of negative memories before the intervention and at the follow-up was 16.51

(95% CI: [7.73, 25.29]) with substantial heterogeneity ($I^2 = 88.99\%$, $p < .001$), indicating a

decrease with 16.51 on a 0-100 VAS in the control condition. Likewise, the mean difference

between pre-intervention and follow-up emotionality scores of negative memories was 21.10

(95% CI: [11.42, 30.77]) with considerable heterogeneity ($I^2 = 91.27\%$, $p < .001$). The mean

vividness and emotionality difference scores with 95% confidence intervals are included in
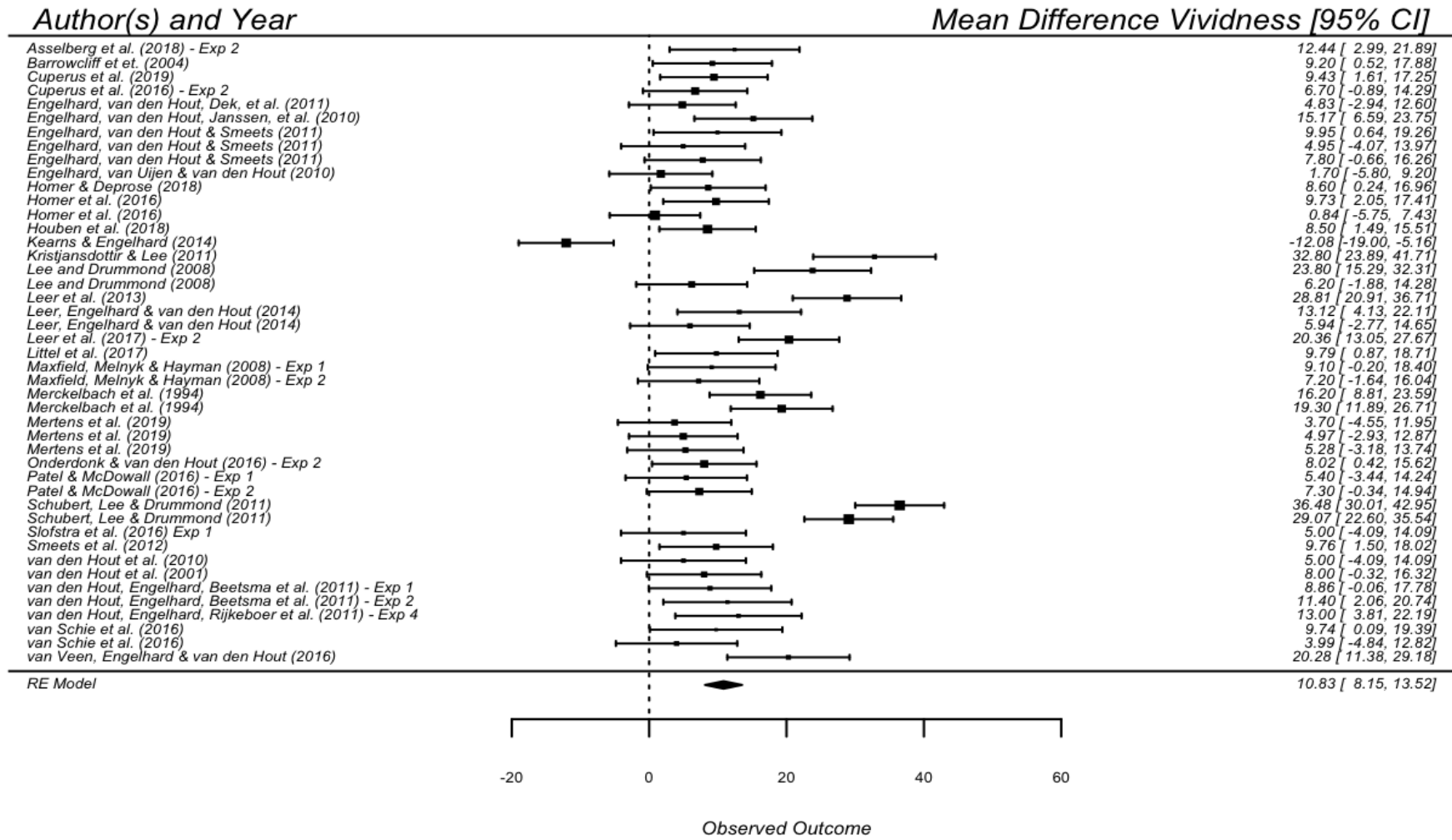
the Supplementary Materials (S.4).

Author(s) and Year	Mean Difference Vividness [95% CI]
Asselberg et al. (2018) - Exp 2	12.44 [2.99, 21.89]
Barrowcliff et et. (2004)	9.20 [0.52, 17.88]
Cuperus et al. (2019)	9.43 [1.61, 17.25]
Cuperus et al. (2016) - Exp 2	6.70 [-0.89, 14.29]
Engelhard, van den Hout, Dek, et al. (2011)	4.83 [-2.94, 12.60]
Engelhard, van den Hout, Janssen, et al. (2010)	15.17 [6.59, 23.75]
Engelhard, van den Hout & Smeets (2011)	9.95 [0.64, 19.26]
Engelhard, van den Hout & Smeets (2011)	4.95 [-4.07, 13.97]
Engelhard, van den Hout & Smeets (2011)	7.80 [-0.66, 16.26]
Engelhard, van Uijen & van den Hout (2010)	1.70 [-5.80, 9.20]
Homer & Deprose (2018)	8.60 [0.24, 16.96]
Homer et al. (2016)	9.73 [2.05, 17.41]
Homer et al. (2016)	0.84 [-5.75, 7.43]
Houben et al. (2018)	8.50 [1.49, 15.51]
Kearns & Engelhard (2014)	-12.08 [-19.00, -5.16]
Kristjansdottir & Lee (2011)	32.80 [23.89, 41.71]
Lee and Drummond (2008)	23.80 [15.29, 32.31]
Lee and Drummond (2008)	6.20 [-1.88, 14.28]
Leer et al. (2013)	28.81 [20.91, 36.71]
Leer, Engelhard & van den Hout (2014)	13.12 [4.13, 22.11]
Leer, Engelhard & van den Hout (2014)	5.94 [-2.77, 14.65]
Leer et al. (2017) - Exp 2	20.36 [13.05, 27.67]
Littel et al. (2017)	9.79 [0.87, 18.71]
Maxfield, Melnyk & Hayman (2008) - Exp 1	9.10 [-0.20, 18.40]
Maxfield, Melnyk & Hayman (2008) - Exp 2	7.20 [-1.64, 16.04]
Merckelbach et al. (1994)	16.20 [8.81, 23.59]
Merckelbach et al. (1994)	19.30 [11.89, 26.71]
Mertens et al. (2019)	3.70 [-4.55, 11.95]
Mertens et al. (2019)	4.97 [-2.93, 12.87]
Mertens et al. (2019)	5.28 [-3.18, 13.74]
Onderdonk & van den Hout (2016) - Exp 2	8.02 [0.42, 15.62]
Patel & McDowall (2016) - Exp 1	5.40 [-3.44, 14.24]
Patel & McDowall (2016) - Exp 2	7.30 [-0.34, 14.94]
Schubert, Lee & Drummond (2011)	36.48 [30.01, 42.95]
Schubert, Lee & Drummond (2011)	29.07 [22.60, 35.54]
Slofstra et al. (2016) Exp 1	5.00 [-4.09, 14.09]
Smeets et al. (2012)	9.76 [1.50, 18.02]
van den Hout et al. (2010)	5.00 [-4.09, 14.09]
van den Hout et al. (2001)	8.00 [-0.32, 16.32]
van den Hout, Engelhard, Beetsma et al. (2011) - Exp 1	8.86 [-0.06, 17.78]
van den Hout, Engelhard, Beetsma et al. (2011) - Exp 2	11.40 [2.06, 20.74]
van den Hout, Engelhard, Rijkeboer et al. (2011) - Exp 4	13.00 [3.81, 22.19]
van Schie et al. (2016)	9.74 [0.09, 19.39]
van Schie et al. (2016)	3.99 [-4.84, 12.82]
van Veen, Engelhard & van den Hout (2016)	20.28 [11.38, 29.18]
RE Model	10.83 [8.15, 13.52]

Figure 2. Forrest plot for studies comparing vividness ratings of negative memories before and after the dual task intervention (experimental group).
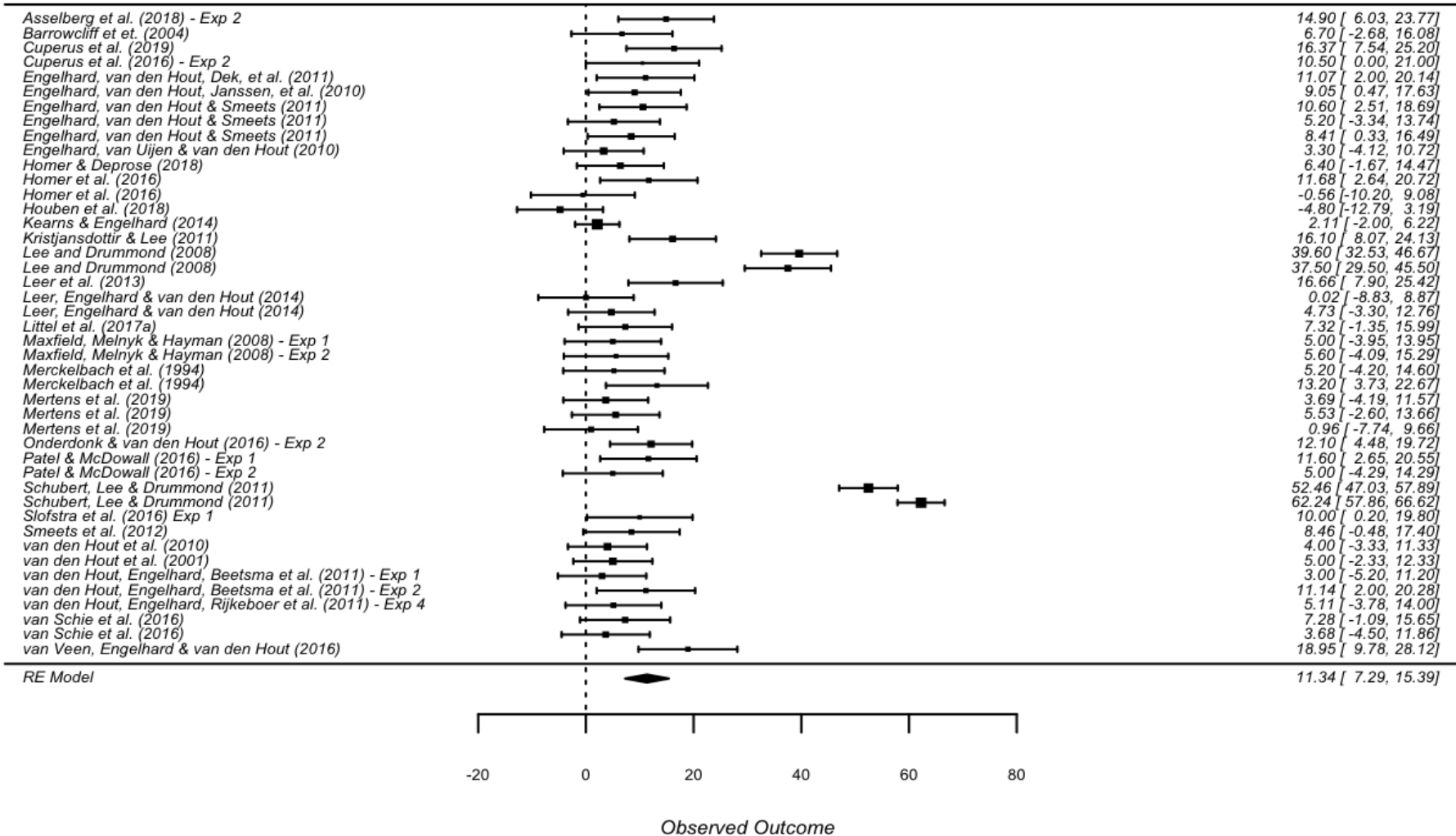
Figure 3. Forrest plot for studies comparing emotionality ratings of negative memories before and after the dual-task intervention (experimental group).
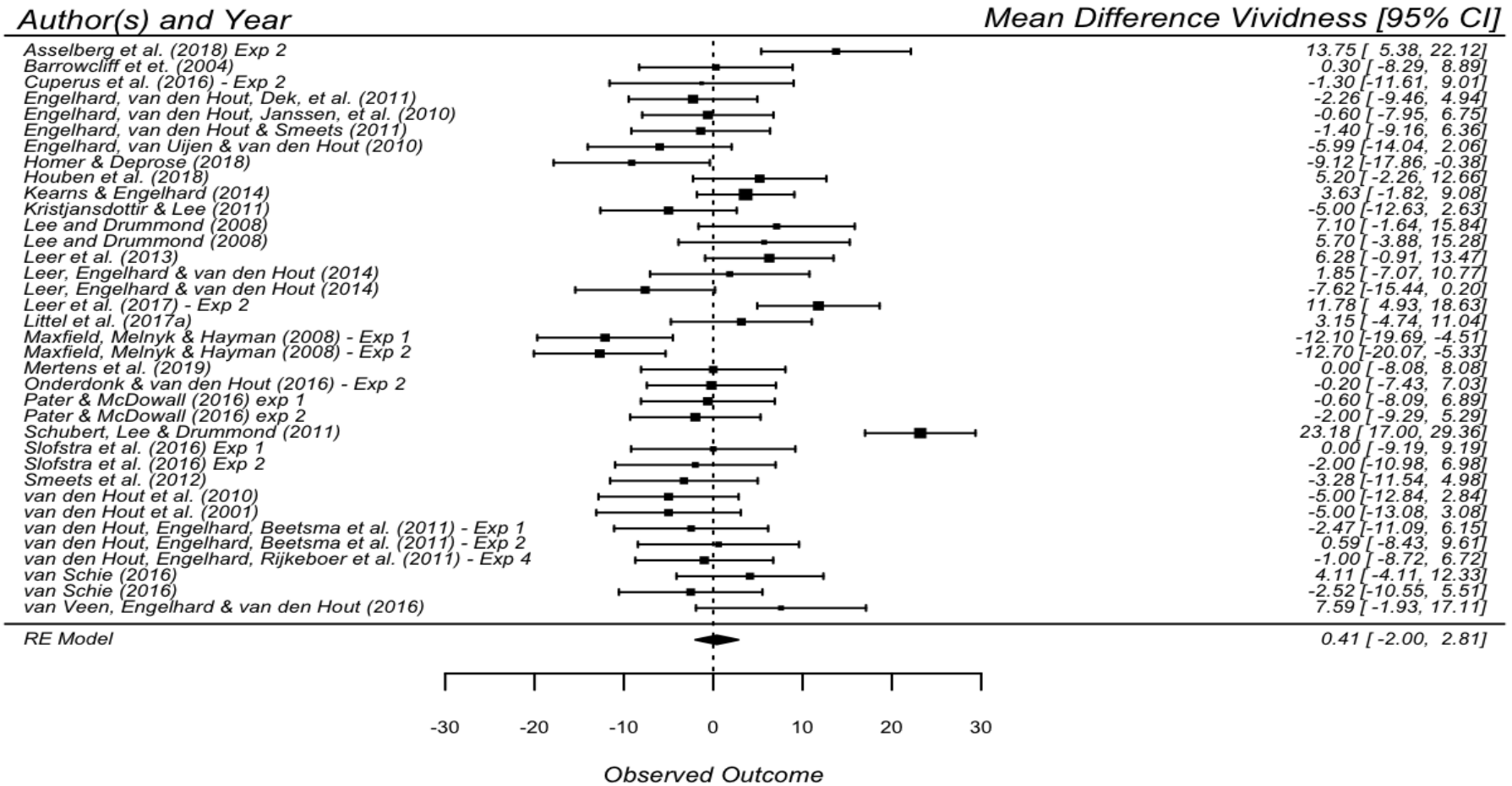
Figure 4. Forrest plot for studies comparing vividness ratings of negative memories before and after keeping the negative memory in mind (control group).
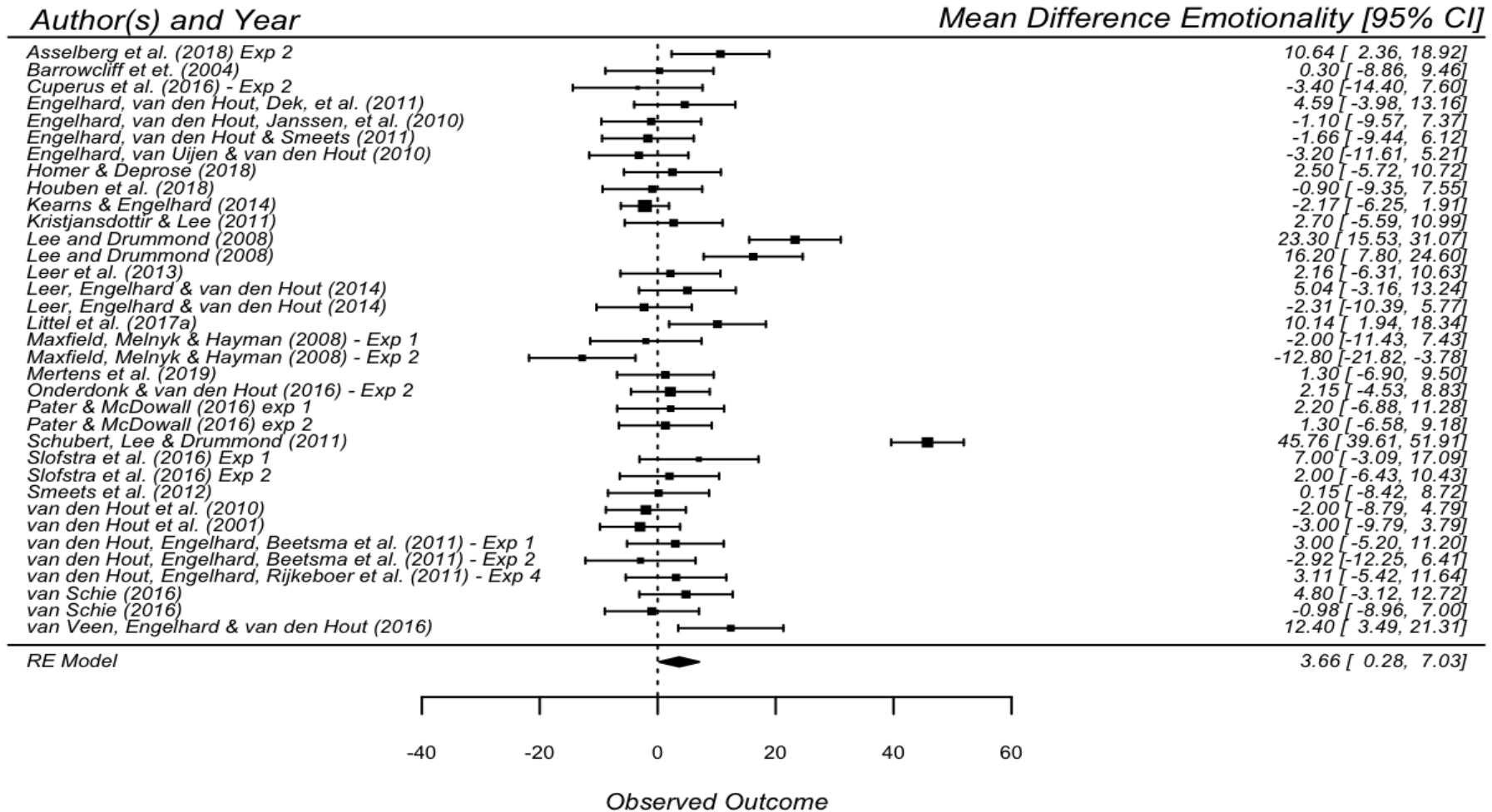
Figure 5. Forrest plot for studies comparing emotionality ratings of negative memories before and after keeping the negative memory in mind (control group).

3.3. Moderator Analyses

Two studies used a procedure in which participants continued to engage in EM until their SUD rating was close to zero (Lee & Drummond, 2008; Schubert, 2011). This procedure resembles EMDR therapy, but it differs from the other studies in this meta-analysis, which used fixed intervention durations. The decreases in memory vividness and emotionality ratings were larger for these two studies (see Figures 2 and 3). Specifically, for vividness, the mean difference from the mean was 1.24 SDs for Lee and Drummond's (2008) study and 2.23 SDs for Schubert et al.'s (2011) study. For emotionality, these SDs were 2.15 for Lee and Drummond's (2008) study and 3.31 for Schubert et al.'s (2011) study. We therefore examined the moderators with and without these two studies (following the leave-one-out method; Viechtbauer & Cheung, 2010). In relation to the first four moderators (difficulty of task, type of memory, duration of intervention and fixation or free gaze), intervention duration emerged as significant moderator for emotionality ratings within the experimental condition. Longer intervention duration was more effective in decreasing emotionality compared to medium and short duration. However, when the two studies were removed, this moderator was no longer significant. The other moderators were not significant for vividness or emotionality within the experimental condition or the control condition (Table 2 shows the test statistics).[6]

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[6] Additionally, we included lab and publication year as moderator in the analyses. For the experimental condition, the lab was a significant moderator for both vividness and emotionality, but not when the studies by Lee and Drummond (2008) and Schubert et al. (2011), were excluded, which were from the same lab. For the control condition, the lab was a significant moderator for vividness, also when these two studies were excluded, and for emotionality, but only when all studies were included. Publication year was significant for both vividness and emotionality 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).

Table 2. *Statistical details relating to the moderator analyses* .

Factor	Df	QM (estimate)	*p*
Experimental condition			
Vividness			
Difficulty of dual-task	2	0.32	.853
Intervention duration	2	1.62	.444
Type of memory	1	0.03	.857
Emotionality		0.35	.951
Difficulty of dual-task	2		
Intervention duration*	2	8.60	**.035**
Type of memory	1	0.01	.999
Control condition			
Vividness			
Fixation or free gaze	1	0.66	.416
Emotionality			
Fixation or free gaze	1	0.18	.669

Note: Moderation by this factor was no longer significant, $Q(2) = 1.16$, $p = .559$, when the studies by Lee and Drummond (2008) and Schubert et al. (2011) were excluded. These can be considered outliers due to their different procedure (i.e., continuation of the intervention until distress ratings reached zero).

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 (26 out of 44) did not exclude participants with knowledge about how EMDR works, even though such knowledge may facilitate expectation effects. Yet, so far only two studies have examined the role of expectations in EMDR (Gosselin & Matthews, 1995; Littel et al., 2017). Though both 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, Nelson, &

Simonsohn, 2011). Only one of 44 studies was pre-registered (Mertens, Krypotos, et al.,

2019). (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' behavior; e.g., Doyen, Klein, Pichon, &

Cleeremans, 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 44 studies (van Schie, van Veen,

Engelhard, Klugkist, & van den Hout, 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). 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 only significant for emotionality in the

experimental condition (see Table 3). Surprisingly, the Trim-and-Fill method showed that the

missing studies were on the opposite side than what would be expected with publication bias

(Borenstein et al., 2009), meaning that when the missing studies were imputed the average

effects increased instead decreased. However, as mentioned in the moderator analysis, two

studies by Schubert et al. (2011) and Lee and Drummond (2008) were outliers due to

procedural differences. When these studies were excluded, the funnel plot showed missing

studies on the left side of the average mean, which suggests publication bias (see Figure 6).

Nevertheless, the average mean difference for emotionality within the experimental condition

after correcting for funnel plot asymmetry was still reliable (M = 4.44, 95% CI: [2.55, 6.33], *p* < .001).

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 25 significant (< .05) *p*-values for vividness and 16 for emotionality. The *p*-curve results showed clear evidential value for both vividness (right skew test: Z = -8.42, p < .0001; estimated power of the studies = 89%; 90% CI [76%, 95%]) and emotionality (right skew test: Z = -5.33, p < .0001; estimated power of the studies = 76%; 90% CI [59%, 91%]). Hence, both *p*-curves showed strong evidence for the effects of the dual-task interventions on memory emotionality and vividness ratings. Figures 7 and 8 show the *p*-curves. The associated disclosure table (see Simonsohn et al., 2014a) is included in the Supplementary Materials (S.5).

Table 3. *Egger's regression test for funnel plot asymmetry.*

Factor	*z*	*P*
Experimental		
Vividness	-0.80	.422
Emotionality*	-3.56	**< .001**
Control		
Vividness	-1.02	.309
Emotionality	-1.27	.204

Note: When the studies by Lee and Drummond (2008) and Schubert et al. (2011) were excluded, the funnel plot asymmetry was still significant, *z* = 2.22, *p* = .027, but in the opposite direction. Most likely, this was due to extreme effects in these two studies, shifting the distribution of the funnel plot to the right. Exclusion of these two studies resulted in significant funnel plot asymmetry, indicating missing studies on the left side (i.e., missing studies with smaller effect sizes).
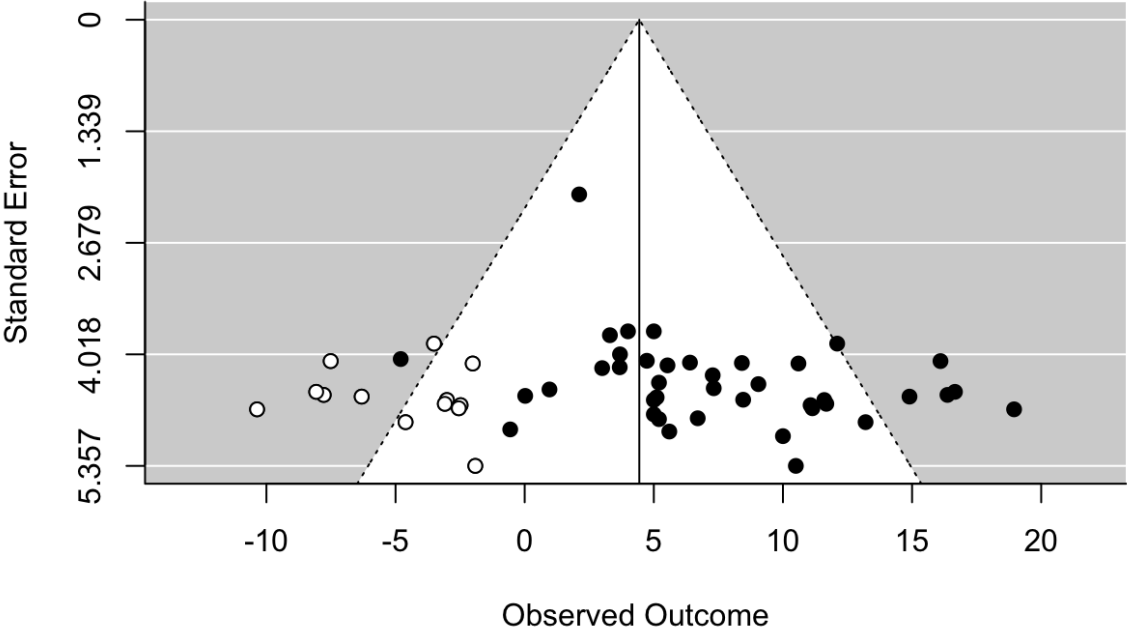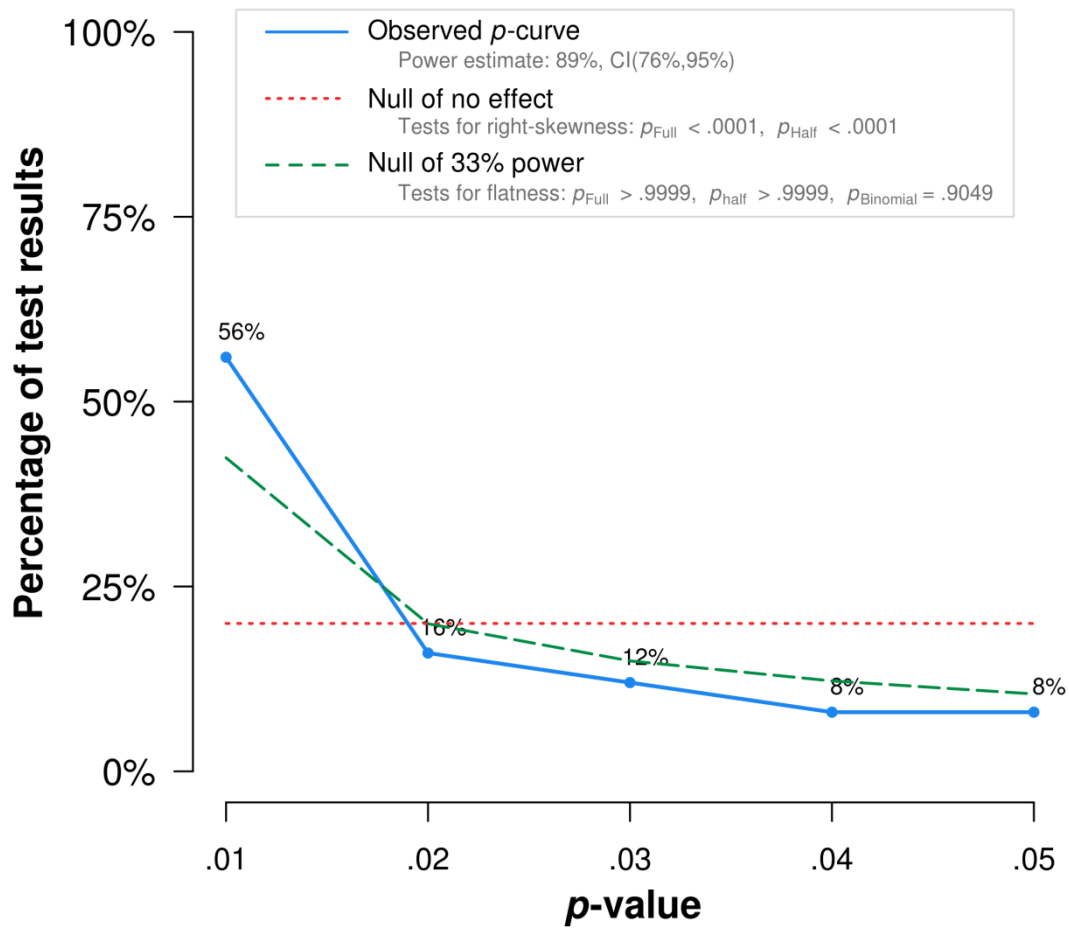
Figure 6. Trim and fill test for funnel plot with average emotionality ratings (experimental condition) against the standard error with two outliers excluded. Black circles represent the studies included in the present meta-analysis and white circles represent imputed missing studies.

Note: The observed p-curve includes 25 statistically significant ($p < .05$) results, of which 20 are $p < .025$. There were 11 additional results entered but excluded from p-curve because they were $p > .05$.

Figure 7. *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).
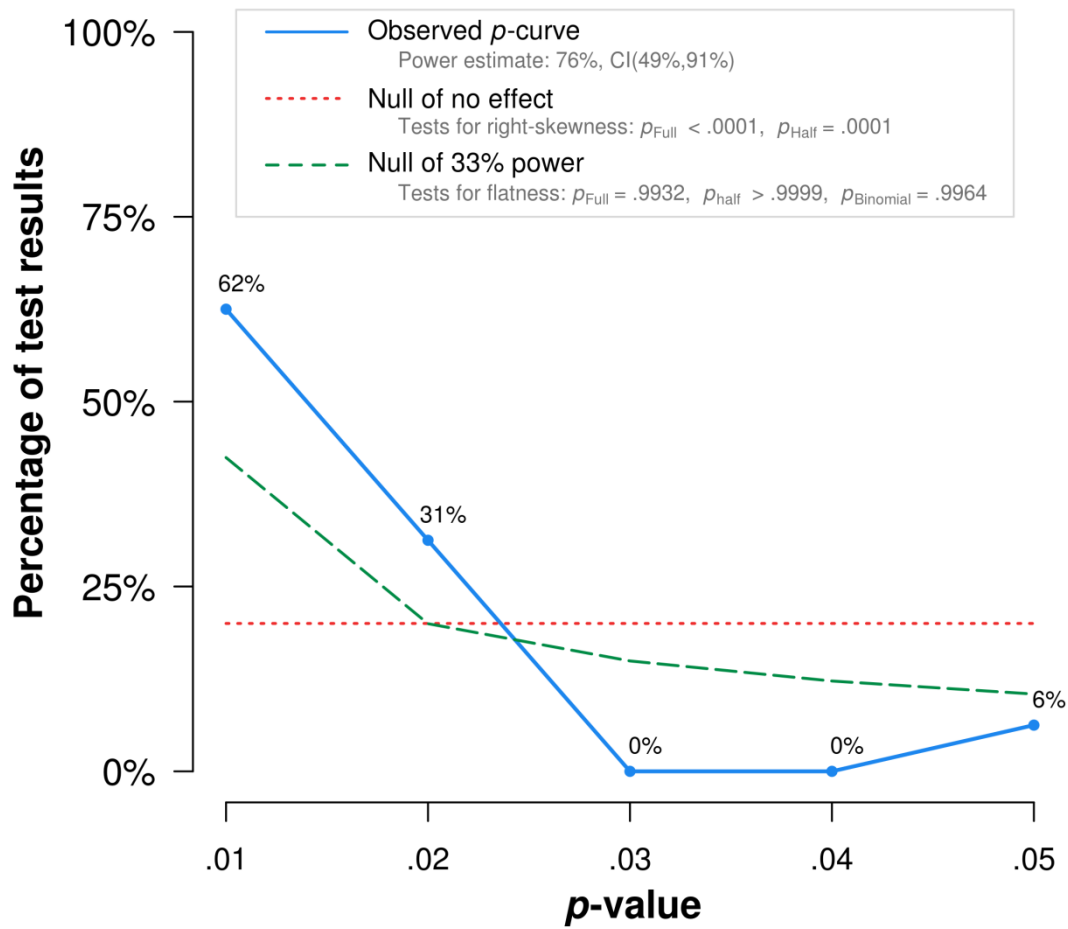
Note: The observed *p*-curve includes 16 statistically significant (*p* < .05) results, of which 15 are *p* < .025. There were 15 additional results entered but excluded from *p*-curve because they were *p* > .05.

Figure 8. *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).

3.6. Objective Memory Performance

Due to the large variety in methods and low number of studies, the effect of the dual-task intervention on objective memory performance will be reviewed rather than analyzed. Table 4 shows the study characteristics and conclusions. Six experiments were included: four of them showed memory impairment in the dual-task condition, compared to the control condition, as predicted by the theory; one experiment found the opposite (memory enhancement), and one

study did not find clear evidence. So these studies provide some preliminary evidence that

objective memory performance may be impaired after a dual-task intervention, which is

consistent with the working memory theory of the dual-task intervention. This may have

important legal implication for the treatment of victims using EMDR (i.e., their witness

statement could be considered unreliable; see Houben, Otgaar, Roelofs, & Merckelbach,

2018). However, such conclusions are very premature at this moment given the limited

available evidence (for a recent failed replication see: van Schie & Leer, 2019). Clearly this is

a topic that awaits more future research.

Table 4. *Summary of studies investigating objective memory performance after a WM intervention.*

Author(s) and Year	Procedure	Outcome measure	Conclusion	Sample Size
Devilly & 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 EM condition and control condition on number of inaccurate recalls.	48
Houben, Otgaar, Roelofs, et al. (2018)	Participants kept a memory of a film clip depicting a car crash in mind while performing EM or not. Misinformation was later induced.	Endorsement of misinformation measured by a recognition task with true and false answer options	Participants in the EM condition endorsed misinformation more than the participants in the control condition. Participants in the control condition had more correct answers	82
van den Hout, Bartelski, & Engelhard (2013)	Participants were shown two pictures and recalled one image while performing EM or merely recalled one image. Next, participants were presented with fragments from the images and asked to indicate whether it belonged to the two pictures shown before.	The time it took to decide whether the fragment was seen before or not.	For the EM condition, and not for the control group, participants were slower to decide whether the fragment belonged to the picture that was held in mind compared to the picture not held in mind	32
Leer, Engelhard, Lenaert, et al. (2017) – Exp 1	The procedure was the same as by van den Hout, Bartelski, & Engelhard (2013).	The time it took to decide whether the fragment was seen before or not.	The results showed that reaction times were slower for the EM condition compared to the control condition.	27
Leer, Engelhard, Lenaert, et al. (2017) – Exp 2	Participants were presented with a male face which was paired with a shock. Next, they recalled the image while performing EM or not. Next, participants were presented with male faces resembling the original male face.	Shock expectancy when presented with the images resembling the original image as measured by self-report and skin conductance.	It was found that participants who engaged in EM were more likely to expect shock when presented with the pictures resembling the image, compared to the no EM group.	52
Phaf (2017)	Participants were either instructed to perform EM while recalling a list of words or to merely recall the words.	Number of words correctly recalled	The results showed that for participants in the EM condition, there was an enhancement of recollections, compared to the participants in the control group.	40

Tsai & McNally (2014)	The participants recalled an aversive film clip while performing a dual-task or not. The dual-task was a picture sorting task with either positive, negative or neutral pictures.	Details remembered from the film clip.	The results showed that participants the positive valence dual-task condition had a higher memory reduction score compared to the negative valence dual-task condition and control group.	80

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. They are also not restricted to the laboratory session, but persist about 24 hours or 1 week after the intervention. The results are consistent with the findings of previous research demonstrating consistent effects of WM interventions on emotional memories and imagery (Landin-Romero et al., 2018; Lee & Cuijpers, 2013; van den Hout & Engelhard, 2012).

Concerning negative memories, 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. Unexpectedly, none of the moderators proved to be significant after excluding the Lee and Drummond (2008) and Schubert et al. (2011) studies, which were considered outliers due to their use of a procedure that was more similar to clinical practice (making horizontal eye-movements during retrieval of an unpleasant memory until SUDs are close to zero). Thus, although there are claims in the literature that longer durations should be more effective, that autobiographical memories might be more resistant to change than memories created in the lab, and that more demanding tasks should be more effective than less demanding ones, the results of the current meta-analysis failed to provide support for these claims. Yet, it is still possible that the moderators do influence the effect and that there

are alternative reasons why this did not emerge in the meta-analysis. For instance, variance

regarding intervention duration may have been insufficient, and, there may been too few

studies to test the effect of memory type (only nine studies used memories created in the lab).

Moreover, the grouping of dual-task difficulty was based on previous studies employing a

reaction time task to assess cognitive load (e.g., Engelhard, van Uijen, et al., 2010; Mertens,

Krypotos, et al., 2019). This has been done for most dual-tasks that were used in the meta-

analysis, but not for all (such as finger tapping). So there may be alternative reasons for the

lack of statistical significance in the moderator analyses. 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) are needed to establish whether such factors

influence the outcomes.

With regard to the pre-post difference in vividness and emotionality in the control

condition, the results demonstrated that there was little difference in vividness and

emotionality ratings from pre to post. Furthermore, contradictory to research showing some

cognitive load of keeping eyes stationary (Lenoble, Janssen, & El Haj, 2018), there was no

evidence that this results in lower ratings of vividness and emotionality after the intervention.

It could be argued, as predicted by the WM theory, that cognitive load of fixating the eyes is

insufficient to compete with the capacity to recall the emotional memory (Engelhard, van den

Hout, & Smeets, 2011). Furthermore, the results demonstrate a small (but significant)

decrease in emotionality ratings from pre to post intervention for negative memories. This fits

with the effects of imaginal exposure, which is an evidence-based treatment for PTSD (e.g.,

van Minnen & Foa, 2006).

Interestingly, the vividness and emotionality decrease at the follow-up test was larger

than at the immediate post-test. This was the case for both the dual-task condition and control

condition. It seems unlikely that this reflects spontaneous memory decay, because most studies examined well-consolidated autobiographical memories which are more resistant to spontaneous memory decay than new memories (McGaugh, 2000). It could reflect problems with reproducing the same memory that was rated in the pre-test, which likely increase over time. Indeed, recalling a memory is closer to reconstruction rather than reproduction. Therefore, it can be recommended to try to use some control over the memory that is rated in each test, for instance, by using the script-driven imagery procedure, which to our knowledge only two dual-task studies have done so far (Kearns & Engelhard, 2015; Thomaes et al., 2016). Another 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. Interestingly, 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.

Evidence for publication bias was demonstrated with a significant Egger's test for funnel plot asymmetry for emotionality of negative memories. When all studies were considered, the results indicated an underestimation of the average effect. After exclusion of the studies by Lee and Drummond (2008) and Schubert et al. (2011), the results showed evidence for missing studies on the left side, thus indicating publication bias for emotionality within the experimental group for negative memories. However, evidence for publication bias with these tests can result from chance or true heterogeneity, so they should be used with

caution (Lau, Ioannidis, Terrin, Schmid, & Olkin, 2006; Terrin, Schmid, Lau, & Olkin, 2003).

Therefore, publication bias was also examined with a *p*-curve analysis.

The results of the *p*-curve analysis showed a right skewed curve for both outcome

variables. 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

positives. Taken together, both the funnel plot test and *p*-curve analysis indicate a low

likelihood that the current meta-analysis shows an overestimation of the effectiveness of dual-

task interventions on emotional memories or images due to publication bias.

Regarding positive memories, the results demonstrated that dual-task interventions are

effective regardless of the memory valence. Furthermore, the effect was stronger for vividness

(12.48; 95% CI: [6.73, 15.39]) than for emotionality (5.35; 95% CI: [2.15, 8.54]).

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, Fivush, & Walker, 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.

Finally, relatively few studies have tested the effect of dual-task intervention on

memory performance. Some studies examined susceptibility to misinformation, some

examined response latency in a stimulus discrimination task, and some examined recall

accuracy. There was evidence for reduced memory performance using these measures.

However, due to the lack of studies, it was not possible to quantitatively examine the results.

Clearly, further research should be conducted to test the possible unwanted effects of EMDR

therapy on emotional memory, which is particularly relevant with respect to eye-witness testimony (see Houben et al., 2018; van Schie & Leer, 2019).

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, Ten Broeke, & De Jong, 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, Verhoeven, van den Hout, & Heitland, 2017) 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 EMDR therapy might not only be useful for PTSD but also for other types of disorders in which maladaptive behaviour-related memory and mental imagery is involved, such as addictive disorders (Müller, 2013). Furthermore, employing modality-specific tasks in EMDR therapy might be particularly useful in treating addictive

disorders given that cravings are often maintained by sensory imagery (i.e., imagining smell or taste: Andrade, May, & Kavanagh, 2012; Littel, Van Den Hout, & Engelhard, 2016). Nonetheless, more research is needed to establish the efficacy of EMDR therapy in addictive disorders and health interventions.

4.2. Limitations

There are three 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 overestimation 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.

The third limitation was that for some questions we examined, only a limited amount of studies were available and thus not all effects reported in the present meta-analysis can be interpreted with the same level of certainty. Hence, it is important for future research to focus

on aspects of this meta-analysis that need more evidence, such as effects of dual-tasks on positive mental images and memory performance.

4.3. Conclusions

In the current meta-analysis we investigated the effects of dual-tasks interventions on emotional memories. Overall, we found a substantial effect (i.e., approximately 5 to 12 points on a 100-point scale in the short term and 21-29 points in the long-term). Furthermore, publication bias analyses indicated little evidence for systematic bias in the literature. However, we found little evidence for moderators of this effect. Taken together, the effects of dual-tasks on emotional memories in the lab seem robust, but more research is required to determine whether specific factors moderate the effect.

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