2023

A critical analysis of the Working Memory model of Eye Movement Desensitization and Reprocessing

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http://hdl.handle.net/10026.1/20198

http://dx.doi.org/10.24382/544
University of Plymouth

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A CRITICAL ANALYSIS OF THE WORKING MEMORY MODEL OF EYE MOVEMENT DESENSITIZATION AND REPROCESSING

by

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A thesis submitted to the University of Plymouth

in partial fulfilment for the degree of

DOCTOR OF PHILOSOPHY

School of Psychology

September 2022

Acknowledgements

To my supervisor, Prof. Jackie Andrade, I would like thank you for your guidance and patience throughout my PhD, and for helping me to develop as a researcher.

I would also like to thank Dr Sophie Homer for offering her wisdom during our many brainstorming lunches, and for her friendship and support.

A special thank you to my family and to Jessica Kitch. You have always given me the confidence to keep pushing myself and your unconditional support kept me motivated to achieve my potential.

I dedicate this thesis in loving memory of my family members who sadly cannot celebrate this achievement with me. You always said I would surprise you – thank you.

Author's Declaration

At no time during the registration for the degree of Doctor of Philosophy has the author been registered for any other University award without prior agreement of the Doctoral College Quality Sub-Committee. Work submitted for this research degree at the University of Plymouth has not formed part of any other degree either at the University of Plymouth or at another establishment. This study was financed with the aid of a studentship form the University of Plymouth. The following external institutions were visited for consultation purposes: Utrecht University (Netherlands).

Presentations at conferences:

Stewart, A. (2015, December). *What Makes EMDR Work? A Comparison of Eye Movements to Matched Cognitive Tasks*. Poster session presented at the annual conference of the British Psychological Society Division of Clinical Psychology, London.

Stewart, A. (2016, June). Taxing Distressing Autobiographical Memory Imagery using Cognitive Tasks. In C. Deeprose (Chair), *Mental Imagery as a Therapeutic Target: From Lab to Clinical Application*. Symposium conducted at the 44th Annual Conference of the British Association for Behavioural and Cognitive Psychotherapies, Belfast.

Stewart, A. (2016, July). *Attenuating Distressing Mental Imagery Using Cognitive Tasks*. Poster session presented at the annual conference of the University of Plymouth Cognition Institute, Plymouth.

Stewart, A. (2017, June). *Working Memory and Emotional Mental Imagery*. Paper session presented at the annual Working Memory Discussion Meeting, Skipton.

Stewart, A. (2017, July). *How Eye Movements Affect Distressing Mental Imagery*. Poster session presented at the University of Plymouth 9th annual conference of the School of Psychology, Plymouth.

Stewart, A. (2017, August). *Attenuating Distressing Mental Imagery Using Cognitive Tasks.* Paper session presented at the annual conference of the British Psychological Society Cognitive Section, Newcastle.

Stewart, A. (2018, June). *Attenuating Distressing Mental Imagery Using Eye Movements: How Important is Visuospatial Load?* Paper session presented at the annual Working Memory Discussion Meeting, Skipton.

Stewart, A. (2018, June). Attenuating Distressing Mental Imagery using Cognitive Tasks. In J. Andrade (Chair), *Mental Imagery and Behaviour Change*. Symposium conducted at the University of Plymouth 10th annual conference of the School of Psychology, Plymouth.

Stewart, A. (2019, June). *How Moving Your Eyes Can Blur Perceptions of the Past*. Paper session presented at the annual conference of the University of Plymouth Cognition Institute, Plymouth.

Stewart, A. (2019, June). How Moving Your Eyes Can Blur Perceptions of the Past. In A. Smith (Chair), *Applied Psychology*. Symposium conducted at the University of Plymouth 11th annual conference of the School of Psychology, Plymouth.

Word count of main body of thesis: 73125

Signed

Date 25.09.2022

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A critical analysis of the working memory model of eye movement desensitization and reprocessing

Abstract

Eye Movement Desensitization and Reprocessing (EMDR) is one of the foremost interventions for posttraumatic stress disorder (PTSD). Treatment aims to desensitise and reprocess trauma memories by combining imaginal exposure to the trauma with concurrent bilateral stimulation, usually in the form of eye movements (EMs). Multiple explanations have been proposed to account for therapeutic effect of EMs in EMDR. This thesis examined a leading theoretical account: the working memory (WM) hypothesis.

To investigate the theory that EMs desensitise negative imagery in EMDR by taxing visuospatial WM, a series of experiments were conducted in which healthy subjects formed a visual image depicting a negative autobiographical memory while performing an EM task, an auditory task - designed to place similar demands on the central executive – and/or keeping both eyes stationary. We reliably found that EMs did not reduce image vividness and emotionality more than auditory interference. Evidence was mixed regarding the effect of EMs compared to fixation, although null-results may be explained by the use of a less powerful between-subjects design. These findings challenge the view that EMs interfere with distressing imagery in EMDR by taxing visuospatial WM, and are more consistent with the view that the general cognitive load of EMs can fully explain their desensitising effect on imagery in EMDR.

An important gap in current understanding of EMDR is how the WM interference created by EMs contributes to the reprocessing of trauma memories. A novel procedure was developed for use in laboratory settings to test the prediction that EMs facilitate memory reprocessing. In an initial study, healthy participants allowed their mind to wander between sets of negative recall with concurrent EMs, or fixation. Preliminary results showed that EMs did not facilitate mind wandering, although this may have reflected limitations in the study design. This novel procedure provides an avenue for future research on a revised model of how WM interference contribute to important processes in EMDR, beyond the immediate desensitisation of imagery.

Contribution to knowledge

This thesis addresses important questions about the mechanism by which EMs contribute to therapeutic changes in distressing memories, and how these changes contribute to the effectiveness of EMDR. I provide a critical review of key evidence supporting the WM hypothesis of EMDR and identify limitations of this evidence. For example, I highlight methodological issues in previous EM studies and present a series of experiments aimed at addressing some of these issues. These experiments resolve the contentious issue of whether EMs reduce the vividness and emotionality of negative imagery by taxing visuospatial WM resources. Based on the current findings, tentative recommendations are made regarding the delivery of EMDR and specifically how to improve the therapeutic effects of EMs and other dual-tasks. I also highlight the limitations of the WM hypothesis as an explanation for other important therapeutic processes in EMDR, namely the adaptive reprocessing of trauma memories. I propose a revised WM model of EMDR that can account for how the immediate therapeutic effects of EMs on negative memories may facilitate the adaptive reprocessing of trauma memories. I outline a novel method that can be used in a laboratory setting to test this revised model, and present the results of an initial experiment using this method. Preliminary findings suggest this novel procedure is capable to recreating the conditions for memory reprocessing in laboratory settings, and therefore provides a framework for future research on the revised WM model of EMDR. I recommend how the methods described in this thesis can be used to investigate the effects of taxing WM on different components of the trauma memory and during different phases of EMDR. Such research would provide a more comprehensive picture of the mechanism by which WM contributes to the effectiveness of EMDR.

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1 Chapter 1: literature review

1.1 What is EMDR, does it work, and are eye movements important?

1.1.1 What is EMDR?

EMDR was developed by Francine Shapiro to alleviate the distress caused by symptoms of PTSD, though it is now used to treat a variety of mental health issues. As an integrative therapy, EMDR's methods of delivery draw upon several other therapeutic paradigms, including cognitive-behavioural and psychodynamic therapies. The standard EMDR protocol is divided into eight distinct phases: client history and treatment planning; preparation; assessment; desensitisation; installation; body scan; close; and re-evaluation (Shapiro, 2018), and uses what is known as a three-pronged approach, in which clients process past traumas, current triggers of distress, and consider future templates of adaptive behaviour. The vast majority of research and debate has centred on the desensitisation phase, specifically the use of bilateral stimulation as a way of desensitising emotional responses to distressing autobiographical memories.

In a typical session of EMDR, desensitisation begins with the client focusing on several aspects of a trauma memory at once. Specifically, they are asked to generate a mental image of the entire or most upsetting part of the trauma, while also focusing on the dominant emotion, physical sensation and negative thought evoked by this image. Next, the therapist instructs the client to focus on each aspect of the trauma memory and then initiates a set of bilateral stimulation, usually in the form of EMs: the client tracks the therapist's fingers from one side of their range of vision to the other, for around 24 s. If, for example, the client is unable to perform the EM procedure, the therapist may choose to use other types of bilateral stimulation, such as binaural auditory sounds or alternate tapping of the clients' hands. Clients are informed in advance of desensitisation not to attempt to hold on to the trauma memory during bilateral stimulation, but that they should just notice whatever happens to their thoughts/mental images/emotions/bodily sensations during bilateral stimulation.

At the end of each set of bilateral stimulation, the client is instructed to let go of whatever they are thinking about; allowing their mind to go blank. This is followed by the question 'what comes to mind now?' In response, clients often report the emergence of new memories/images/thoughts/emotions/sensations associated with the original trauma episode, or that one or more aspects of the trauma memory have changed e.g. become less vivid or emotional (Shapiro, 2018). Whatever the client reports at the end of one set of bilateral stimulation is typically used as the target for the next set. If the client reports no change to the targeted material in successive trials, they are asked to focus again on the original trauma memory and bilateral stimulation continues. The desensitisation phase concludes once the client can focus on the trauma memory without feeling distressed.

1.1.2 Does EMDR work?

The efficacy of EMDR for reducing symptoms of PTSD is supported by several meta-analyses, the results of which are summarised in Table 1. The general finding from this research is that EMDR offers superior outcomes to inactive controls (waitlist/usual care: Bisson & Andrew, 2007; Bisson et al., 2007; Bisson, Roberts, Andrew, Cooper, & Lewis, 2013; Bradley, Greene, Russ, Dutra, & Westen, 2005; Davidson & Parker, 2001; Rodenburg, Benjamin, de Roos, Meijer, & Stams, 2009; Van Etten & Taylor, 1998) and comparable efficacy to Cognitive Behavioural Therapies (CBT: Bradley et al., 2005; Ho & Lee, 2012; Seidler & Wagner, 2006; Van Etten & Taylor, 1998). A smaller number of meta-analyses have found that EMDR is more efficacious than CBTs (Clark et al., 1998; Rodenburg et al., 2009), while others find superior outcomes for CBT compared to EMDR (Haagen, Smid, Knipscheer, & Kleber, 2015). One meta-analysis found no added benefit of EMDR compared waitlist for global PTSD symptoms (Gillies, Taylor, Gray, O'Brien, & d'Abrew, 2013). The large body of evidence supporting EMDR's efficacy has led to being recommendation in several clinical guidelines for the treatment of PTSD (Australian Centre for Posttraumatic Mental Health, 2013; Department of Veterans Affairs & Department of Defence, 2017; National Institute for Health and Care Excellence, 2018; World Health Organisation, 2013).

Although EMDR and CBT produce objectively similar clinical benefits, this does not mean that they work for the same reasons. Qualitative evidence shows that each therapy involves a unique set of experiences, which may reflect fundamental differences in treatment mechanisms. For example, whereas CBT is characterised by improvements in emotion regulation, clients who receive EMDR are more likely to report that distressing memories become less clear, more distant, and are therefore harder to recall after therapy (Cotter, Meysner, & Lee, 2017). There are also notable differences between the therapies in the way that traumatic memories are processed. Whereas CBT involves repeated imaginal exposure to the trauma memory, EMDR clients spend most of their time processing new associations that emerge during bilateral stimulation and relatively little time focused on the original trauma image.

To understand how EMDR uniquely contributes to therapeutic gains, investigations have naturally focused on the components of EMDR that are most idiosyncratic. Of these components, the use of bilateral EMs has received most research interest.

Table 1: a summary of meta-analytic research on the comparative efficacy of EMDR for PTSD.[1]

	Mean Effect Size (Cohen's *d* and Hedge's *g*, unless otherwise stated)	
	EMDR vs. Inactive control	EMDR vs. Cognitive Behavioural Therapies
Van Etten and Taylor (1998)	EMDR superior to waitlist in terms of PTSD symptom severity at post-treatment, self-reported/clinician rated (EMDR: *d* = 1.24/0.69; waitlist: *d* = 0.44/0.75).	Compared to pre-treatment, EMDR and CBT/exposure led to equivalent reductions in PTSD symptom severity at 15-week follow up, self-reported/clinician rated (EMDR: *d* = 1.33/2.27; CBT/exposure: *d* = 1.63/1.93).
(Davidson & Parker, 2001)	EMDR superior on outcome measures to waitlist/no treatment at post-treatment (*r* = .39).	EMDR equivalent to CBT/in vivo exposure in terms of post-treatment outcome measures (*r* = -.44).
Braddley, Greene, Russ, Dutra and Westen (2005)	EMDR superior to wait-list in terms of post-treatment PTSD symptomology (*d* = 1.25).	Compared to wait-list, EMDR was equivalent to CBT and exposure in terms of PTSD symptoms at post-treatment (EMDR: *d* = 1.25; CBT: *d* = 1.26; exposure: *d* = 1.26).
Seidler and Wagner (2006)		EMDR equivalent to trauma-focused CBT in terms of global PTSD symptomology at post-treatment (Hedge's *d* = 0.28) and 3-15 months follow up (Hedge's *d* = 0.13)
Bisson, Ehlers, Matthews, Pilling, Richards and Turner (2007)	EMDR superior to waitlist/usual care in terms of PTSD symptom severity at post-treatment (self-reported: *d* = -1.13; clinician rated: *d* = -1.51).	EMDR equivalent to trauma focused CBT in terms of PTSD symptom severity at post-treatment (self-reported: *d* = -0.17; clinician rated: *d* = 0.02).

[1] Several studies were excluded from Table 1 either because the full-text article was not accessible, the intervention received by the control group was not clearly described or was considered not relevant, or the effect sizes for EMDR and control were not compared. The excluded studies were as follows: Chen et al. (2014); Maxfield and Hyer (2002); Waller, Mulick, and Spates (2000); Sack, Lempa, and Lamprecht (2001); Benish, Imel, and Wampold (2008); Watts et al. (2013); Tran and Gregor (2016); Sherman (1998).

(Bisson & Andrew, 2007)	EMDR superior to waitlist/usual care at post-treatment for clinician rated PTSD symptom severity (*d* = -1.51), but not self-reported symptom severity (*d* = -1.07).	EMDR equivalent to trauma-focused CBT/exposure in for PTSD symptom severity at post-treatment (self-reported: *d* = -0.17; clinician rated: *d* = 0.03) and at 2-5 month follow-up (self-reported: *d* = -0.01; clinician rated: *d* = -0.14).
Rodenburg et al. (2009)	EMDR superior to wait-list in terms of PTSD symptomology at post-treatment (*d* = 0.67).	EMDR superior to CBT in terms of post-treatment trauma symptoms (*d* = 0.25).
Ho and Lee (2012)		No significant difference in PTSD symptoms between EMDR and trauma focused CBT post-treatment (*g* = 0.23).
Gillies, Taylor, Gray, O'Brien and D'Abrew (2012)	EMDR equivalent to waitlist at post-treatment for PTSD total scores (*d* = -0.61) but superior for re-experiencing symptoms (*d* = -0.75)	
Bisson et al. (2013)	EMDR superior to waitlist/usual care at post-treatment in terms of clinician rated PTSD symptom severity (*d* = -.1.17), but not self-reported symptom severity (*d* = -0.80).	EMDR equivalent to trauma-focused CBT/exposure in terms of PTSD symptom severity at post-treatment (self-reported: *d* = -0.30; clinician rated: *d* = -0.03) and at 1-4 month follow-up (self-reported: *d* = -0.04; clinician rated: *d* = -0.19).
Chen, Zhang, Hu, and Liang (2015)		EMDR superior to CBT (various types) at post-treatment in terms of PTSD symptomatology (*d* = -0.43), particularly intrusion (*d* = -0.37) and arousal (*d* = -.34) symptoms.
Haagen, Smid, Knipscheer and Kleber (2015)		EMDR leads to smaller decreases in PTSD symptoms (*g* = 0.38) than both exposure (*g* = 1.06) and cognitive processing therapy (*g* = 1.33).
Cusack et al. (2016)	EMDR superior to inactive control (e.g. waitlist, usual care) for PTSD symptoms (*d* = 1.08).	

1.1.3 Are eye movements an important part of EMDR?

Theoretical accounts about the contribution of EMs to EMDR were largely developed after its introduction (Shapiro, 1991, 2001). A focal point of subsequent research has been the debate about whether performing bilateral EMs makes a unique or specific contribution to beneficial therapeutic outcomes, or whether EMDR only relies on processes common to other types of therapy.

In their meta-analysis of dismantling studies, Davidson and Parker (2001) found no incremental benefit of EMs when comparing EMDR to the same procedure without EMs. Lee and Cuijpers (2013), however, pointed out weaknesses in Davidson and Parker's methodology. Firstly, the authors had used a fixed-effects rather than a random-effects model for their analysis despite heterogeneity of the studies that were included. Secondly, they had failed to weight studies according to sample size when calculating the average effect size. In a revised meta-analysis that also included newly published data, Lee and Cuijpers (2013) found an incremental benefit of EMs compared to various no-EM tasks in terms of improving PTSD symptoms and reducing the vividness and emotional impact of aversive memories. They concluded that EMs positively contribute to the processing of emotional memories in EMDR.

Upon closer inspection, the dismantling studies analysed by Lee and Cuijpers varied markedly with respect to the nature of the 'no-EM' control task. In treatment studies, for example, participants assigned to the no-EM control task in some studies focused on a stationary stimulus during EMDR (e.g. a spot of light, the therapist's hand), whereas in other studies the control group were asked to keep their eyes closed during the desensitisation phase. The extent to which EMs are judged to contribute to EMDR depends in part on the effectiveness of the control condition; this is relevant to Lee and Cuijpers study, as certain no-EM tasks may be more effective for desensitising distressing memories than others. At present, no meta-analysis of EMDR dismantling studies has reported a subgroup analysis based on the type of task performed in the obligatory no-EM condition. However, individual studies highlight the importance of this analysis in discerning the added benefit of EMs in EMDR. In a recent large clinical study by Sack et al. (2016), individuals diagnosed with PTSD were assigned to EMDR with EMs, or to one of two variants of EMDR in which the EM component was removed: EMDR while focusing on the therapist's stationary hand; EMDR without visual fixation (i.e. eye closure or staring unfocused into the room). Analysis of pre-post changes in symptom severity and frequency showed similar improvements in both groups who received EMDR while focusing on the therapist's stationary hand, which exceeded the benefits of EMDR without visual fixation. These findings have two implications. First, the finding that EMs and visual fixation produced similar treatment benefits challenges the notion that EMs make a positive contribution to the efficacy of EMDR. Again, more definitive evidence requires an analysis of previous and recent dismantling studies, grouped according to the type of control task. Secondly, the finding that EMDR with central fixation was more effective than EMDR without visual fixation adds to the evidence that EMDR does not simply rely on traditional exposure. Taken together, the findings of Sack et al. (2016) and Lee and Cuijpers (2013) are consistent with qualitative evidence that EMDR generates a sense of 'distancing' from emotional memories as opposed to reliving through imaginal exposure (Lee, Milton, & Everitt, 2006). As I will argue, evidence from dismantling studies and research on EM mechanisms fits with the view that EMs work by dividing attention and thus preventing re-experiencing of the trauma memory.

1.1.4 Summary of EMDR and the contribution of EMs

There is an abundance of evidence supporting the efficacy of EMDR as an intervention for PTSD. This evidence suggests that EMDR offers similar benefits to exposure-based therapies and CBT, but appears to work by distinct mechanisms.

Dismantling studies, in general, support the additional benefit of performing EMs while focusing on upsetting memories, although preliminary clinical evidence suggests these benefits may be due to dual focus on a secondary stimulus during recall and not because of EMs per se. A key question, which links directly to theories of cognitive processing during EMDR, is how EMs and other types of dual-attention stimuli contribute to the desensitisation of emotional memories in EMDR. This question is addressed in the following section.

1.2 How do eye movements desensitise emotional memories in EMDR?

1.2.1 The current status of EMDR mechanism research

Several reviews of the EMDR mechanism literature have been published in the last decade. These reviews have focused, to varying degrees, on the cognitive (Coubard, 2016; Gunter & Bodner, 2009; Jeffries & Davis, 2013; Oren & Solomon, 2012; Shapiro, 2012; Van den Hout & Engelhard, 2012), psychophysiological (Elofsson, von Scheele, Theorell, & Sondergaard, 2008) and neurobiological (Pagani, Högberg, Fernandez, & Siracusano, 2013; Propper & Christman, 2008), processes by which EMs may improve EMDR's efficacy. Collectively, they suggest that EMs contribute to EMDR through multiple mechanisms.

One of the most intensively researched explanations of EMDR is the WM hypothesis, which is described in detail in the rest of this chapter. Briefly, the WM hypothesis states that performing EMs and forming a mental image of a traumatic event are both processes that tax the limited capacity of WM, therefore the two compete for finite cognitive resources when they occur concurrently. According to this account of EMDR, competition for WM capacity during bilateral stimulation drives reductions in the vividness and therefore the emotional intensity of trauma recollection.

Research on the WM account of EMDR has been particularly influential in informing the treatment of distressing mental images. For example, the evidence that taxing WM reduces the vividness and emotional intensity of concurrent emotional recall (see following sections) supports the inclusion of dual-attention stimuli in EMDR. Furthermore, studies that have found the emotional benefit of dual tasks is influenced by WM factors, such as the load they place on WM, have informed recommendations about how EMDR should be delivered to maximise its effectiveness (Beer et al., 2011). Beyond the treatment of PTSD, early work on the WM hypothesis has also influenced current thinking about how to treat other mental difficulties that are characterised by problematic imagery. For example, dual-task procedures like those used in EMDR research have been proposed as an intervention for intrusive images related to substance craving (Littel, van den Hout, & Engelhard, 2016; McClelland, Kemps, & Tiggemann, 2006), performance anxiety (Engelhard et al., 2012), public speaking anxiety (Homer, Deeprose, & Andrade, 2016), and suicidal ideation (Bentum et al., 2017). These are but a few examples of the growing interest in WM theory and its application in the treatment of distressing imagery.

We shall see in the remainder of this chapter that even within this subset of research on EMDR, there is uncertainty about how WM interference affects emotional recall in therapy. For example, there is continued debate about whether the literature supports the view that EMs interfere with emotional imagery because they impose a general cognitive load, or specifically because they involve visuospatial WM processing that competes with the recollection of primarily visual images. Answering this question should inform the choice of dual-attention stimuli (e.g. EMs or tones) used in EMDR. Another important question relevant to EMDR is whether the effects of WM interference on imagery serves the primary purpose of helping clients to engage in other aspects of therapy, such as generating positive associations to the trauma memory, or whether EMs and other dual-attention tasks permanently change the trauma memory, through reconsolidation of the memory in a weakened form. Subsequent chapters in this thesis address these questions using novel methodologies that improve upon previous studies. The aim of this research is to provide a more secure foundation for cognitive explanations of EMDR.

1.2.2 Foundations of the Working Memory hypothesis

Most research attention aiming to unpick the underlying mechanisms of EMs in EMDR has been based on the WM model of (Baddeley, 1986; Baddeley & Hitch, 1974). WM describes the ability to temporarily store and manipulate information to perform complex cognitive tasks, such as totting up a shopping bill or keeping a road sign in mind while navigating a complex junction. According to the WM model, we perform such tasks using temporary storage and rehearsal processes that are specific to information in auditory and visual modalities: the phonological loop and visuospatial sketchpad, respectively. In early versions of the model, the phonological loop and visuospatial sketchpad were coordinated and controlled by an attentional component called the central executive. In 2000, Baddeley introduced a fourth component called the episodic buffer (Baddeley, 2000). The episodic buffer temporarily stores multimodal, ‘bound’ representations derived from information stored in the phonological loop and visuospatial sketchpad, or retrieved from long-term memory (Baddeley, Allen, & Hitch, 2017). In either version of the model, the sensory qualities of a representation in WM are determined by the extent to which it is maintained in the phonological loop or visuospatial sketchpad.

Baddeley (1986) hypothesized that the different components of WM have limited capacity and that they support mental imagery as well as short-term memory. These hypotheses are supported by evidence that performance on short-term memory and imagery tasks is similarly disrupted by concurrent processing in the same modality (Beech, 1984; Logie, 1986; Logie, Zucco, & Baddeley, 1990; Quinn, 1988; Quinn & McConnell, 1996). Baddeley and Andrade (2000) showed that the same is true for the self-reported vividness of imagery. In a series of experiments, they asked participants to retain images of briefly presented visual and auditory stimuli or to generate visual or auditory images from long-term memory, prompted by verbal cues (e.g. imagine a rose garden, or imagine the sound of a telephone ringing). While imagining, participants performed the verbal task of counting aloud (also referred to as articulatory suppression), which loads the phonological loop, or the visuospatial task of tapping a specific pattern on a keyboard, which loads visuospatial WM. In the control condition, they simply focused on keeping the image in mind as vividly as possible. Across experiments, images were most vivid in the control condition and were least vivid when in the same sensory modality as the concurrent task. Baddeley and Andrade interpreted these findings as evidence that vivid imagery reflects the availability in WM of rich sensory information. They argued that image vividness is boosted by temporary storage of sensory information in visuospatial and verbal WM systems, hence the modality-specific effects of concurrent task loads, and by the retrieval and manipulation of this sensory information via the central executive, hence why images were less vivid under a secondary task in a different modality than in the control condition. That vivid imagery - and by analogy vivid recollection - depends on WM offered a potential explanation for the therapeutic effect of EMs in EMDR.

Several early papers on EMDR suggested that EMs distract from emotional visual images or help them to fade (Dyck, 1993; Kleinknecht & Morgan, 1992; Merckelbach, Hogervorst, Kampman, & de Jongh, 1994) though Merckelbach et al. (1994) found no support for this idea when comparing the effect of EMs on emotional arousal with the control task of finger tapping. Later, Andrade, Kavanagh, and Baddeley (1997), building on the initial findings from Baddeley and Andrade (2000) study, proposed that EMs act on visual imagery in a similar way to other visuospatial tasks like pattern tapping. That is, EMs interfere with imagery through modality-specific WM interference, as well as through the general cognitive or executive load they impose. Consistent with this proposal, a series of experiments by Andrade et al. (1997) showed that the vividness of visual mental images significantly decreased if participants performed EMs or complex spatial tapping during image visualisation, but not if they counted aloud. Crucially, EMs also reduced the vividness, and more importantly, the emotionality of images related to negative autobiographical memories, which suggested that EMs might desensitise vivid trauma memories in EMDR by disrupting their representation in WM.

Since Andrade et al. (1997) proposed their WM hypothesis of EMDR, there have been a large number of studies designed to test key predictions of the WM hypothesis regarding the effects of EMs on emotional imagery. The following section begins with an overview of key evidence that EMs reduce the vividness and emotionality of mental imagery, which primarily comes from research on negative autobiographical events. Within this section, I explain how EMs can desensitise emotional responses to affective imagery given that Baddeley's WM model is a model of cognition and not emotion. In the sections that follow, I analyse key pieces of evidence that the effects of EMs on emotional imagery reflects the disruption of WM at the level of the central executive and, potentially the visuospatial sketchpad.

1.2.3 The immediate effects of eye movements on emotional mental imagery

According to the WM hypothesis of EMDR, EMs and other dual-attention stimuli reduce the emotionality of trauma recall by degrading sensory representations or images of the incident in WM. The WM model (Baddeley, 1986) makes no predictions about the effect of dual tasking on emotion. However, a close relationship between imagery and emotion is consistent with an embodied cognition approach (Barsalou, 2008; Kavanagh, Andrade, & May, 2005; Wilson-Mendenhall, Barrett, Simmons, & Barsalou, 2011) in which mental imagery involves the activation of conceptually related sensory and affective information – in other words, a re-experiencing of the original event or stimulus. Others authors (Holmes & Mathews, 2005, 2010) have similarly argued that to the extent vivid mental imagery conveys a sense of immediate perceptual experience, recalling the sensory details of a trauma can re-invoke feelings that were present during the original incident. Reducing image vividness by applying a dual-task (Baddeley & Andrade, 2000) can therefore reduce the intensity of emotions associated with the imagined material.

Numerous studies have found that making EMs while retrieving an emotional mental image reduces its subjective vividness and emotionality more than retrieval without EMs. The majority of this evidence comes from laboratory research on the negative autobiographical memories of healthy samples (Barrowcliff, Gray, Freeman, & MacCulloch, 2004; Gunter & Bodner, 2008; Hornsveld et al., 2010; Kavanagh, Freese, Andrade, & May, 2001; Kemps & Tiggemann, 2007; Kristjánsdóttir & Lee, 2011; Leer, Engelhard, & van den Hout, 2014; van den Hout, Eidhof, Verboom, Littel, & Engelhard, 2014; van den Hout, Muris, Salemink, & Kindt, 2001; van Schie, van Veen, Engelhard, Klugkist, & van den Hout, 2016; van Veen, Engelhard, & van den Hout, 2016; van Veen et al., 2015). Additionally, EMs have been found to reduce the vividness and emotionality of mental images related to traumatic events (Lilley, Andrade, Turpin, Sabin-Farrell, & Holmes, 2009; van den Hout et al., 2012), recently encoded aversive stimuli (Leer, Engelhard, Altink, & van den Hout, 2013; Leer et al., 2017, experiment 2), feared future scenarios (Engelhard et al., 2012; Engelhard, van den Hout, Janssen, & van der Beek, 2010) and positive autobiographical memories (Barrowcliff et al., 2004; Engelhard, van Uijen, & van den Hout, 2010; Hornsveld et al., 2011; Kemps & Tiggemann, 2007; van den Hout et al., 2001). The subjective effects of EMs are also corroborated by changes in behavioural/physiological measures of imagery and emotion, which are less likely to be influenced by demand characteristics.

For example, participants show impaired recognition of recently presented pictures that have been recalling while making EMs (Leer et al., 2017; van den Hout, Bartelski, & Engelhard, 2013; van Schie, Engelhard, & van den Hout, 2015), consistent with a degrading effect of EMs on imagery. Other studies provide objective support for the desensitising effect of EMs, as indicated by reduced physiological arousal (e.g. lower heart rate: for a review of studies, see Söndergaard & Elofsson, 2008) and reduced activity in emotion-processing brain regions (Thomaes, Engelhard, Sijbrandij, Cath, & Van den Heuvel, 2016) when mental images are retrieved following concurrent EMs. Relatively few studies have found no added effect of performing EMs during recall on the vividness or emotionality of imagery compared to recall without EMs (Leer, Engelhard, Dibbets, & van den Hout, 2013; Thomaes et al., 2016; van Schie et al., 2015; van Schie & Leer, 2019), although the authors cite plausible reasons for the null-results, such as lack of statistical power.

The WM hypothesis states that changes in the emotionality of imagery are caused by changes in its vividness. It follows that the effects of EMs on image vividness and emotionality should follow a similar decreasing trend. More precisely, the effect of EMs on image vividness should precede, or occur in tandem with, but not follow changes in image emotionality. Most research on the effects of EMs offers little insight into the relationship between imagery and emotion. This is because outcomes are measured one to two minutes after the initial set of concurrent EMs (Hornsveld et al., 2010; van den Hout et al., 2001; van den Hout et al., 2012), meaning pre-post changes in image vividness and emotion provides little information about the time-course of EM effects. Studies using measures of imagery after each period of recall (Kavanagh et al., 2001; Lilley et al., 2009) supports the close relationship between the effects of EMs on image vividness and emotionality. However, these studies do not indicate the order in which EM effects occur, because imagery and emotion were both affected after the initial period of dual tasking. Further detail about the time-course of EM effects is provided by research in which outcomes were measured as soon as 2 seconds after the onset of concurrent EMs (Smeets, Dijs, Pervan, Engelhard, & van den Hout, 2012). This research demonstrates that EMs cause almost immediate reductions in image vividness, compared to delayed reductions in image emotionality. That changes in subjective emotion do not precede changes in image vividness is important, because it rules out the possibility that EM effects on vividness ratings reflect attenuated emotional responses to image retrieval (for further discussion, see van den Hout et al., 2001). While the evidence regarding the time-course of EM effects are consistent with the WM hypothesis, they do not necessarily prove a causal relationship. That is, EMs may affect imagery and emotion through unrelated mechanisms, whereby the distinct time-course of these effects reflect the number or duration of underlying processes – perhaps underlying processes involved in emotional change take longer than those involved in changes to the contents of WM.

Several studies have failed to find EM effects on both vividness and emotionality of imagery. Some studies have found a selective effect of EMs on the emotionality of imagery but not its vividness (e.g. de Jongh, Ernst, Marques, & Hornsveld, 2013; Engelhard, van Uijen, et al., 2010), while others have found the opposite (Maxfield, Melnyk, & Hayman, 2008, experiment 1; van den Hout et al., 2011b, experiment 2; van den Hout et al., 2011a, experiment 4). These results challenge the close association between imagery and emotion, and suggest that the emotional benefits of EMs does not always depend on the regarding of imagery in WM. Failed attempts to replicate an effect of EMs on image vividness have been attributed to lack of statistical power (de Jongh et al., 2013; Thomaes et al., 2016), or to the use of less emotionally arousing imagery (for further discussion on the role of emotional arousal, see Leer, Engelhard, Altink, et al., 2013; van Schie et al., 2015). However, studies without such issues have still failed to find an effect of EMs on image vividness (Engelhard et al., 2010). It has been argued that evidence of a selective effect of EMs on image vividness but not emotionality may reflect differences in the way that participants appraise the degradation of the image (Gunter & Bodner, 2008). According to this argument, the blurring of an upsetting image for one person may foster self-efficacy beliefs about controlling the image, and should therefore result in a positive emotional response. On the other hand, a person may react negatively to the same experience because of anxiety that the image, which may be a deceased loved one, will be permanently lost. Individual differences in appraisal might explain why EMs tend to affect the vividness of imagery more reliable than its emotionality.

Collectively, research on the WM hypothesis mirrors the use of EMs as part of EMDR's three-pronged treatment approach. Laboratory studies in which EMs disrupt negative imagery supports the combination of EMs with negative treatment targets in EMDR, namely mental images of past traumatic events, and images of present triggers of distress. Evidence that EMs effect future and positive imagery is also relevant to the later stages of EMDR, in which EMs are combined with future templates: the client mentally rehearses managing challenging future scenarios. Moreover, positive or 'safe place' imagery, which clients use as a relaxation tool, is also combined with EMs in the preparation phase of therapy (2001, p.125). EMs are reported to facilitate the processing and emotional benefits of positive material in EMDR (Shapiro, 2001, 2018), however this would appear to contradict the emotional blunting effect of EMs on positive images found in laboratory settings. This apparent discrepancy has led to debate about whether using EMs with positive material may have adverse effects on treatment outcomes (Hornsveld, de Jongh, & ten Broeke, 2012; Hornsveld et al., 2011; Leeds & Korn, 2012). As I will argue in chapters 8 and 9, it may be that reconciling this debate depends on how one frames the role of WM interference in EMDR. In chapter 8, I explore the possibility that EMs, by degrading emotional imagery, facilitates the retrieval of novel information from associated memory networks. Such an account is consistent with the effects of EMs on negative and positive imagery in laboratory studies and compliments the desensitising and reprocessing effects of EMs in therapy.

In summary, the WM hypothesis is supported by evidence that emotional mental images become less vividness and less emotional after concurrent EMs. Preliminary findings suggest that changes in image vividness precede changes in emotion, which is consistent with the view that imagery (re)invokes the sensory and affective experiences present at encoding. However, the literature is inconclusive as to whether EMs must interfere with imagery to have an emotional benefit - multiple EM studies show that changes in the emotionality of imagery can occur independently of changes in its vividness. It has been argued that the WM account of EMDR is further complicated by evidence that EMs disrupt the sorts of positive imagery that clients use for relaxation or to rehearse adaptive future behaviours. As I will argue, these apparent limitations of the WM model of EMDR may be less controversial if one adopts a different view of how WM operates in EMDR – reductions in image vividness alone may aid the retrieval of information from adaptive memory networks (chapter 8).

In the following sections, I review evidence for some of the key predictions made by the WM hypothesis, namely that the effects of dual-tasks on emotional imagery should depend on the WM load of the dual task, the WM capacity of the individual, and the overlap between the sensory modality of the task and image. This evidence provides context for the experiments described in subsequent chapters and the discussion of recommendations for future researh.

1.2.4 WM capacity and dose-response

According to WM theory, the effects of dual-attention stimuli on emotional memories should be larger for individuals with low WM capacity and smaller for those with high WM capacity. This is because low WM capacity confers less ability to hold a memory vividly in mind during dual task performance, which, in effect, enhances the impact of concurrent WM interference. In contrast, high WM capacity provides sufficient resources for the vivid maintenance of a memory during WM taxation, meaning there should be less interference between the task and image. Consistent with this prediction, multiple studies have found a significant negative correlation between WM capacity and the reduction in vividness/emotionality caused by EMs (Engelhard, van Uijen, et al., 2010; van den Hout et al., 2011b, experiment 2, although see experiment 1), mental arithmetic (van den Hout et al., 2010), and figure drawing (Gunter & Bodner, 2008). In all cases, central executive functioning was used as the index of WM capacity, and in most cases was measured as the extent to which dual tasking impaired performance on RT measures (with the exception of Gunter & Bodner, who used reading span). Given the multifaceted nature of WM, and even executive function, there is room to expand such measures in order to determine which WM processes best predict the effects of taxing WM on imagery. For example, researchers could measure performance on tests of visuospatial and verbal WM (see chapter 3 and 4) to determine if the effects of dual-tasks on imagery are moderated by the capacity of modality-specific WM stores.

A WM hypothesis also predicts that the benefits of EMs and other dual-attention stimuli should follow a 'dose-response' relationship, in that the effects on imagery should depend on the amount of interference created by the competing task. One way the dose of interference can be increased is by imposing a greater general cognitive load during recall. For example, several studies have found greater reductions in memory vividness and/or emotionality using faster versus slower EMs (Maxfield et al., 2008; van Schie et al., 2016; van Veen et al., 2015), and complex versus simple spatial pattern tapping (Andrade et al., 1997). Another way dose can be increased to achieve larger effects on imagery is by extending the amount of time spent dual tasking, such as pairing EMs with recall a greater number of times (Leer et al., 2014). Though this does not increase the WM load of the task, presumably performing more sets of dual-tasking increases the overall amount of image interference produced by taxing WM.

Crucially, because WM capacity is finite, increasing task load should not result in linearly increasing emotional benefits. From a certain point, the demands imposed by the competing task will prevent retrieval of the target emotional memory into a labile state in which it can be degraded. The relationship between task load and memory benefits is therefore described as an 'inverted U', in that effects on imagery are predicted to be smallest at the highest/lowest dose of interference. So far, only one study has shown that complex and simple concurrent tasks produce smaller emotional benefits than moderately difficult tasks (Engelhard, van den Hout, & Smeets, 2011), though the effects on vividness in this study were less compelling. Other studies have found no difference in the benefits of different interference tasks, even though one imposes demonstrably greater WM demands than the other (Altink, Terwisga, Helms, & Oostbroek, 2012; Mertens et al., 2018; van den Hout et al., 2010). While these latter studies may also show an effect overtaxing WM, it can be argued the null-effects of increasing task load is evidence against a WM hypothesis.

Stronger evidence for the WM model relies on showing that the effect of EMs on imagery depends on the interaction between the WM load of the task and the WM capacity of the individual. More precisely, WM theory predicts that a complex task will be overly taxing for someone with low WM capacity, and a simple task will be too easy to disrupt recall for someone with high WM capacity, while emotional benefits will be greatest if task load is titrated to the WM capacity of the individual. Preliminary research offers little support for this prediction. The only study to test this prediction (van Schie et al., 2016) found that low WM capacity participants experienced greater reductions in memory vividness and emotionality than those with high WM capacity, who in turn experienced similar effects on memory of fast and slow EMs. Given that only two speeds of EMs were used in this study (fast or slow), the question remains as to whether greater benefits are received when WM load is tailored to each individual based on their WM capacity, as opposed to a uniform level of taxation during recall.

As it stands, there is consistent evidence that the amount of WM interference produced by a task and the WM capacity of an individual independently influence the effects of dual tasking on emotional memories. However, it is unclear why these mediating factors do not interact as predicted by a WM hypothesis. One possibility is that the benefits of dual-attention stimuli rely on more than just the general cognitive load imposed by a task, or the availability of general-purpose WM resources, which have been the focus of research to date. As we discuss in more detail below, there has been considerable research on the proposal that modality-specific WM processes also contribute to the effects of EMs and other tasks on emotional imagery. Future research could investigate if the benefits of EMs, for example, are predicted by individual differences in visuospatial WM capacity, or whether increasing the verbal WM load of a task produces a dose-dependent decrease in the vividness of auditory images. It may also be interesting to compare these results to those of existing studies on capacity and dose, to determine if measuring/taxing modality-specific WM processes is a better predictor of the impact on imagery. If, as WM theory predicts, the visuospatial and verbal WM stores play a role in the effects of dual-attention stimuli, then titrating the modality-specific demands of a task to the visual/verbal WM capacity of an individual should maximise reductions in distress and memory vividness. As a starting point, future clinical studies could include baseline measures of visual and verbal WM capacity and measure their relationship with clinical outcomes.

1.2.5 Modality-specific interference versus general cognitive load

The study by Baddeley and Andrade (2000) of image vividness suggests there are two ways in which EMs and other types of dual-attention tasks can interfere with mental imagery. First, the modality-specific processing demands of the competing task can selectively disrupt sensory representations in WM by taxing limited capacity storage and rehearsal processes, namely the visuospatial sketchpad and phonological loop. Second, the amodal, central executive processing demands of the task can disrupt the retrieval of sensory information long-term memory, thereby limiting the amount available for the generation of imagery.

There has been divided opinion among researchers about which WM processes contribute to the benefits of dual-attention stimuli in EMDR. Gunter and Bodner (2008) have argued that the central executive plays a dominant role in the effects of EMs and other tasks on negative memories. They predict that the greater the cognitive load imposed by a task, the more effective it will be at reducing the distress experienced when recalling an emotional event. Andrade et al. (1997) predicted that in addition, for tasks that impose equal attentional loads, a primarily visual task will have a greater impact on visual imagery than one that is non-visual, and a task that involves greater verbal processing will interfere with verbal imagery more than a non-verbal task – interference will be modality-specific.

The modality-specific interference hypothesis has potentially important implications for the delivery and outcomes of EMDR. Therapists are encouraged to swap EMs for other tasks, such as binaural tones (Shapiro, 2018) with little theoretical justification for choosing one task over another. An interaction between task and image modality offers a clear rationale, which is that tasks should be more beneficial when matched with the sensory characteristics of a client's distressing memory. Furthermore, there may be unintended consequences for other aspects of therapy if therapists rely solely on the general cognitive demands of a task to interfere with imagery. As I discuss in more detail in chapter 8, a reportedly crucial aspect of EMDR is the retrieval of trauma-related information, which clients often experience during bilateral stimulation (Shapiro, 1991, 2018). This process, which involves shifting focus between images, thoughts and sensations likely to relies on finite attentional resources (Smallwood & Schooler, 2006), and may therefore become less efficient if the dual-attention task is too demanding. A modality-specificity hypothesis overcomes this potential issue, as it raises the possibility of designing tasks that impose negligible demands on attention, but that interfere effectively with imagery by loading heavily on to the storage and rehearsal processes of WM. Given the potential implications for therapy, I will now review evidence regarding the modality-specific interference hypothesis of EMDR. I focus on studies in which the effect of EMs on images related to emotional autobiographical memories was studied, as this evidence is most relevant to the WM model EMDR.

Underpinning early tests of the WM hypothesis was the assumption that emotional memories were primarily visual and therefore more susceptible to visual rather than verbal/auditory interference (Andrade et al., 1997). Various visuospatial tasks, including EMs, have been shown to reduce the vividness and/or emotionality of emotional memories. These include spatial tapping (Andrade et al., 1997), plasticine modelling (Andrade, Bosworth, & Baugh, 2012) and Tetris (Engelhard, van Uijen, et al., 2010). Crucially, researchers have contrasted the effects of EMs on visual imagery with that of non-visuospatial tasks, which is necessary to rule out the possibility that EM benefits are due to general processing demands alone. Andrade et al. (1997), for example, found that the recollection of emotive photographs was rated as less vivid and emotional if the photographs had been recalled while performing EMs, compared to articulatory suppression (which loads verbal WM). Lilley et al. (2009) later extended these findings to trauma memories that participants described as primarily visual. Because these authors explicitly measured or controlled the sensory modality of the image, the results were interpreted as evidence that EMs disrupted the maintenance of imagery in visuospatial WM. However, a key limitation in both studies (Andrade et al., 1997; Lilley et al., 2009) was that general processing demands of the tasks were unknown. It is entirely plausible the EM task - monitoring and responding to bilaterally flashed letters – required more attention than counting from one upwards, which participants performed during recall in the articulation condition. By failing to establish that EM and auditory imposed a similar general cognitive load, differences between the effects of these tasks on the target image could be attributed to general rather than modality-specific interference.

Other researchers have measured the general load of the tasks used to disrupt imagery, but as in previous studies, the tasks were not matched. van den Hout et al. (2011a) used RT measures to show that EMs taxed the central executive more than receiving binaural tones (experiment 3). They also showed that EMs reduced the vividness of negative memories more than tones (experiment 4), which was later replicated using trauma memories (van den Hout et al., 2012). Because EMs were more demanding, it was concluded that their effect on imagery supported a central executive hypothesis; however, the possibility cannot be ruled out that the visuospatial WM load of EMs contributed to effects on imagery in these studies. Although the findings are difficult to interpret, the methods used by van den Hout et al. (2011a) are nonetheless useful for testing a modality-specificity hypothesis. Researchers could use a similar approach of measuring the general cognitive load of two tasks, ensuring that the tasks are then matched before being combined with memory recall.

An alternative approach to rule out a purely central executive explanation is to compare how the same tasks affect images containing different sensory information. Kemps and Tiggemann (2007) used this approach in their study, which currently offers strongest support for the modality-specificity WM hypothesis. In the first of two experiments, the authors asked participants to focus on the image of an emotional event while performing no task, EMs, or articulatory suppression. Participants then rated the extent to which this image contained visual, auditory, and other sensory details. Compared to other tasks, EMs led to significantly greater reductions in ratings of image vividness and emotionality. Crucially, the difference in ratings between EMs and articulation reduced to non-significance when statistically controlling for the extent to which images were mainly visual/auditory. If EMs affect imagery due to general cognitive load alone, one would not expect discrepancies in image modality to mediate interference effects. These initial findings were corroborated by a second experiment in which participants were instructed to generate specifically visual and auditory images of an emotional event. This second experiment found that EMs reduced the vividness and emotionality of visual images more than articulation, while the opposite occurred for auditory images. The results of Kemps and Tiggemann (2007) demonstrate that disrupting the maintenance of imagery in visuospatial/verbal WM reduces its emotional intensity by a small to moderate, but statistically significant amount. Whether this additional impact of modality-specific interference is clinically meaningful is not yet know, although preliminary research has been carried out in clinical settings.

One recent attempt to replicate Kemps and Tiggemann's study in therapeutic settings failed to replicate their results. In a single experiment by Matthijssen, Verhoeven, van den Hout, and Heitland (2017), PTSD patients received a modified form of EMDR in which they were asked to recall two emotional memories - one mainly visual and one mainly auditory - while performing EMs, articulatory suppression, or central fixation. Results showed that irrespective of task condition, the emotionality of visual and auditory memories decreased significantly from pre to post task. In other words, not only did the study fail to replicate an effect of modality, there was also no significant effect of taxing WM during recall. The authors noted that the analyses lacked statistical power due to methodological issues, which to some extent may explain the unexpected findings. Indeed, visual inspection of the data (Matthijssen et al., 2017, figures 2 and 3) shows a trend toward greater reductions in distress in the experimental task conditions. However, it is less likely the results for modality reflect a lack of power, as there was little evidence that EMs affected visual imagery more than auditory interference. Why the effect of modality observed in laboratory settings should not translate to therapy is unclear. Future research could investigate mediating factors if this discrepancy is reliably replicated. As it stands, these data offer preliminary evidence that task/image modality makes little difference to distress when recalling a negative event during EMDR. They also add to the results of several studies that challenge a modality-specificity hypothesis.

Several other studies have found an effect of taxing WM on imagery, but no evidence that this effect depends on task modality. In two experiments, van den Hout et al. (2011b) found that EMs and attentional breathing led to similar reductions in the vividness (experiment 2) and emotionality (experiment 1) of negative memories. Crucially, both tasks were found to produce similar degrees of impairment when performed during a RT task, suggesting they were matched in terms of general cognitive load. Attentional breathing requires little obvious visuospatial processing, therefore the results would appear to contradict a modality-specificity hypothesis. However, because the recall instructions used in this study were general with respect to the retrieval of sensory information, participants may have recalled memories that contained a strong auditory component, for example. If so, there would have been little opportunity to observe how EMs affect the storage of imagery in visuospatial WM.

Lack of information about the sensory features of the target memory is a common issue among studies that find no effect of modality. In their study, Gunter and Bodner

(2008, experiment 3) compared EMs to auditory shadowing (passively listening to a recording of the word “ta”) and found that both tasks led to similar reductions in the vividness, emotionality, and completeness of negative memories. The authors assumed that the auditory task must have been sufficiently complex to disrupt memories to the same extent as EMs, via the central executive. However, this explanation seems unlikely given that EMs tax the central executive significantly more than passively receiving auditory interference (van den Hout et al., 2011a, experiment 3). If the auditory task imposed a smaller central executive load, the surprisingly large effect of the auditory task could be explained by an overlap between the modality of the task and auditory detail within the image. As the sensory modality of the memory was not reported - participants were asked to recall memories in a general way with no mention of specific modalities – the potential impact of modality in this study cannot be ruled out.

Stronger evidence against the modality-specificity hypothesis comes from research where the authors measured the sensory information present in the targeted memory. Kristjánsdóttir and Lee (2011) asked participants to recall and then rate a negative memory in terms of the strength of its different sensory characteristics. The memory was then recalled during EMs, auditory interference, or no dual-task procedure. Analysis revealed that compared to auditory interference, performing EMs during recall led to greater reductions in vividness. Crucially, the effect of EMs was not significantly moderated by statistically controlling for the degree to which memories contained mainly visual or auditory detail. That is, the overlap between image and task modality did not appear to influence the effect of dual-tasking. While these findings contradict the first experiment of Kemps and Tiggemann (2007), a stronger test would have been to ask participants to generate mainly visual/auditory memories, or to focus on one sensory aspect of the memory during task performance (Kemps & Tiggemann, 2007, experiment 2). Nevertheless, the findings offer further evidence against the proposal that EMs exert their effects on imagery by taxing modality-specificity WM processes.

A recent study (Mertens, Bouwman, Asmervik, & Engelhard, 2020) in which the modality of the target image was controlled offers perhaps the strongest evidence against the modality-specific WM hypothesis. In this study, participants focussed on a distressing visual or auditory mental image while performing an EM and auditory task (see chapter 2) than had been designed to place similar demands on the central executive. Visual and auditory images were respectively based on negative photos and sounds from the International Affective Picture/Digital Sounds databases, meaning images contained only a single sensory modality. In contrast to the findings of Kemps and Tiggemann (2007), an initial experiment found that the EM and auditory task caused a similar reduction in the emotionality of the auditory and visual images – there was no modality-specific interference. In terms of vividness, there was some evidence that the decrease in vividness was slightly larger when the modality of the image and task were matched; although effects were smaller than those found by Kemps and Tiggemann. A second experiment replicated the procedure with a larger sample and found clearer evidence against an effect of modality. Specifically, the effect of the EM and auditory tasks on vividness and emotionality did not differ depending on the modality of the image held in mind. This use of pictures and sounds as the basis for imagery by Mertens et al. (2020) addresses the limitation of previous research in which the modality of the image may have contained sensory information of multiple modalities. However, it could be argued that the use of imagery for recently presented pictures and sounds may not provide an insight into the contribution of modality-specific WM systems when the target image is based on autobiographical memory. Their study nevertheless compliments the findings of several studies mentioned above that found no effect of modality when studying negative autobiographical memories.

In summary, research on the WM hypothesis shows that the vivid recollection of emotional events is disrupted by both concurrent visuospatial and verbal/auditory tasks. Of the studies that suggest this disruption is caused mainly by the general-processing demands of a task, most are inconclusive because of methodological limitations: either the central executive task load of the tasks was not measured or matched, or the sensory characteristics of the memories was not experimentally controlled, manipulated or reported. Three EM studies have controlled both task modality and recall modality, although only two had sufficient power. These studies (Kemps & Tiggemann, 2007; Mertens et al., 2020) offer conflicting evidence about the importance of matching the modality of the task to that of the image. One suggesting that modest, additional reductions in distress can be achieved by selectively disrupting the main sensory aspects of the recalled negative episode (Kemps & Tiggemann, 2007, experiment 2), whereas the other suggests the extent to which dual-tasking reduces the vividness and emotionality of the image is unaffected by the modality-specific WM demands of the task. Therefore, the current evidence base does not support replacing existing dual-attention stimuli with simple visual/auditory tasks, as doing so is likely to eliminate the substantial effect that taxing executive processes has on image vividness and emotionality.

Future research aimed at establishing the relative contribution of different WM systems to EM effects will require researchers to carefully select and test the WM load of the dual-tasks, while also assessing (or controlling if possible) the extent to which different sensory modalities are present in the targeted image. Such research can inform the selection of dual-attention tasks in EMDR to improve their therapeutic efficacy.

1.2.6 The long-term effects of eye movements on emotional mental imagery

There are several reasons to predict that the immediate effects of EMs on imagery will be preserved long-term. Kavanagh et al. (2001) suggested that EMs might serve as a response aid, akin to wearing gloves to handle a feared spider or snake, thus helping a client to engage in other aspects of treatment rather than avoiding thinking about the distressing episode altogether. According to this perspective, the benefit of taxing WM during recall is limited to the desensitization of the target image *during* the dual-task procedure. Another possibility is that EMs provide a strong contextual cue that helps integrate the trauma memory with the broader memory network (Brewin, Gregory, Lipton, & Burgess, 2010). Research on memory consolidation and reconsolidation suggests a third mechanism: EMs might not only change the way the original memory is processed and retrieved, but could permanently change the memory.

Memory consolidation refers to the process by which information during encoding becomes a more stable memory trace. Importantly, the recall or reactivation of consolidated memories can cause them to re-enter a labile state, in which they are temporarily susceptible to the influence of new learning. Reviewed in greater detail elsewhere (Agren, 2014) is evidence that behavioural and pharmacological interventions during this brief reconsolidation window can cause the original memory to become re-encoded into long-term memory in a diminished or altered form (Nader, Schafe, & LeDoux, 2000). A recent study in mice, for instance, demonstrates that increased activity in memory-specific hippocampal neurons during exposure treatments - indicative of reconsolidation - is linked to a reduction in fear (Khalaf et al., 2018). Additionally, in humans, James et al. (2015) found that participants experienced fewer intrusive memories of a trauma film if they had performed Tetris shortly after recalling scenes from the film – during reconsolidation – compared to participants who did not receive interference after memory reactivation (see also Schiller et al. 2010). These findings support the hypothesis that trauma memories can be permanently weakened if manipulated during the reconsolidation window.

It can be argued that EMDR represents a reconsolidation procedure, whereby an aversive memory trace is recalled into a labile state, degraded by concurrent task performance, and reconsolidated in an attenuated form. If so, the immediate effects of WM interference on the target memory and associated image should be long lasting. In the following sections, I evaluate support for the hypothesis that EMs permanently alter trauma memories in EMDR via memory reconsolidation. To this end, I evaluate evidence that EMs cause lasting changes to the vividness and emotionality of imagery against the standards required to demonstrate memory updating via reconsolidation.

Research suggests there are certain boundary conditions to reconsolidation. The necessary conditions for updating of traumatic memories have been discussed elsewhere (Treanor, Brown, Rissman, & Craske, 2017). For example, memories only become susceptible to updating when they are cued by specific reminders, and when the current situation creates disparity between what is anticipated and what is currently experienced, otherwise known as a prediction error (for a review of evidence, see Fernandez, Boccia, & Pedreira, 2016). Other research suggests older memories tend to be more resistant to updating (Eisenberg & Dudai, 2004; Elsey & Kindt, 2017), yet this boundary condition can be overcome if sufficient time is spent reactivating the memory trace (Suzuki et al., 2004). Extended periods of reactivation alone, however, come at the risk of impeding reconsolidation, as an individual will become able to reliably anticipate the outcome of retrieving the memory. There is also a temporal boundary in which reconsolidation can occur, with evidence suggesting a six hours window after retrieval in which memories can be updated (Nader, Schafe, & LeDoux, 2000), though this is based on animal models of fear conditioning and may not apply to the reconsolidation of human autobiographical memories. Reconsolidation studies use specific cueing techniques and find that memories become labile only when they are cued by a brief reinstatement of the original encoding context. Research by Hupbach, Hardt, Gomez, and Nadel (2008, experiment 3) indicates that imaginal exposure to the original encoding context may be insufficient to reactivate the memory into a labile state; re-exposure to the original spatial context in which the memory was encoded may be necessary (experiment 1 and 2).

That EMDR therapy reportedly produces lasting changes to traumatic memories suggests its treatment protocols (Shapiro, 2018) satisfy the necessary conditions for memory reconsolidation. As mentioned earlier, the aversive memory trace recalled in EMDR may enter into a labile state and following concurrent WM interference, undergo reconsolidation in an attenuated form, preserving the immediate effects on the vividness and emotionality of the memory. Firstly, although the memories clients process in EMDR are often well-established representations of events from years ago, given the substantial duration clients spend reactivating the memory during the assessment and desensitisation phases of therapy, it is unlikely the age of the target memories would prevent successful reconsolidation. Furthermore, the time between activation of the target memory and the performance of bilateral stimulation occurs within the crucial six-hour window when the memory would be susceptible to the effects of WM interference. Lastly, the novelty of the EM procedure in EMDR and the reduction in vividness and emotionality of the trauma image may generate the sort of prediction error required for memory updating to occur. However, that EMDR does not involve re-exposure to the original context in which the trauma occurred may pose a barrier to the preservation of the therapeutic changes to the trauma memory. Evidence from studies in which the effects of EMs were studied outside of the reconsolidation window offer mixed evidence that EMs cause long-term effects on imagery. However, the results of these studies must be considered in the context of research on the boundary conditions to reconsolidation, and related standards on studying reconsolidation in humans.

Elsey, van Ast, and Kindt (2018) have outlined a useful framework for investigating reconsolidation in human samples. First, they suggest that reactivation of the memory and reconsolidation-manipulation should occur at least 24-hours after encoding, to ensure the memory is fully consolidated. Most of the EM studies reviewed so far, and those discussed below, meet this criteria as they investigate memories that are at least a week old. Relatively few, however, use a sufficiently long delay between the concurrent task manipulation and follow-up measures to establish a lasting effect outside the reconsolidation window - Elsey et al. suggest a delay of at least 24 hours, with follow-up measures preferably taken after sleep. Second, Elsey et al. state that evidence of memory updating requires that researchers demonstrate updating only occurs when the memory is reactivated and the experimental manipulation is performed, but not when reactivation or the manipulation occur separately (James et al., 2015); evidence of updating in such instances indicate non-specific effects. Almost all of the EM studies reviewed in this chapter have included a no-task control condition (i.e. reactivation only), but few have tested the interaction between reactivation and manipulation (van Veen et al., 2016), and none have tested this interaction outside of the reconsolidation window.

To summarise, the concurrent recall and EM procedure used in EMDR and laboratory EM studies meets several, but perhaps not all of the conditions necessary for the immediate effects of EMs on emotional memories to be reconsolidated. If memories are reconsolidated in EMDR, this should lead to long-term benefits in terms of the reduction in distress caused by the trauma memory, due to a lessening of its perceptual similarity to the actual trauma. In laboratory settings, reconsolidation of EM effects would be evidence by finding the immediate effects of EMs are maintained after at least 6 hr (or preferably after 24 hr), and are only present at follow-up if memory recall and EMs were performed together, but not if they were performed separately. Below I summarise previous laboratory studies that have tested the long-term effects of EMs on distressing memories.

Kavanagh et al. (2001) found no difference between recall with EMs and exposure in terms of the vividness and emotionality of emotional memories assessed one week after the intervention, despite EMs providing greater benefits during dual task interference. This may indicate only temporary benefits for EMs, however only short EM trials were used (8s each) meaning the WM ‘dosage’ may not have been great enough to produce a lasting impact. Follow-up measurements were also made by telephone and this change in procedure may have reduced sensitivity. Lilley et al. (2009) similarly failed to find lasting EM effects at one week post-intervention, but as with earlier studies there was only a telephone interview for follow-up and EM trials were brief.

Lee and Drummond (2008) provide support for lasting effects of EMs. Using both a longer follow-up delay and greater task dosage, they observed significantly reduced self-reported distress when emotional memories previously recalled during EMs were retrieved again one week later, although effects on vividness were less pronounced. The exact nature of the follow-up assessment in this study is unclear. Schubert, Lee, and Drummond (2011) found the same pattern of results when trauma memories were assessed by telephone 1-week after receiving EMDR (with or without EMs). Participants in this study performed additional EMDR procedures between post-task and follow-up assessments, and these procedures may have contributed to the long-term effects of disrupting memory recall.

We are aware of four studies that appear to have used demanding dual-task procedures, assessed memory outcomes after a sufficiently long follow-up delay, and appear to have used similar procedures to assess memories at baseline and follow-up. Two of these studies show sustained EM benefits. Leer et al., (2014) found that compared to recall alone, recall while making EMs led to significantly greater reductions in memory vividness and emotionality 24 hours later. (Gunter & Bodner, 2008) found sustained EM benefits when participants repeated recall procedures a week later, though memory vividness recovered slightly. In contrast, Littel et al. (2017) found substantial reductions in vividness at 24-hour follow-up, but no benefit for EMs over recall-alone. However, in this study, EMs were combined with an additional intervention (propranolol) which the authors anticipated would block the effects of EMs on image vividness from being found at follow-up. van Veen, van Schie, van de Schoot, van den Hout, and Engelhard (2019) similarly found an overall reduction in negative memory vividness and emotionality at post-task and 24-hours later, but no significant added effect of EMs compared to memory exposure alone. It is noteworthy that these studies also used the same testing environment on each occasion the memories were rated (see below).

As it stands there is mixed evidence whether performing EMs during recall causes lasting effects on the quality of negative autobiographical memories. Two of the three studies to have found evidence of long-term changes used the same testing conditions at baseline and follow-up; while this is important for experimental sensitivity, it raises the question of whether any lasting effect of EMs is limited to the context of the original intervention. Future research on reconsolidation as a mechanism of action should assess the effects of EMs across multiple recall settings as well as multiple time points; failure to find generalisation of effects across contexts or time would call into question whether the memory has been permanently changed. Efforts should also be made to assess the long-term effects of different competing tasks that are used in EMDR, and to assess the effects on memory using a wider range of outcomes, e.g., changes in trauma symptom severity, objective changes in emotional arousal. Finally, future experimental tests of the lasting effect of EMs on trauma memories should use reminders of the original trauma to cue recall, rather than participants' free recall.

1.2.7 Summary of research on the WM hypothesis on EMDR

The WM hypothesis of EMDR claims EMs and other dual-attention stimuli contribute to the effectiveness of EMDR by taxing the limited WM resources that allow emotional events to be vividly re-experienced through mental imagery. Support for the WM hypothesis comes from evidence that 1) WM is important for generating and maintaining mental imagery 2) cognitive tasks such as EMs tax WM resources 3) mental images become less vivid and emotional under dual task conditions 4) competing tasks such as EMs affect imagery in a dose-dependent way 5) the effects of dual-tasks on emotional imagery can be partly explained by individual differences in WM capacity. However, the findings of several studies raise doubts about the validity of the current WM model as an explanation for the therapeutic outcomes in EMDR.

Firstly, evidence is mixed regarding the long-term effects of concurrent WM interference on negative memories. The quality of this evidence is varied and there are reasons to question whether studies meets the standards required to demonstrate reconsolidation of the immediate effects of dual-tasking on the vividness and emotionality of distressing memories.

Secondly, the results of several studies raise doubts about the close relationship between the effects of EMs on the vividness of imagery and the subsequent reduction in emotionality. Some studies, for instance, have found effects of EMs on image vividness but not emotionality, while other research suggests the emotional benefits of EMs may not be caused by their impact on image vividness. If the detail of the image in WM is not strongly related to its emotionality, this raises questions about whether the WM interference caused by EMs desensitises emotional responding to the trauma memory in EMDR. In chapter 8, I explain how degrading the trauma image via WM interference may contribute to other therapeutic processes in EMDR, without necessarily having to reduce the emotionality of imagery.

A third limitation of the existing evidence for the WM hypothesis is that there are important differences between the procedures used in laboratory EM research and those used in EMDR. For example, several components of the trauma memory are held in mind during EMs in EMDR, whereas participants in most studies on the hypothesis focus only on a mental image associated with the memory. Relatively little effort has been dedicated to understanding how the WM interference caused by the dual-attention tasks in EMDR effect other components of memory, such as thoughts and beliefs (see chapter 9 for a review of this evidence). Another key difference in laboratory studies is that they exclude the free association component of EMDR. As I explained earlier, in a typical session of EMDR, clients often notice entirely new images, thoughts, bodily sensations and emotions during and after sets of EMs. Rather than ask the client to mentally reconstruct the original memory – which is usually what happens in laboratory studies - EMDR therapists encourage the client to use whatever information comes to mind during the next phase of dual-tasking. The decision to exclude free association under experimental conditions allows researchers to make clearer conclusions about the effects of loading WM on the target held in mind, but at the expense of creating a scenario that is unlike EMDR. Consequently, most of the evidence regarding the WM model is only relevant in understanding how EMs desensitise the target memory. There is reason to suspect that the WM model can explain the contribution of dual-tasking during other processes in EMDR, such as free association and the integration of positive and future templates (see chapter 8 and 9 for further discussion).

Lastly, fundamental predictions of the WM hypothesis regarding the contribution of modality-specific WM stores to the effects of dual-tasks in EMDR are not clearly supported in the literature due to important methodological limitations in several key studies. These limitations, namely the failure to match the general cognitive demands of the dual-tasks and/or failure control for the sensory features of the target image have led to spurious claims that the effects of EMs on imagery depends in part of competition for modality-specific WM resources. Addressing these limitations is important, as there are potential implications for EMDR practice. Specifically, if the modality-specific WM load of dual-attention tasks is important, this affords the possibility to design tasks that interfere effectively with the sensory features of imagery, but that spare central executive resources for engaging in other important processes in EMDR, such as free association.

The aim of this thesis is to solve the problem regarding the modality-specific versus modality-general hypothesis of EMDR, and to address the longstanding question of how WM interference may contribute to the reprocessing of trauma memories in EMDR. To this end, chapters 2 describes the general procedure that was used to test the modality-specific hypothesis in a series of five experiments (chapters 3-7). I argue that methods used in these experiments improve on several previous studies, by attempting to match the general WM demands of the experimental tasks as well as the sensory modality of the target image. Additionally, in chapter 8, I propose a revision to the existing WM model of EMDR that explains how the immediate effects of WM interference on imagery may facilitate the free association process in EMDR, and thereby contribute to the reprocessing of trauma memories in EMDR. To test this theory, and more generally the contribution of EMs to reprocessing, I present a novel procedure that provides a new framework for investigating the role of a WM mechanism in EMDR.

2 Chapter 2: concurrent tasks and memory recall procedure in experiments 1-5

It is important to establish which WM processes underlie the benefit EMs in EMDR. This knowledge could potentially be used to improve the effectiveness of existing dual-attention tasks, or used to develop more efficient tasks. For example, if the role of EMs in EMDR is to prevent the vivid rehearsal of negative imagery in visuospatial WM (Andrade et al., 1997), then EMs should be used instead of binaural tones when the client's image is mainly visual. Furthermore, if the modality of the competing task is important, new tasks can be designed that interfere effectively with imagery by loading heavily on visuospatial/auditory WM, but that are simple enough not to tax the central executive. The benefit of using simple tasks is that they should not interfere with other parts of therapy that rely on the central executive, such as retrieving trauma-related information between sets of EMs (see chapter 8). In summary, there are potentially important implications for the delivery and effectiveness of EMDR from investigating if the visuospatial WM load of EMs contributes to their impact on negative imagery.

I argued in chapter 1 that there is limited evidence that EMs interfere with negative imagery in EMDR by taxing visuospatial WM. Some of the studies showing that EMs reduce the vividness and/or emotionality of distressing imagery more than auditory task failed to ensure the tasks were matched in terms of their general cognitive load. Consequently, it is possible the effect of EMs in these studies was due to interference with central executive processes that support vivid imagery. Additionally, in several EM studies, the participants were not instructed to focus on the visual details of a negative memory, which means we cannot be certain that EMs were interfering with imagery held in visuospatial WM. We carried out a series of experiments that addressed these limitations, the findings from which are described in chapters 3-7.

In experiments 1-5, we explicitly instructed participants to form a visual mental image of distressing autobiographical memory. By instructing participants to generate a visual image, this meant the visuospatial interference caused by the EM should interfere with the storage and rehearsal of imagery in visuospatial WM. Additionally, we asked participants to hold this image in mind while performing an EMs task or an auditory task that was closely matched in terms of general cognitive load. In the remainder of this chapter, I will describe the EM and auditory tasks and the imagery instructions using in experiments 1-5. I will also summarise the general procedure, which was similar for all experiments, and highlight the notable differences between the experiments. Briefly, key differences were the study design (between or within-subjects) and the number of times participants were asked to recall and rate the distressing image (pre-post only, or also between blocks of dual-tasking).

2.1 Interference tasks used in experiments 1-5

The EM and auditory tasks used in experiments 1-5 were those used by Boomsma (2013), where the EM task was based on earlier work by Andrade et al. (1997). The EM and auditory tasks were designed to load visuospatial and verbal WM, respectively, while placing similar demands on central executive resources by requiring the same response speed, type of decision, and response modality. Pilot data indicated the these tasks are similar in terms of their self-rated difficulty (Boomsma, 2013), although validation against objective measures of executive function is required (for example, see Mertens et al., 2020). We also used a third task, central fixation, to control for the effect of dual-focus of attention the distressing image.

For the EMs task (Figure 1), the computer displayed a striated pattern of vertical black and white lines (15 mm wide) spanning the width of the screen. These lines were included to increase the visual WM load of the task. During the task, a single letter (black font; bold type; 5 mm in height) appeared on alternate sides of the computer screen (26 cm width) for 300 ms, with an inter-stimulus interval (ISI) of 200 ms. Letters were targets or a visually similar fillers (respectively, targets were v, q, d, n and fillers were w, p, p, m). Participants were instructed to move their eyes in order to fixate on the letter, while keeping their head still, and to press the space bar as soon as a target appeared. The purpose of the target-detection component was to ensure participants would pay attention to the task and that they made EMs in order to detect the targets. To determine if participants were paying attention to the task, the computer recorded when a button press was made and whether the response was valid (response to target within 150 - 2000 ms after target onset) or invalid (outside the valid response interval, or a response to a filler letter). There were two targets and 40 filler letters on each task block. Targets were presented pseudo-randomly and were not used in the first five and last three trials in a task block. There were two practice blocks (containing the letters v and w) and six test blocks (letters q and p were used in blocks one and four; letters d and p in blocks two and five; and letters n and m in blocks three and six). At the start of each task block, an example of the target letter was presented on screen for 3 s alongside the message "press the space bar when you see the target letter".

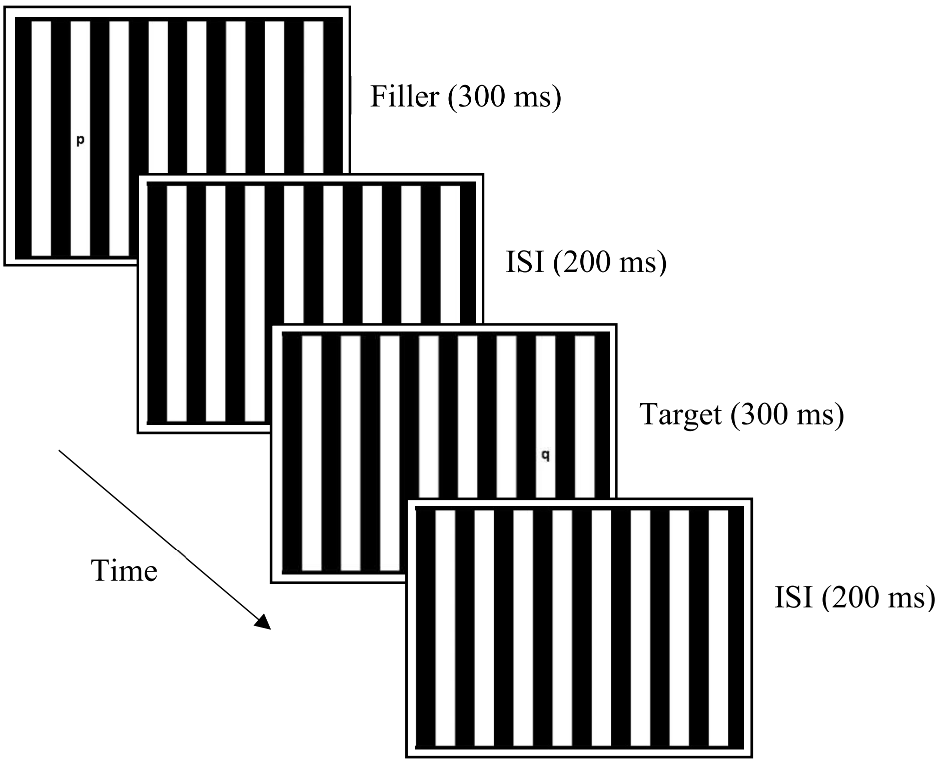

Figure 1: an illustration of the EM task used in Experiments 1-5.

The concurrent auditory task was designed to have a similar structure and cognitive load to the EMs task (Boomsma, 2013). Rather than seeing the letters on screen, letter sounds were played through headphones to both ears simultaneously. We used simultaneous rather than alternating presentation to avoid taxing spatial WM resources. Likewise, to minimise visual interference, the background display was white and a small black fixation cross was included in the middle of the screen to minimise EMs. Participants were instructed to look at the middle of the screen and to press the space bar as soon as they heard the target. Letters Targets and filler letters (targets: v, b, d, n and fillers e, p, p, m, respectively) were recorded as sounds files in a male and female voice. The gender of the speaker was sampled randomly by the computer. This variation was included to ensure that targets could not be identified automatically as 'odd balls' against a background stream of identical repeated sounds. Each letter was presented for 400 ms, with an ISI of 200 ms. The stimulus onset-offset was longer than in the EMs task to ensure that letter sounds were clearly discernible. So that blocks of the EMs and auditory task were the same duration, only 32 filler letters and two targets were presented per task block in the auditory task. Targets appeared pseudo-randomly, avoiding the first five and last three trials in a block. There were two practice blocks (containing the letters e and v) and six test blocks (letters b and p were used on blocks one and four; letters d and p on blocks two and five; and letters n and m on blocks three and six). Consistent with the EM task, at the start of each task block in the auditory task, an example of the target letter was presented on screen for 3 s alongside the message "press the space bar when you hear the target letter". Example targets were presented visually because presenting them in a male or female voice may have caused gender to become part of the rule i.e. only respond to a male saying 'p' – participants were required to respond to targets regardless of the speaker's gender.

For the central fixation task, participants looked at small black cross in the middle of the computer screen. Similarly to the EM and auditory task conditions, the fixation task consisted of eight 20 s task blocks (two practice and six experimental). A message appeared for 3 s at the start of each block presented on the computer screen, which read "continue looking at the middle of the screen".

2.2 Memory recall and imagery instructions

Continuing with the definitions used in chapter 1, the term 'memory' hereafter refers to the autobiographical memory that participants recalled, while 'image' describes the sensory representation of that memory held temporarily in WM.

The memory recall and image generation procedures in experiments 1-5 were similar to those in previous EMDR studies. Participants were first instructed to recall a negative memory using instructions from Andrade et al. (1997): "recall one occasion that has made you feel very fearful, anxious, or distressed and that still has an emotional impact when you think of it now (e.g. witnessing an accident or going unprepared to an examination)". Participants were then instructed to form an image of this memory using instructions adapted from Kemps and Tiggemann (2007): "focus exclusively on the visual aspects of the memory i.e. anything you can see. Notice the image that the experience brings to mind, and only the image. Try to bring this image to mind as clearly and as vividly as if you were actually seeing it".

2.3 General procedure and key differences between experiments 1-5

In experiments 1-5, participants were instructed to generate a visual mental image which they then held in mind while performing a concurrent task. Immediately after the dual-task procedure, they were asked to rate the image in terms of its vividness and emotionality, as well as how the image made them feel on five emotional domains: upset; hostile; ashamed; nervous; and afraid. A common procedure used in previous EM studies involves asking participants bring the target image to mind for around 10 s after dual-tasking before re-rating it (e.g. van den Hout et al., 2001). While such studies have found an effect of dual-tasking on imagery, we reasoned that instructing participants to engage in brief imaginal exposure would reduce allow the image to recover, therefore reducing the likelihood of detecting an effect of taxing WM. We reasoned that providing the opportunity to regenerate the image might allow the contents of WM to be refreshed with information stored in long-term memory. We therefore instructed participants to rate their image as soon as dual-tasking had ended, as we predicted this is when effect of dual-tasking on imagery would be larger.

The key negative memory recall procedures in each experiment are shown in Figure 2 below. Experiment 1b used a between-subjects design. Participants rated a distressing mental image before and after performing the EMs, auditory, or fixation task. Establishing a clear effect of dual-tasking on imagery in experiment 1b turned out to be less straightforward than anticipated from the literature reviewed in chapter 1. In experiment 2b, we removed the requirement for participants to rate their mental image at baseline in case this had influenced the ratings taken at post-task. Additionally, follow-up ratings were taken after a brief period of imaginal exposure around 20 s after dual-task, and again after a 24 hr delay. These additional ratings were used to replicate the procedures used in previous EM studies and to determine how long an effect of dual-tasking would last. In experiment 3, we re-introduced a baseline rating and used a within-subjects design to increase the power of the study. In experiment 4, we introduced additional ratings of imagery, whereby participants stopped several times during the concurrent task to re-rate their image. In experiment 5, we repeated the procedure from experiment 4 using a between-subjects design, to test our observation that the repeated rating procedure in experiment 4 made improved the sensitivity of the study to the effects of our experimental tasks.

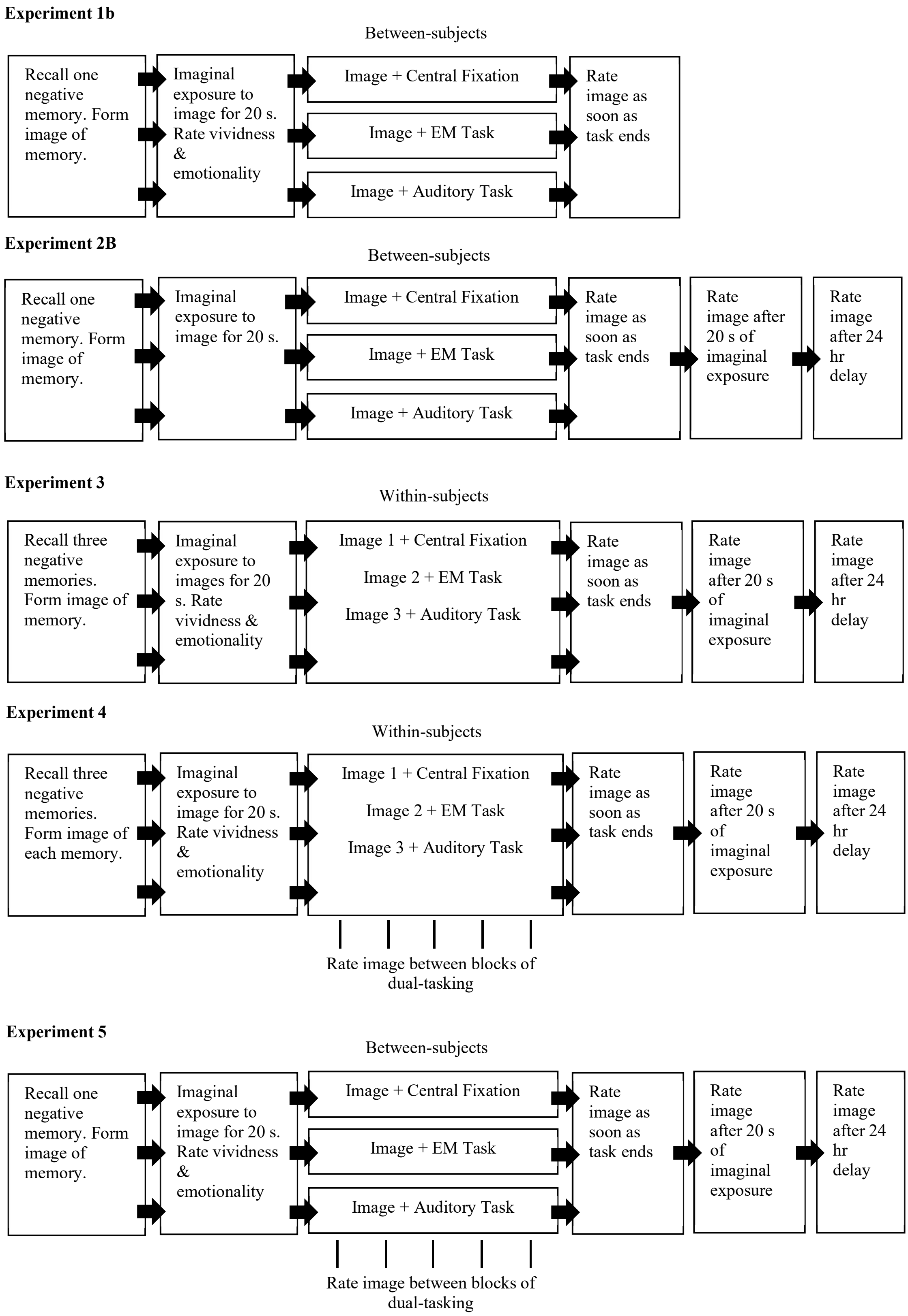

Figure 2: visual representation of the negative recall procedure used in experiment 1b, 2b, 3, 4 and 5.

3 Chapter 3: Comparing the impact of auditory and eye movement interference on negative image recollection

Andrade et al. (1997) proposed that EMs reduce the vividness and emotionality of negative mental imagery in EMDR by loading on the central executive and visuospatial sketchpad of WM. In contrast, Gunter and Bodner (2008) argued that the effects EMs have on imagery can be explained by their general cognitive load alone. As I explained in chapter 1, if the visuospatial load of EMs contributes to their effectiveness in EMDR, it should be possible to improve the effectiveness of EMDR by developing tasks that interfere with imagery because they load heavily on visuospatial and verbal WM, and that preserve executive resources for other processes in EMDR because they generate a small general cognitive load. Previous studies have investigated Andrade et al's hypothesis; however, as I discussed in previous chapters, some of these studies contain important methodological limitations. The aim of experiment 1 was therefore to address the limitations of these previous studies, using the methodology described in chapter 2, in order to test the hypothesis that the effects of EMs on imagery are due in part to interference with visuospatial WM.

Experiment 1 improves on previous EM studies in two ways. First, we instructed participants to form a negative mental image that contained only visual information. A limitation in previous studies is that participants were not instructed to focus on the visual details of the negative image during EMs, but instead were given general instructions to recall and imagine a distressing autobiographical memory. The issue with the instructions used in previous studies is that participants may have focussed on other sensory aspects of the memory during the EMs. If participants generated an image that contained little visual detail, this image would have been less susceptible to interference from the visuospatial load of the EM task, and any reductions in image vividness and emotionality would therefore be attributable to the general cognitive load of the task. The instruction to focus only on the visual aspects of the memory in experiment 1 meant that we can be more confident that the images participants generated were susceptible to the visuospatial WM interference caused by our EM task. Experiment 1 also improves on previous studies by attempting to control for the general cognitive load of the EM and auditory tasks. As discussed, previous studies have tested the modality-specific interference hypothesis by comparing the effect on imagery of an EM and auditory task, but without ensuring the tasks placed similar demands on the central executive. We attempted to control the effect of taxing the central executive by comparing EMs to an auditory task that had been closely matched in terms of their general cognitive demands (see chapter 2). By comparing tasks that were presumably more closely matched for general cognitive load, the aim was to test if the visuospatial WM load of the EM task would cause additional reductions in image vividness and emotionality. Additionally, in experiment 1a, we used objective measures of the visual and verbal STM to establish that the EM and auditory tasks selectively interfered with the visuospatial sketchpad and phonological loop. I explained in chapter 1 that van den Hout and colleagues have measured the central executive load of their interference tasks to determine if the effect of these tasks on imagery was due to their general cognitive demands (Engelhard et al., 2011; Engelhard, van Uijen, et al., 2010; Van den Hout et al., 2011b; Van den Hout et al., 2011a). In the current experiments, objective measures of visual and verbal STM were used to confirm that the EM and auditory tasks would interfere selectively with the storage of negative imagery in visuospatial WM. To our knowledge, this is the first time in the EMDR literature that the modality-specific WM load of the interference tasks has been measured. We would argue that checking how the interference tasks load WM is important when the broader aim is to test how the WM demands of these tasks interfere with imagery.

To summarise, experiment 1 had two aims: to check that our experimental tasks selectively load on visuospatial and verbal WM; and to test if the visuospatial WM load of the EM task interferes with distressing visual imagery more than the auditory and fixation tasks. To help the reader understand the procedure of experiment 1, the method and results for the STM tests (experiment 1a) and negative imagery procedures (experiment 1b) are reported separately. NB. experiment 1a and 1b were performed by the same participants and were separated by an optional 2 min rest period. Unfortunately, due to an error in the computer program used to display the stimuli for the auditory STM test, only data for the visual STM test were analysed. Further details of this error and how this was resolved will be discussed.

3.1 Experiment 1a: Measuring the WM demands of the interference and control tasks.

The aim of experiment 1a was to objectively measure the WM demands of the EM, auditory and fixation tasks described in chapter 2. Participants performed each task while retaining a briefly presented visual matrix, or a string of letter sounds for later recall. We predicted that recall accuracy for both visual and verbal stimuli would be impaired by performing the EM and auditory tasks during retention of the stimulus compared to the central fixation task. Crucially, we predicted an interaction between test and task modalities: we expected recall accuracy for visual stimuli to be lowest in the concurrent EMs condition, while verbal span accuracy was expected to be lowest in the auditory task condition. This cross-over interaction would support our assumptions that the EM task selectively loaded the visuospatial sketchpad of WM and the auditory task selectively loaded the phonological loop.

3.1.1 Participants

The sample size was based on power analysis for experiment 1b, presented later in this chapter. Again, the same participants were used in experiments 1a and 1b. We recruited 120 participants (86 female; modal age was between 18-21 years) through the University of Plymouth in exchange for course credits or payment. The sample consisted mainly of psychology undergraduates and also included members of the public. The majority of participants were native English speakers (84%).

3.1.2 Materials

The EM, auditory and fixation interference tasks used in experiment 1 were those described in chapter 2. The visual and verbal STM tests were adapted from Andrade, Pears, May, and Kavanagh (2012). All stimuli were presented on a 22 in. computer monitor. Participants responded using computer mouse and keyboard.

Stimuli for the visual memory test were 24 matrices (allowing two practice and six recorded blocks per task condition). Each matrix was arranged in a 4x4 array (measuring 11.5 cm^2). Half of the matrices in each task condition contained seven black squares and half contained eight squares, which were presented alternately[2] across experiment blocks. The position of the black squares within each matrix was pseudo-random, avoiding obvious patterns, such as an L shape, and ensuring no two matrices were the same. Memory accuracy was recorded using a blank 4x4 matrix – participants could add or remove a black square by clicking on cells within the matrix. One point was awarded for each square recalled in its original location. Points were not deducted for omissions or errors (recalling a square in a location that was originally blank).

[2] This was an oversight, as the order was meant to be random. While this may have reduced the WM load of the test, participants did not perform at ceiling on the visual STM test. Furthermore, as the analysis described later shows, it was still possible to detect an effect of task condition i.e. the test was not so easy that it masked the effect of dual-task modality on STM test performance.

Stimuli for the verbal memory test were 24 strings of consonants, each six letters long. Letter strings were generated pseudo-randomly from a pool of eleven consonants (f h k l j c r s t x y), avoiding repeats and common acronyms. Letters were recorded as sound files and played through headphones to both ears simultaneously, at a rate of one letter per second. Participants responded by typing letters into a blank textbox presented on the computer screen. Participants could type up to a maximum of six letters. Each letter recalled in its original serial position was awarded one point. Points were not deducted for omissions or errors (recalling a letter not presented in the original list, or recalling a letter from the original list but in the wrong serial position).

As mentioned earlier, it was not possible to analyse the results of the verbal memory STM test due to an error in the program that presented the stimuli. This error meant that rather than each block presenting a different letter string, the same letter string was presented on blocks 1-3, and a different letter string was similarly repeated on blocks 3-6. This allowed participants to learn the letter strings through repetition, making them easier to retain without the need for subvocal rehearsal. Therefore, we were concerned this error would reduce the modality-specific interference caused by the auditory task. Analysis not reported here confirmed that in all task conditions, recall accuracy was higher when participants had heard the same letter string multiple times compared to only once. A second and more important issue was that an error in selection of the letter sounds resulted in half of the letter strings in the auditory task condition contained rhyming consonants. If two items share phonological similarity, they are harder to recall when retention relies on subvocal rehearsal. Therefore, our interference task conditions were confounded by the difficulty of the memory test, as it would have been more difficult for participants to rehearse the rhyming letter strings in the auditory task condition than in other conditions, even without dual-task interference. It is for these reasons we decided not to analyse the results of the verbal STM test.

3.1.3 Design and procedure

Experiment 1a was a fully within-subjects design in which participants performed visual and verbal STM tests under concurrent EM, auditory, and central fixation task conditions. The interference tasks were blocked by the modality of the STM test. Half of the participants were selected at random to perform the visual STM test first and half the verbal STM test first. As our sample size was 120, it was not possible to fully counterbalance the order in which participants performed the interference tasks across both types of STM test. Instead, task order was counterbalanced for the first STM test participants performed, and was randomly selected for the second. Participants performed all eight blocks of one interference task before moving on to the next. For each block, participants were given 6 s to memorise a matrix/letter string. This was followed by a retention period, during which they performed the interference task for 20 s. Next, participants had 10 s to recall and record the location of the squares in the matrix, or the sequence of letters. Figure 3 below provides an example of the procedure for the visual STM in the fixation task condition.

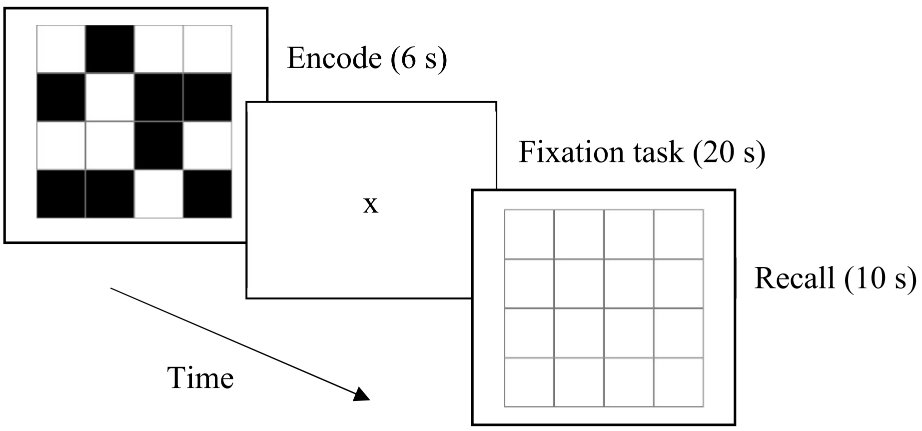

Figure 3: an illustration of the visual STM test procedure in experiment 1a.

Participants were tested in groups of up to six. To minimise distraction, participants sat in self-contained cubicles, or in an open plan room with dividing walls between each person. Participants sat approximately 45 cm from the computer monitor during testing and wore headphones throughout. The experiment brief and task instructions were presented on the computer screen, and participants provided written consent at the start of the experiment. The full procedure took approximately 35 min. An optional 5 min break was offered between the first and second STM test, and at the end of the procedure.

3.1.4 Results

As mentioned earlier, there were two errors on the verbal STM test which affected the difficulty of the task, specifically repeated letter strings and, in one condition, rhyming consonants. We therefore analysed the results of the visual STM only. This meant we could not test our prediction regarding the interaction between the modality of the STM test (visual/verbal) and the type of concurrent task. We instead tested our prediction that EMs would impair performance on the visual STM test more than the auditory task, and that both tasks would impair performance more than the fixation task, as the latter creates a smaller general cognitive load. The results of this analysis are presented below.

Figure 4 shows the mean recall accuracy for the visual STM test in each task condition. Recall accuracy on the visual STM test was defined as the percentage of correct responses per task condition. Each square that was recalled in the correct location was counted as a correct response. The maximum number of correct responses in each task condition was forty-five.

One participant did not provide a response on several blocks in the EM condition and therefore their scores for all task conditions were excluded from the analysis. Histograms and Q-Q plots showed that scores in the fixation and auditory conditions were negatively skewed, as the majority of participants performed at or close to ceiling. The distribution of scores in all task conditions was approximately normal after being transformed using an arcsine transformation. We therefore compared visual STM test performance between conditions using parametric tests.

Repeated measures ANOVA revealed that recall accuracy differed significantly between task conditions, $F(2, 118) = 86.84$, $p < .001$, $\eta p^2 = .42$. Post-hoc paired t-tests using Bonferroni correction indicated that recall accuracy was significantly lower in the EM condition than in the auditory task condition, $p < .001$, and the fixation task condition ($p < .001$). Accuracy did not differ significantly between the auditory and fixation task conditions ($p = 1.00$).

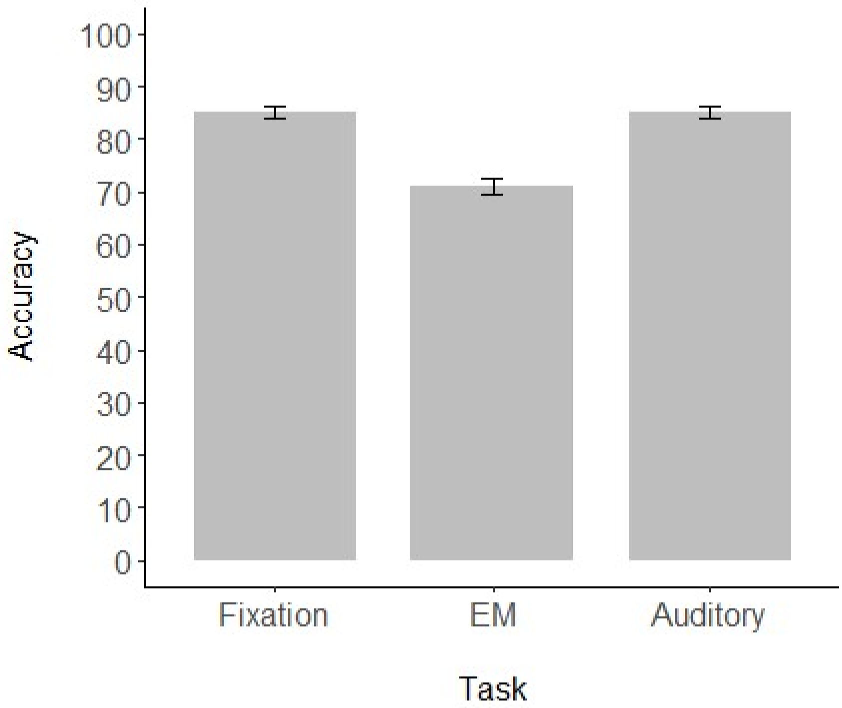

Figure 4: mean percentage of correct responses on the visual STM test in each task condition.

3.2 Experiment 1b: comparing the effect of the interference tasks on distressing imagery

The aim of Experiment 1b was to test Andrade et al. (1997) WM account of EMDR. The same participants from Experiment 1a recalled a single negative memory, formed a visual mental image of this memory, and then held this image in mind while performing one of the three concurrent tasks.

Based on research by Baddeley and Andrade (2000), we predicted that participants would report a significant reduction in the vividness and emotionality of negative imagery in both the EM and auditory task conditions. Crucially, we predicted that the reduction in image vividness and emotionality would be largest in the EM task condition compared to the fixation and auditory tasks conditions, as EMs tax both the central executive and visuospatial sketchpad.

We took the opportunity in experiment 1b to test if dual-tasking would reduce other types of negative emotion, beyond the emotionality of the target image. Specifically, participants completed ratings of negative affect before and after the dual-task procedure. Our predictions about the effect of the EM and auditory task conditions on negative affect were the same as the predictions for emotionality; however, results were merely exploratory and were not used to draw conclusions about the validity of the WM hypothesis.

3.2.1 Sample selection

A between-subjects design was used in experiment 1b to test our prediction about the effect of the EM and auditory tasks on image vividness and emotionality. At the time of testing, the effect of EMs had only been compared to the effect of an auditory control task using a within-subjects design (Lilley et al., 2009). Given that within-subjects designs provide greater statistical power, it was not appropriate to estimate the sample size needed to find a significant effect of EMs compared to the auditory task, as this would increase the risk of type 2 error in the current experiment. Therefore, we estimated the sample size required to find an effect of EMs compared to the fixation task.

Again, most EM studies have used a within-subjects design to compare EMs to a task that involves exposure-alone, or exposure while keeping both eyes stationary. Of the laboratory EM studies that have used a between-subjects design (Leer, Engelhard, Altink, et al., 2013; Smeets et al., 2012; Van den Hout et al., 2013), only Smeets et al. can be considered relevant to the current experiment – the distressing image was based on an autobiographical memory, and the dependent variable was image vividness and emotionality. Leer et al. measured the effect of EMs on imagery using skin conductance, rather than rating of subjective image vividness and emotionality. van den Hout et al. tested the effect of EMs on imagery of novel pictures that were emotionally neutral, therefore the results may not be generalizable to the effect of EMs on emotional imagery. Furthermore, EMs may interfere more easily with images of novel stimuli than those based on autobiographical memories, as the latter have undergone greater elaboration in long-term memory and therefore may be more resistant to being altered by concurrent WM interference.

Smeets et al. found that compared to focussing on a stationary circle, making EMs caused a significant decrease in the vividness ($d = 0.73$) and emotionality ($d = 0.45$) of negative imagery. While we predicted an effect of EMs on vividness and emotionality, the latter was used to estimate the effect of EMs in experiment 1b for a few reasons. As I mentioned in chapter one, the results of previous laboratory EM studies show that the effect of EMs on ratings of image vividness tends to be more reliable that the effect on ratings of emotionality. Additionally, the effect of EMs on image vividness is predicted by WM theory, whereas the effect of EMs on emotion is assumed to be caused by the effect of EMs on image vividness. Given that our aim was to test the WM account of EMDR, evidence that EMs affect image vividness was considered more important to the experiment aims. Lastly, as I discuss in later chapters, the reductions in image vividness caused by EMs may help clients to retrieve new images and memories, which Shapiro

(2001) suggests is how trauma memories become connected with new adaptive information. Put differently, evidence that EMs reduce image vividness may be just as important as reductions in emotionality in terms of understanding how EMs contribute to the therapeutic effects of EMDR for trauma memories. In summary, the sample size for experiment 1b was calculated using evidence that EMs reduce image vividness more than fixation. We focussed on vividness, rather than emotionality, as finding an effect of EMs on vividness was considered most pertinent to testing WM hypothesis, and was considered at least as important to understanding the therapeutic benefits of EMDR.

Power analysis using GPower indicated that a sample size of 93 was required to detect a significant difference between EM and fixation task conditions in terms of the decrease in image vividness, assuming $d = 0.73$ (Engelhard et al., 2011), $p = .05$, power = .80. As this estimate was based on a single result, we reviewed the wider literature to determine if our estimate was typical of most EM research. Those investigating the effect of EMs on negative imagery using healthy subjects, where the study was run in laboratory settings, have typically recruited between 20 and 40 participants per task condition (see Lee & Cuijpers, 2013). As our estimate of 31 was within the middle of this range, we decided that a sample of 120 participants, or 40 per task condition would provide sufficient power to detect an effect of EMs, while also increasing the power to detect an effect of modality, which will presumably be smaller.

3.2.2 Design

Experiment 1b was a between-subjects design. Participants were randomly allocated to either central fixation, EM, or auditory task conditions. Each condition consisted of six 20 s blocks of imaginal exposure and concurrent task performance. Blocks were performed sequentially and were separated by a brief instruction (see procedure). Participants rated the image for vividness and emotionality before the dual-task procedure – following 20 s of imaginal exposure – and immediately after the sixth blocks of dual-tasking.

3.2.3 Outcome measures

Vividness and emotionality of imagery were measured before and immediately after the dual-task procedure. The questions were "How vivid is the image in your mind right now" and "How emotional is the image right now". Participants answered by adjusting a sliding scale on the computer screen, which ranged from 0 (no image at all vivid; neutral, respectively) to 10 (image as clear and as vivid as real life; as bad as if it were happening, respectively).

Negative affect was measured before and after dual-tasking. Participants answered questions from the negative subset of the International Positive and Negative Affect Schedule Short Form (Thompson et al., 2007). The question "How does the image make you feel right now in terms of the following emotions" was displayed at the top of the screen, followed by separate rating scales for the five negative emotions: upset; hostile; ashamed; nervous; and afraid. Each scale ranged from 1 (not at all) to 5 (definitely). Again, participants adjusted a sliding scale on the computer to indicate their response.

We also asked participants about their experience of performing the concurrent task. The questions were "How difficult was the task", "How pleasant was the task", and "to what extent were you able to hold the image in your mind during the task". Each question was followed by a sliding scale, which could be adjusted between 0 (not at all difficult; pleasant; not at all, respectively) and 10 (extremely difficulty; extremely unpleasant; all of the time, respectively). Ratings of subjective difficulty were used to check if the general cognitive load of the concurrent tasks was affected by our experimental manipulation. Task pleasantness was measured because of anecdotal reports from an earlier pilot study, which suggested that participants tended to dislike performing the EM task. We hypothesised that performing an unpleasant task might lead to higher ratings of image emotionality and negative affect, even if the image is unaffected. Therefore, our aim was to assess if task pleasantness could have affected the emotional benefit of dual-tasking. Finally, there were two reasons that we measured the extent to which imagery was held in mind while dual-tasking (we refer to this measure hereafter as the ‘accessibility’ of the image). First, if an image is not held in mind, perhaps because the WM load of the competing task is too great, then WM interference should not affect the sensory detail of the image. Furthermore, if an image is entirely accessible, this might indicate the participant is either not performing the concurrent task, the WM load of the concurrent task is too low, or the individual has a large WM capacity to be able to perform the task without imagery being affected. In all cases, dual-tasking is expected to have little impact on image vividness or emotionality. Therefore, ratings of image accessibility were used to determine if our concurrent tasks should have interfered with the target image.

3.2.4 Procedure

Participants were instructed to recall a single negative autobiographical memory. The wording of the instructions was taken from Andrade et al. (1997), are read “recall one occasion that has made you feel very fearful, anxious, or distressed and that still has an emotional impact when you think of it now (e.g. witnessing an accident or going unprepared to an examination)”. Next, participants were asked to form a visual mental image of the memory. These instructions were adapted from Kemps and Tiggemann (2007), and asked participants to “focus exclusively on the visual aspects of the memory i.e. anything you can see” and to “notice the image that the experience brings to mind, and only the image”. This was followed by the instruction to “bring this image to mind as clearly and as vividly as if you were actually seeing it”. Participants were given 20 s to generate the image before completing the baseline vividness, emotionality and negative affect rating scales.

For the dual-task procedure, participants performed six successive blocks of dual-tasking. On each block, participants generated the image of their negative memory for 20 s, while simultaneously performing the EM, auditory or fixation task. There was a 5 s rest period between each block, during which time the following reminder appeared on the computer screen: “hold the image of the memory in your mind during the next task”. As soon as the dual-task procedure finished, participants re-rated image vividness, emotionality, and negative affect. They then rated the difficulty and pleasantness of the task, and the accessibility of the image during the dual-task procedure.

3.2.5 Results

Vividness

Figure 5 shows the mean vividness scores at baseline and post task in each task condition. Histograms and Q-Q plots showed that baseline and/or post-task vividness scores were negatively skewed in all three task conditions. Vividness scores continued to show significant skewness after being transformed using square and exponent transformations. We therefore used non-parametric tests to investigate if our dual-task conditions had significantly different effects on image vividness.

To test if there was an interaction between the type of dual-task performed and the extent to which image vividness had decreased from baseline to immediately after post-task, we used a type of non-parametric test known as aligned rank transformation (Wobbrock, Findlater, Gergle, & Higgins, 2011), which was performed in RStudio (RStudio Team, 2020) using the Artool package. Briefly, this method involves creating separate ranked scores for the main and interaction effects in a factorial study, which are then analysed using ANOVA. The benefit of this method, compared to traditional non-parametric tests, is that it allows the interaction between factors to be tested without inflating the risk of type-I error (Wobbrock et al., 2011).

We analysed aligned rank scores using a mixed 2 (Time: baseline rating; rating taken immediately after dual-tasking) x 3 (Task: fixation; EMs; auditory) ANOVA. Analysis revealed a significant main effect of time, $F(1, 118) = 29.067$, $p < .001$, which was due to an average decrease in image vividness from baseline. The main effect of dual-task condition was not significant, $F(2, 117) = 1.617$, $p = .202$. Crucially, there was no significant interaction between time and task, $F(2, 117) = 2.361$, $p = .099$, meaning decreases in image vividness did not vary significantly between dual-task conditions.

The power calculations used to determine the sample size for experiment 1b were based on studies in which the effect of EMs on imagery was tested using parametric methods, which are generally more powerful than equivalent non-parametric tests. This means our failure to find a significant difference between the effects of dual-tasking on image vividness may be due to a lack of statistical power. To explore if parametric tests would lead to the same conclusions about the effect of our dual-task manipulation, we also analysed vividness scores using a mixed 2 x 3 ANOVA. This analysis also revealed a significant effect of time, $F(1, 118) = 41.635$, $p < .001$, $\eta p^2 = .262$, and failed to find a significant effect of task, $F(1, 117) = 1.423$, $p = .245$, $\eta p^2 = .036$, or a significant interaction between time and task condition, $F(1, 117) = 2.208$, $p = .115$, $\eta p^2 = .036$. In summary, non-parametric and equivalent parametric tests failed to find a significant effect of dual-task condition on image vividness.

Emotionality

Figure 5 shows the mean emotionality scores at baseline and post task in each task condition. Histograms and Q-Q plots showed that in the EM and fixation task conditions, baseline/post-task emotionality scores were not normally distributed. As scores were skewed differently in different conditions, it was not possible to transform all of the scores using a same method. Therefore, we analysed emotionality scores using non-parametric tests.

In line with our analysis of vividness, aligned rank transformation was used to convert emotionality scores, which were then analysed using a mixed 2 x 3 ANOVA. This analysis failed to find a significant main effect of task, $F(2, 117) = 1.431$, $p = .243$, but did reveal a significant main effect of time, $F(1, 118) = 31.336$, $p < .001$, and a significant interaction between time and task condition, $F(2, 117) = 4.983$, $p = .008$. To investigate this interaction, Mann-Whitney U tests were used to compare the EM and fixation tasks to investigate the effect of taxing the central executive, and to compare the EM and auditory tasks in order to investigate the effect of taxing visuospatial WM. We adjusted the alpha level for each comparison by applying Bonferonni correction – we divided alpha (.05) by the number of comparisons (2), meaning the adjusted-alpha for each test was .025. Results showed that the EM task led to a significantly greater reduction in emotionality from baseline compared to the fixation task, $U = 565.5$, $p = .022$. In contrast with our prediction, the EM and auditory tasks did not cause significantly different decreases in image emotionality, $U = 750.5$, $p = .630$. Numerically, the auditory task caused a larger decrease in emotionality than the EM task, not a smaller decrease as we had predicted. This means it was not the case that there was benefit of EMs compared to the auditory task that was too small to detect given our sample size and choice of statistical analysis.

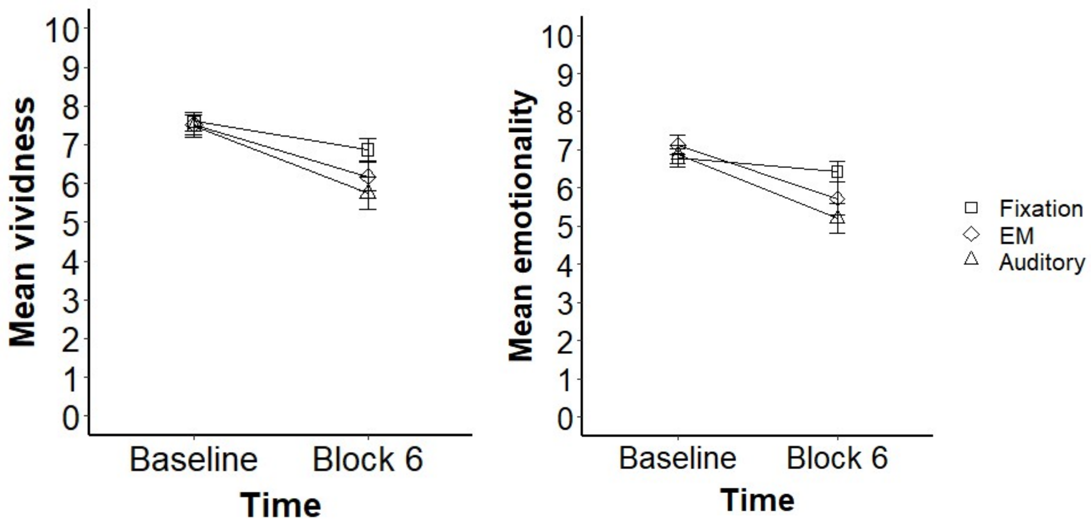

Figure 5: mean rating of image vividness and emotionality at baseline and immediately after the final block of dual-tasking in experiment 1b. Error bar represents the standard error.

Negative Affect

Negative affect was calculated by summing the scores from the five emotion items: upset; hostile; ashamed; nervous; afraid. This was in line with scoring guidelines for the Positive and Negative Affect Schedule (Watson, Clark, & Tellegen, 1988). Figure 6 shows the mean scores for negative affect at baseline and post task in each task condition.

Histograms and Q-Q plots showed that post-task negative affect scores in the EM condition were positively skewed and remained non-normal after log, square root, and inverse transformations. Therefore, we analysed negative affect scores using non-parametric tests. We converted negative affect scores using aligned rank transformation and analysed the transformed scores using a mixed 2 x 3 ANOVA. Results showed a significant main effect of time, $F(1, 118) = 43.57$, $p < .001$, which was due to an average decrease in negative affect scores from baseline. The main effect of dual-task condition was not significant, $F(2, 117) = 2.218$, $p = .113$, and there was no significant interaction between time and task, $F(2, 117) = 0.27$, $p = .764$, meaning the decrease in negative affect from baseline did not vary between dual-task conditions.

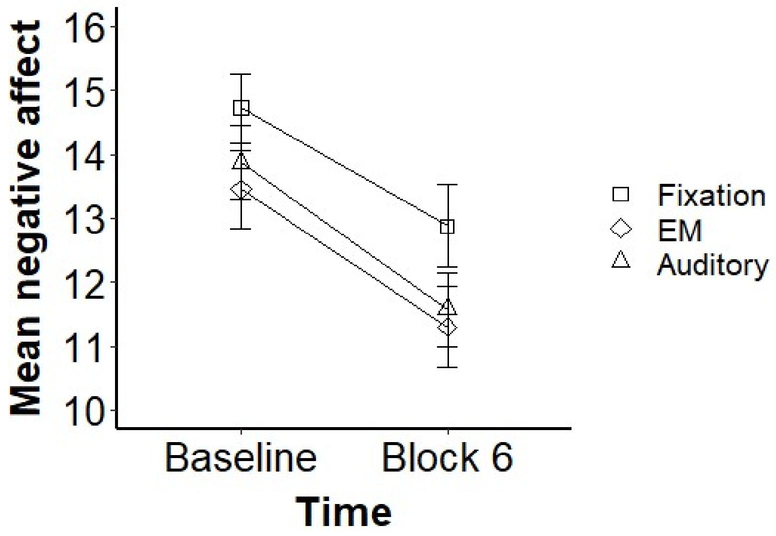

Figure 6: mean negative affect in each task condition at baseline and immediately after the final block of dual-tasking in experiment 1b. Error bar represents the standard error.

Task difficulty, pleasantness, and image accessibility

Histograms and Q-Q plots showed that ratings of task pleasantness were normally distributed in all task conditions. In contrast, ratings of task difficulty were positively skewed in all task conditions. We transformed scores for task difficulty using log, square root, and reciprocal transformations, but these transformed scores also failed to meet the assumption of normality. We therefore analysed task difficulty scores using non-parametric tests. Ratings of image accessibility in the EM and fixation task conditions also failed to meet the assumption of normality due to kurtosis, therefore we also analysed these scores using non-parametric tests.

For pleasantness scores, a one-way ANOVA failed to find a significant differences between task conditions, $F(2, 117) = 0.86$, $p = .47$, $\eta^2 = .014$. We performed separate Kruskal-Wallis H tests for ratings of task difficulty and image accessibility. There were no significant differences between the mean ranked difficulty of the dual-task conditions, H (2) = 3.62, p = .16, ε^2 = .03. Similarly, the mean ranked accessibility of the image did not differ significantly between task conditions, H (2) = 1.90, p = .39, ε^2 = .02.

Table 2: Condition means (and standard deviations) for ratings of task difficulty, task pleasantness, image accessibility.

	Fixation	EM	Auditory
	M (SD)	M (SD)	M (SD)
Task difficulty	2.75 (2.78)	3.575 (2.571)	3.6 (2.609)
Task pleasantness	4.925 (2.841)	4.15 (2.723)	4.775 (2.842)
Image accessibility	6.35 (2.527)	6.05 (2.501)	5.55 (2.459)

3.3 Discussion of experiment 1

The current experiment had two aims. The first was to test Andrade et al. (1997) hypothesis using a method that addressed the limitations of previous studies. Specifically, participants in experiment 1b were instructed to generate a distressing mental image that contained only visual information, and then held this image in mind while performing an EM task, an auditory task designed to place similar demands on the central executive, or a fixation task (focussing on a stationary cross). We predicted that compared to fixation, performing the EM and auditory tasks during imagery would cause a larger decrease in image vividness and emotionality, due to greater interference with the central executive. Crucially, we predicted that EMs would affect imagery more than the auditory task because the EM task should interfere with imagery in visuospatial WM, whereas the auditory task should load verbal WM. The second aim of this experiment was to objectively confirm that our EM and auditory tasks selectively loaded on visuospatial and verbal WM, respectively.

Experiment 1a showed that our EM task interfered more with visuospatial WM performance than the auditory task, supporting our assumption that the EM task loaded more heavily on visuospatial WM resources. Unfortunately, we were not able to show that the converse held for the auditory task due to an error in programming the verbal STM test. We addressed this error in experiment 2 (chapter 4). Based on the results of experiment 1a, we can conclude that EMs either load visuospatial WM more than the auditory task, or that EMs load central executive resources more than the auditory task. Either way, the results of experiment 1a would lead us to expect that the EMs task would reduce vividness and emotionality of visual imagery more than the auditory task.

Experiment 1b showed that, consistent with predictions, image vividness decreased over time. However, there was no predicted effect of task condition on this change in vividness from baseline to post-task: the decrease was similar for fixation, auditory and EMs. Therefore, the greater demands that EMs place on visuospatial/central executive resources relative to the fixation and auditory tasks did not appear to contribute to larger changes in image vividness.

The results for emotionality were somewhat more in line with predictions. Emotionality ratings decreased more from baseline to post-task in the auditory and EMs conditions than with fixation. Crucially, we failed to find a difference between the effect of the EM and auditory task on emotionality. Indeed, the auditory task was numerically, but not statistically associated with a slightly larger decrease in image emotionality than the EM task. This pattern of results is evidence against our prediction that imagery would be affected significantly more by EMs, due to their greater visuospatial demands.

The results of experiment 1b contradict previous research on the WM hypothesis of EMDR. Most of the EM studies that have been carried out in laboratory settings have found larger decreases in vividness and emotionality when the images of a negative autobiographical memory is held in mind while making EMs, compared to exposure without EMs (Barrowcliff et al., 2004; Gunter & Bodner, 2008; Hornsveld et al., 2010; Kavanagh et al., 2001; Kemps & Tiggemann, 2007; Kristjánsdóttir & Lee, 2011; Leer et al., 2014; Van den Hout et al., 2014; Van den Hout et al., 2001; van Schie et al., 2016; van Veen et al., 2016; van Veen et al., 2015). Our results are more in line with studies that have found EMs selectively reduce image emotionality but not vividness (de Jongh et al., 2013; Engelhard, van Uijen, et al., 2010). Although WM theory does not predict that dual-tasks will reduce the emotional intensity of imagery, it is assumed that EMs reduce the emotionality of imagery by making the image appear less vivid and therefore less like the imagined event is happening in the present moment. This is consistent with our observation of similar decreases in image vividness and emotionality from pre to post task within task conditions. However, our results suggest the effect of the EMs task on image emotionality was more robust than its effect on vividness. Again, this is in contrast to most of the EM literature, which tends to find more reliable effects of EMs on vividness. It is unclear why the effect of EMs in our study was only found for measures of emotionality. Crucially, the effects of EMs compared to the auditory task were in contrast with our predictions.

The results for emotionality suggest that EMs have no benefit over an auditory task that putatively loads executive resources just as much (although we were unable to demonstrate this because of an error in experiment 1a). This finding contradicts previous studies (Andrade et al., 1997; Lilley et al., 2009; Van den Hout et al., 2012) and is consistent with argument that it is the general or executive WM load of EMs, rather than their modality-specific WM load that interferes with imagery. Previous research compared auditory and EMs tasks that differed in ways other than their WM load. For example, Lilley et al. (2009) used an auditory task that required making a verbal response, whereas no verbal response was made in the EM condition. A strength of our study is the matching of the tasks for elements for the EM and auditory tasks, such as rate and type of decision, response mode etc. Another strength is that we assessed participant's perceptions of task difficulty and pleasantness. The lack of an effect of task condition on these measures offers reassurance that had we observed any differential effects of condition on vividness or emotionality, this could not be attributed to differences in subjective difficulty or how much participants enjoyed or disliked the task.

There are several possible reasons why our findings contradict previous evidence that EMs reduce image vividness and emotionality by taxing central executive and visuospatial WM resources. First, it could be argued that our failure to find a larger effect of EMs on image vividness and emotionality compared to the auditory task suggests the EM task may not have loaded visuospatial WM enough to interfere with the visual details of the distressing image. As mentioned earlier, due to the limitations of the verbal STM test in experiment 1b, our finding that EMs interfered more than other tasks on tests of visuospatial STM is not conclusive evidence that the EM task places greater demands on visuospatial WM, as the larger effect of EMs may have been caused by the general cognitive demands of the task alone. We address this limitation in experiment 2a, described in the next chapter, in which we tested the modality-specific WM demands of the concurrent tasks using an improved verbal STM test. As such, experiment 2b offers a stronger test of our assumption that the EM task places greater demands on visuospatial WM than the other tasks, and therefore should interfere to a greater extent with distressing visual mental imagery.

As the results for image vividness and emotionality were non-normal, the effect of task condition was analysed using non-parametric tests, which are generally less powerful than parametric tests. It may be tempting to conclude that our analysis lacked the statistical power to detect larger decreases in vividness and emotionality in the EM task condition compared to the auditory task. However, simulation research shows that non-parametric methods of testing interaction effects offer a robust and powerful alternative to parametric methods when the data violates the assumption of normality (Sawilowsky, 1990). The aligned rank transformation test used in experiment 1b, compared to traditional parametric tests, has the advantage that it allows the interaction between factors to be tested without inflating the risk of type-I error (Wobbrock et al., 2011). This is part of the reason the aligned transformation method is increasingly used in peer-reviewed research (Elkin, Kay, Higgins, & Wobbrock, 2021). Perhaps more convincingly, we observed a numerically larger decrease in image emotionality in the auditory task condition than the EM condition. As this difference was in the opposite direction to our prediction, it was not the case that our study lacked the statistical power to detect a larger effect of EMs compared to the auditory task.

All participants in experiment 1 performed the STM procedure before the negative image procedure. This meant that participants were familiar with the tasks at the point they were asked to perform the tasks during negative recall. One of the important instructions on the EM and auditory tasks was that participants had to respond when they detected a target letter among distractors. Having practiced these tasks by completing experiment 1a, participants may have learned that only two target letters were presented per task block. Knowing this, they may have stopped paying attention to the concurrent task after the second target letter had been detected, which would reduce the overall impact of the task on image vividness and emotionality. This possibility is consistent with our finding that subjective difficulty of the EM and auditory task was similar to the fixation task, which simply involved looking at a stationary cross – we predicted the fixation task would be easier. It is possible participants may have offset the WM demands of the EM and auditory tasks by switching their attention between the task and the image after the second target letter had been presented. In experiment 2, the order in which participants performed the STM and negative image procedures was counterbalanced to address the concern that performing the STM tests first would lessen any effect of tasks on recall.

Given our finding that the EM and auditory tasks caused a larger reduction in emotionality than the fixation task, it is possible the numerically larger effect of the EM and auditory tasks on image vividness compared to the fixation task may have been significant had we used a more powerful within-subjects design. One advantage of using a within-subjects design in EM studies is that it controls for individual differences in the interpretation of vividness and emotionality, which can reduce variation in scores between task conditions. Only two other EM studies have used a between-subjects design to test the effect of EMs on images of negative autobiographical memories (Smeets et al., 2012; van Veen et al., 2016). In contrast to the current experiment, both of these studies found an effect of EMs on image vividness and emotionality compared to a fixation task. However, there were potentially important differences that may explain why an effect of EMs in these studies was more likely to be detected. In the study by van Veen et al. participants held the image in mind while performing 16 blocks of EMs, equivalent to 384 s of interference between the EM and image. In contrast, the current experiment used only six blocks of EMs, equivalent to 144 s of interference between the EM and image. Studies have found that increasing the dose of EM interference by increasing the number of blocks of dual-tasking leads to larger reductions in image vividness (Leer et al., 2014). Presumably, adding more blocks of EMs increases the overall amount of interference caused by the WM load of EMs. Indeed, van Veen's found stronger evidence that EMs reduced image vividness more than fixation after 16 experiment blocks, compared to eight blocks - the added effect of EMs was larger the higher the dose of EMs. In the study by Smeets et al. participants rated the target image a total of 21 times across a four 24 s blocks of dual-tasking, whereas participants in the experiment 1b only rated the image at baseline and post-task.

It may be that repeated rating of the image increased the sensitivity of their procedure to the effects of EMs (see chapter 6 and 7 for further discussion). One way to test if our null-results were due to our choice of a between-subjects design was to replicate experiment 1b using a more powerful within-subjects design. This was the aim of experiment 3, which I will discuss in chapter 5.

In EM studies by van den Hout et al. (e.g. Van den Hout et al., 2001) participants usually hold the target image in mind for around 20 s after the dual-task procedure before rating its vividness and emotionality. This ensures the image rating procedures at baseline and post-task are the same, and gives participants enough time to bring the image to mind. Baseline ratings of image vividness and emotionality in experiment 1b were taken after 20 s of imaginal exposure. However, post-task ratings were taken immediately after the dual-task procedure, in contrast to the van den Hout procedure. A possible limitation of our method is that participants had less time to generate the details of the image after the task. As such, the observation that vividness and emotionality decreased over time in all task conditions may be an artefact due to participants having less time to generate a vivid image after the task. As we predicted that dual-tasking would cause a decrease in vividness and emotionality from baseline to post-task, it was important to ensure that this decrease was attributable to the effect of the dual-task and not the image rating procedure. We addressed this in experiment 2b: participants performed an additional image rating procedure at post-task which resembled the procedure used to measure imagery at baseline. The same procedure was used in experiments 3 and 4, described later, the results of which suggest that allowing participant's to generate the image eliminates the immediate effects of EMs. The possible effects of choosing different rating procedures is addressed in chapter 10.

4 Chapter 4: Comparing the visual and verbal WM load of auditory and eye movement tasks, and their interference with negative recollection after dual-tasking.

Experiment 2 replicated experiment 1 but with several important improvements. Firstly, we counterbalanced the order in which participants performed the STM tests (experiment 2a) and the negative imagery procedure (experiment 2a) to address the concern that performing the STM tests first would lessen any effect of tasks on recall. In experiment 1, all participants performed the negative imagery procedure after the STM tests. This allowed participants to practice the EM and auditory tasks numerous times before they performed the task while holding a negative image in mind. It is possible that participants learned that there were only two target letters per block of the dual-task procedure and therefore after detecting the second target, they could pay full attention to the target image. This would have stopped interference between the concurrent task and the image and might explain why the EM task did not affect image vividness in experiment 1b. Furthermore, if participants were not paying full attention to the EM and auditory tasks, this may explain why these tasks were given similar difficulty ratings to the fixation task, which we expected participants to find easier.

A second improvement on experiment 1 was that the stimuli presented on the verbal STM in experiment 2a were numbers, whereas the stimuli used in experiment 1a were letters. We decided not to use letters because the EM and auditory tasks also present letters in the form targets/fillers. Participants might recall letters presented during the dual-attention task because the aim of the STM test is to recall letters. Using numbers avoided this issue, and meant the dual-attention tasks should only interfere with recall performance by interfering with the phonological loop and/or the central executive.

Participants in experiment 2a were given a questionnaire which asked about the strategy they used to remember the stimuli presented on the visual and verbal STM tests. We included this questionnaire because of anecdotal evidence that participants had used a variety of strategies to perform the STM tests in experiment 1a. Multiple participants reported using a spatial/motor strategy, which involved drawing in the air with their finger to remember the shapes formed by the black squares in a matrix. Another participant numbered the cells within a matrix and rehearsed the numbers of cells that contained a black square. Various strategies were also reported for the verbal STM, including visual imagery. WM theory suggests that if a verbal strategy is used to perform the visual STM test, for example, the visuospatial interference caused by EMs would have a smaller impact on recall accuracy. Potentially, individual differences in recall strategy could eliminate the interference caused by the modality-specific WM demands of our concurrent tasks, which in turn would lead to inaccurate conclusions about how these tasks affect negative imagery. We therefore asked participants in the current experiment how they remembered the stimuli on each STM test, and then used this information to evaluate the likelihood that our primary analysis for experiment 2a was affected by individual differences in recall strategy.

Baseline ratings of image vividness and emotionality were removed in experiment 2b. Instead, participants were asked to rate their image immediately after the concurrent task. Further ratings were taken after a period of imaginal exposure, during which participants generated their image as vividly as possible, and then again after 24 hours. We decided to remove baseline ratings because participants might use this rating as an anchor point when asked to rate the image on subsequent occasions. This means we might be measuring the participant's ability to recall the rating they gave at baseline, rather than measuring the vividness and emotionality of the image after dual-tasking. A limitation of not recording imagery before the intervention is that we cannot test if there are baseline differences between groups in terms of image vividness and emotionality. However, given that participants were randomly assigned to task conditions in experiment 2b, we can reasonably assume that the images participants selected were similarly vivid and emotional before performing the dual-tasking procedure.

In most studies that have tested the WM hypothesis of EMDR, once the participant has held a negative image in mind while performing a dual-task, they are then given around 20 s to generate and focus on the image before rating its vividness and emotionality. In experiment 1b, participants were given 20 s at the start of the experiment to generate their mental image, but were asked to rate the image immediately after the concurrent task. The rationale for measuring imagery immediately after dual-tasking was to estimate the vividness and emotionality of the image during dual-task performance, which is when we assumed the effect of taxing WM would be largest. Our concern in experiment 1b was that the decrease in image vividness and emotionality from baseline may have been due to participants having less time after the dual-task than at baseline to generate a detailed image. Experiment 2b therefore introduced an additional image rating at post-task which followed 20 s of imaginal exposure. Using a similar rating procedure at baseline and post-task meant we could rule out the possibility that reductions in image ratings were due to our rating procedure. The additional advantage of including the additional post-task image rating in experiment 2b was that it allowed us to test if any effects of task condition on imagery were still present after participants were given longer to generate the image.

The inclusion of a 24 hour follow-up in experiment 2b meant that we could also test if effects of dual-tasking were present outside the reconsolidation window. In addition to follow-up ratings of image vividness and emotionality, we also asked participants to estimate how often the target image had intruded since the dual-task procedure. To our knowledge, experiment 2b is the first to investigate how dual-tasking during imagery affects the subsequent intrusiveness of the image. Previous studies using a trauma film paradigm have found that recalling a negative image and then performing a WM task –

without holding the image in mind – makes the image less intrusive (e.g. James et al., 2015), presumably because loading WM prevents the image from intruding during the intervention. The difference is that these studies do not ask participants to rehearse the image during while WM is being taxed. We are not aware of any study in the EMDR literature that has used frequency of intrusions to measure the effect of WM interference on imagery. It seems plausible that images which have less sensory information following dual-tasking would be less likely to match sensory information in the environment, and therefore be less likely to intrude. We predicted that EMs would lead to fewest image intrusions, followed by the auditory task and that the greatest number of intrusions would occur after participants performed the fixation task.

In addition to measuring the vividness and emotionality of imagery, participants in experiment 2b were asked, at the end of the experiment, how difficult was it to bring the image to mind if it dropped out of awareness during the task (0: not at all difficult: 10: extremely difficult). Baddeley and Andrade (2000) proposed that when participants are instructed to rate the vividness of a mental image, they rate the ease with which they generated and maintained the image during the concurrent task. That is, although we intended to measure the similarity between the contents of visuospatial WM and actual perception of the distressing episode, we were instead measuring the ease of extracting sensory information from long-term memory for construction of the image. This distinction is important because our prediction that EMs should affect imagery more than the auditory task is based on the assumption that participants are rating vividness by accessing the contents of visuospatial WM. According to WM theory (Baddeley, 1986), we may instead be measuring the impact of the concurrent tasks on the central executive, which is responsible for extracting the sensory details of the image from long-term memory. If so, we would expect vividness to be equally effected by the EM and auditory tasks, given their assumed equal executive WM load. The instruction to rate how difficult it was to generate the image, which we refer to hereafter as retrieval difficulty, was used to address the concern that a larger effect of EMs on vividness compared the auditory task may reflect interference with the executive-driven process of generating the image, rather than the storage of the image in visuospatial WM. If we were to observe that the EM task reduces image vividness more than the auditory task, and both tasks make it similarly difficult to retrieval the target image, we could be more confident that the larger effect of EMs on imagery reflects greater visuospatial WM interference. However, given the same effect of EMs on imagery, if we were to find that retrieval of the image was harder in the EM task than the auditory task, we should be less confident that the larger effect of the EM task on imagery is due entirely to the visuospatial WM load of the task.

4.1 Experiment 2a: measuring the WM load of the interference and control tasks

We predicted that recall accuracy on the visual and verbal STM tests would be poorer when participants performed the concurrent EM and auditory tasks than when they performed the fixation task. Crucially, we predicted there would be an interaction between the sensory modality of the STM test and the type of concurrent task. Specifically, we predicted that EMs would lead to poorest recall accuracy on the visual STM test, while the auditory task would lead to poorest recall accuracy on the verbal STM test.

4.1.1 Participants

We recruited 144 psychology undergraduates (119 female; modal age was between 18-21 years) from Plymouth University. The same participants took part in experiment 2a and 2b. The sample size was based on the need to test participants in multiples of 72 to fully counterbalance the order of experiment 2a and 2b, as well as the order of the

STM tests and concurrent tasks and in experiment 2a. Participants took part in exchange for course credits. Most participants were native English speakers (87%).

4.1.2 Materials, Design and Procedure

The method used in experiment 2a differed from experiment 1a in three ways, which are described below.

Whereas participants in experiment 1a were tested in small groups, participants in experiment 2a were tested individually in sound attenuated cubicles, in order to minimise distraction.

To address the limitation of using letters in the verbal STM in experiment 1a, stimuli for the verbal STM test in experiment 2a were 24 strings of numbers. Half of the number strings in each task condition contained six numbers and the other half contained seven numbers. Number strings were generated by pseudo-randomly by selecting a number between one and nine, avoiding repeats and the number seven – as seven takes longer to rehearse subvocally, its omission meant that all number strings were equally difficult to maintain in verbal WM. Numbers were recorded as sound files and were played through headphones, at a rate of one number per second. Participants responded by typing the numbers into a blank textbox presented on the computer screen. Participants could type up to a maximum of six or seven letters, depending on the length of the sequence presented. Participants were awarded one point for each number recalled in the correct serial position, and points were not deducted for incorrect responses.

After completing experiments 2a and 2b, we asked participants about the strategy/s they used when rehearsing stimuli for the visual and verbal STM tests. We provided participants with written statements that described a strategy they might have used for each type of STM test. We then asked if they used this strategy, a different strategy, or both. If the participant had used a different strategy, they were asked to describe this briefly in writing. The statement regarding the visual STM test was “I formed an image/picture of the grid and the squares in my mind”, and for the verbal STM test it was “I repeated the numbers in my head (in my own voice/voice of the speaker)”. These were the strategies we assumed participants would use to retain the test stimuli before recall.

The materials, design and procedure of experiment 2a was otherwise identical to experiment 1a. In short, participants performed six blocks of each STM per task condition and completed all three task conditions in a counterbalanced order. Participants performed the concurrent task while trying to retain a briefly presented matrix or number string, which they then had to recall as accurately as possible.

4.1.3 Results

To address the issue that participants performed close to ceiling on the visual STM in experiment 1a, we used a stricter criterion for measuring accuracy in experiment 2a. Whereas accuracy in experiment 1a was measured according to the number of individual squares/numbers recalled, participants in experiment 2a had to recall an entire matrix pattern / number string without any errors for their response to be counted as accurate. This meant it was harder to achieve a high accuracy score on both STM tests in experiment 2a. It was necessary to measure accuracy in the same way for both the visual and verbal STM tests in order to test our prediction about the interaction between dual-task modality and the modality of the STM.

Recall strategy on STM test

Participants were asked which strategy they used to perform the visual and verbal STM tests. Table 3 shows the percentage of participants according to the strategy they used on the visual and verbal STM tests. As expected, most participants used mental imagery and subvocal rehearsal to retain stimuli presented on the visual and verbal STM tests, respectively. A small percentage of participants had used strategies other than those mentioned above. According to the description participants' gave of these alternative methods, it appeared that several participants had encoded the matrices verbally, by numbering the cells within a matrix, as well as spatially, by tapping the position of the black squares on the table or drawing them in the air. Most of the participants who used alternative strategies on the verbal STM test either repeated the numbers aloud – despite prior instruction – or tapped the numbers on the keyboard provided as a form of spatial cue.

Table 3: Percentage of sample who used each type of recall strategy on the STM tests.

Visual STM Test		Verbal STM Test	
Mental imagery	86.1	Subvocal rehearsal	89.6
Other strategy	6.3	Other strategy	6.9
Mental imagery and other strategy	6.9	Subvocal rehearsal and other strategy	2.9

Visual and verbal STM test performance

Figure 7 shows the mean recall accuracy for the visual and verbal STM test under each dual-task condition. Recall accuracy was defined as the percentage of correctly recalled matrices and number strings on the visual and verbal STM tests, respectively. Histograms and Q-Q plots showed that verbal STM accuracy was positively skewed in the EM and auditory task conditions. Scores on the visual STM test were positively skewed in the EM condition, and negatively skewed in the auditory task condition. Since the distribution of data across task conditions was multimodal, performing any one transformation did not correct for skewness in all task conditions. We therefore analysed the data using non-parametric tests.

We converted accuracy scores on the visual and verbal STM tests using aligned rank transformation. The transformed scores were analysed using a 2 (Test: visual STM; verbal STM) x 3 (Task: Fixation; EMs; Auditory) repeated-measures ANOVA. This revealed a significant main effect of STM test modality, $F(1, 142) = 174.088$, $p < .001$, which was due to accuracy scores being lower on average in the verbal STM test compared to the visual STM test. There was also a significant main effect of task modality, $F(2, 141) = 32.99$, $p < .001$, and most importantly, a significant interaction between the STM test and dual-task modality, $F(2, 141) = 74.37$, $p < .001$.

To investigate this interaction, separate Friedman's tests were used to compare dual-task conditions in terms of accuracy on the visual STM and verbal STM test. Regarding the visual STM test, there was a statistically significant difference in recall accuracy depending on the modality of the concurrent task, $\chi^2(2) = 113.42$, $p < .001$. Boxplots showed the distribution of differences between task conditions was symmetrical for the visual STM test. Therefore, post-hoc comparisons between task conditions were performed using Wilcoxon signed-rank tests with Bonferroni correction, meaning alpha was .017. In line with our predictions, accuracy was significantly lower in the EM condition compared to the fixation task condition, $Z = -7.256$, $p < .001$ and the auditory task conditions, $Z = -9.105$, $p < .001$. Surprisingly, accuracy was significantly lower in the fixation task condition than in the auditory task condition, $Z = -3.21$, $p = .001$. A separate Friedman's test showed accuracy on the verbal STM test also differed significantly between task conditions, $\chi^2(2) = 60.915$, $p < .001$. As boxplots showed the distribution of differences between the EM and fixation task were asymmetrical, post-hoc comparisons were performed using Sign tests. In line with our predictions, accuracy on the verbal STM test was significantly lower in the auditory task condition compared to the fixation task condition, $Z = -7.471$, $p < .001$, and the EM task condition, $Z = -4.060$, $p < .001$. Furthermore, verbal STM accuracy was significantly lower in the EM condition compared to the fixation task condition, $Z = -2.696$, $p = .007$.

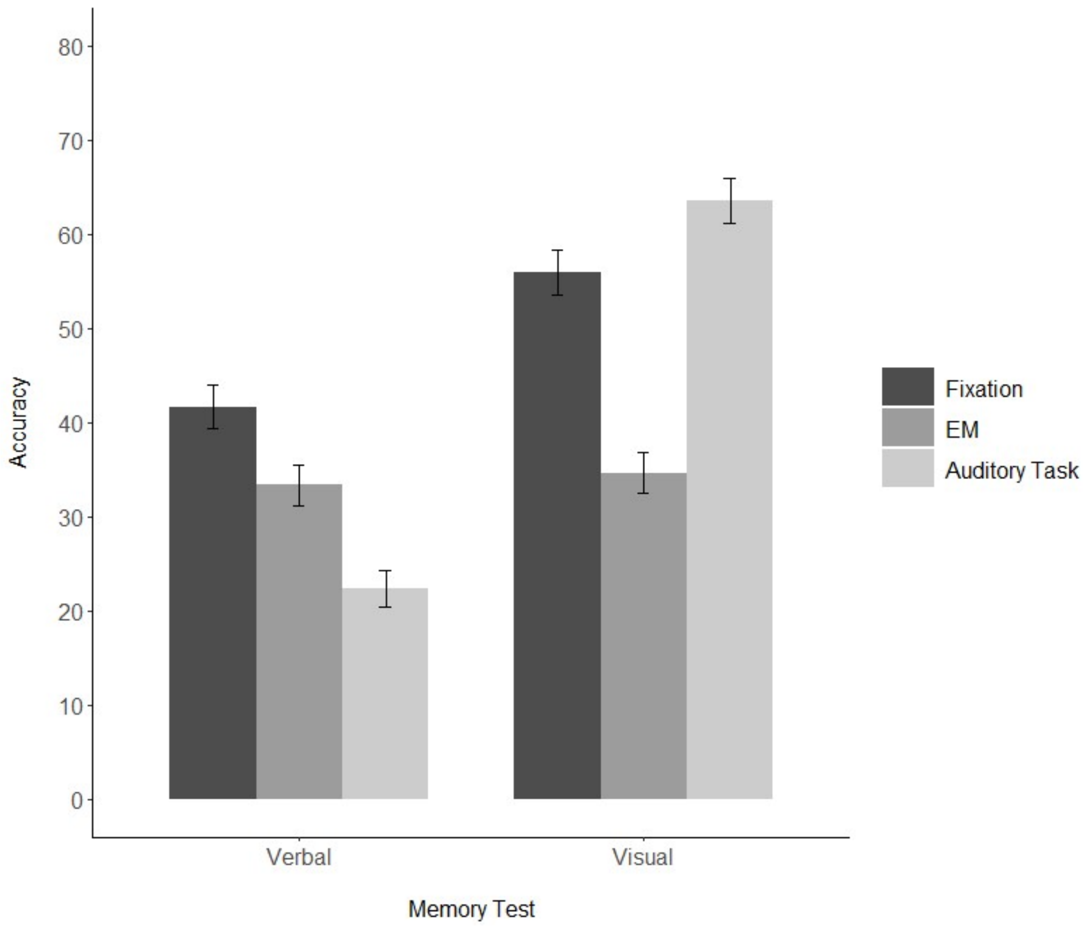

Figure 7: mean recall accuracy on the visual and verbal STM tests under each dual-task condition in experiment 2a. Error bars represent standard error.

4.1.4 Discussion of experiment 2a

The results partially supported our prediction about the cross-modal effects of the EM and auditory tasks. As expected, EMs impaired the recall of number strings more than the fixation task, presumably because the former places greater demands on the central executive. However, the auditory task did not cause poorer recall accuracy for visual stimuli than the fixation task, although numerically the results were in the predicted direction. This was unexpected given that the auditory task was similar in structure to the EM task, and therefore should have impaired the executive processes involved in the visual STM more than the fixation task. One possible explanation is that the stationary cross the participants looked at in the fixation task caused visual interference, which made it more difficult for participants to retain the matrices in visuospatial WM. However, participants were also asked to focus on a fixation cross in the auditory task, to minimise the visuospatial interference caused by natural saccades. The only difference between these tasks were the additional procedural components of the auditory task (see chapter 2). Another explanation is that the auditory task does not load the central executive more than the fixation task, and therefore the larger effect of the auditory task compared to the fixation task on the visual STM test was due to error. Again, we believe this is unlikely given that additional procedural elements of the auditory task, namely monitoring letters and the button press response, should have generated an attentional load that would have interfered with the executive processes involved in the visual STM test. However, as we did not measure the executive demands of our concurrent tasks in the current experiment, we cannot rule out this explanation.

Crucially, we found clear evidence of modality-specific interference between the EM and auditory tasks and the visual and verbal STM tests, respectively. Making EMs impaired recall on the visual STM test more than other tasks, while the auditory task led to poorest recall accuracy on the verbal STM test. That the effect of these tasks depended on the modality of the stimulus being recalled means our results cannot be explained by central executive interference alone. Since the majority of participants reported using visual imagery to retain stimuli on the visual STM test, the current findings suggest that the EM task should interfere with negative visual imagery more than the auditory and fixation tasks, assuming our test of visual STM relies on the same visuospatial WM resources that support vivid imagery. Experiment 1b was used to test the prediction that the larger visuospatial WM load of the EM task would result in a larger decrease in image vividness and emotionality compared to the other tasks.

4.2 Experiment 2b: comparing the effect of the interference tasks on distressing imagery

The main purpose of this experiment was to test the reliability of the results from Experiment 1b. In addition, we assessed vividness, emotionality and negative affect after a longer follow up delay, to determine if the concurrent tasks would have a lasting effect on negative visual mental imagery. In line with experiment 1b, we predicted that image vividness and emotionality would be lower immediately after the EMs task compared to the fixation and auditory tasks. Extending on the previous experiment, we expected this larger effect of EMs would be present after 20 s of exposure to the image at post-task, and at 24 hr follow-up.

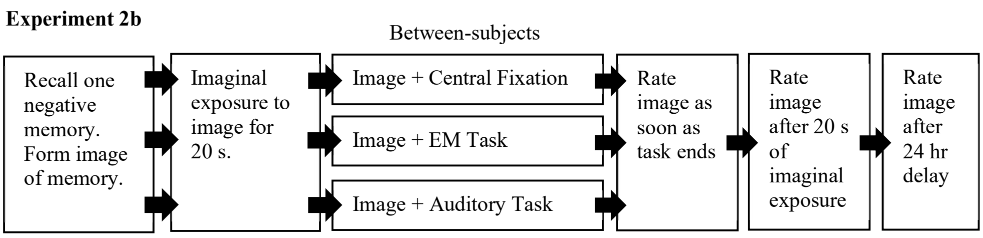

Figure 8: a representation of the general procedure of experiment 2b.

4.2.1 Participants and Procedure

All of the participants from experiment 2b performed experiment 2b after a 2 min break. Participants were randomly allocated to the fixation, EM or auditory task condition.

The materials and procedure were similar to those in Experiment 1b, except participants rated their images at different times. Ratings of vividness, emotionality, and negative affect were recorded immediately after the dual-task and then again after a 1 min and 24 hour delay.

Imagery ratings taken at 1 min and 24 hr post-task were preceded by a 20 s period of imaginal exposure, during which participants held the image in mind while facing a blank computer screen. Participants rated their image at 24 hr follow-up using a short online survey. A link to the survey was emailed to participants who completed it outside of the original testing environment. The survey encouraged participants to find a quiet place on their own. The recall instructions, computer display during recall, and rating scales were the same as those used during the laboratory procedure.

4.2.2 Results

Sixteen participants did not respond to the questionnaire sent at 24 hr follow-up. The remaining sample size at 24 hr follow-up was 40 for the fixation condition, 44 for the EM condition, and 44 for the auditory task condition. The sample size estimate for experiment 2b was based on the effect of EMs on image vividness shortly after dual-tasking, relative to recall alone. That is, power calculations were not performed for the effect of EMs at 24 hr follow-up. It is unclear, therefore, whether our interpretation regarding the effect of EMs at 24 hr follow-up would have been different if all participants had responded to the questionnaire.

Vividness

Figure 9 shows the mean vividness score immediately after the dual-task, after further imaginal exposure to the image, and at 24 hr follow-up in each task condition. Histograms and Q-Q plots indicated that the distribution of vividness scores taken immediately after dual-tasking were approximately normal in all conditions. A one-way ANOVA revealed that image vividness did not differ significantly between task conditions immediately after dual-tasking, $F(2, 141) = 2.977$, $p = .054$, $\eta p^2 = .041$. While this result was marginal and the numerically the effect of EMs was larger than other tasks, we felt that performing planned comparisons would unnecessarily increase the risk of type-1 error given the effect of the auditory task compared to the fixation task was in the opposite direction to our prediction. Even if the effect of EMs compared to the fixation task was statistically significant, it would be misleading to suggest this effect supports a WM hypothesis when the presumably larger WM load of the auditory task did not reduce image vividness more than the fixation task.

The distribution of vividness ratings taken after 20 s of imaginal exposure at the end of the dual-task procedure was negatively skewed in the auditory task condition. Scores continued to show significant skew after adjustment using square transformation, therefore the difference between task conditions at this time point were analysed using non-parametric tests. A Kruskal-Wallis H test showed vividness scores taken after 20 s of imaginal exposure differed significantly between task conditions, H (2) = 22.815, p < .001, $\varepsilon^2 = .16$. To investigate this interaction, planned comparisons were made using Mann-Whitney U tests between the EMs and fixation tasks, to test the effect of taxing the central executive, and between the EM and auditory tasks to test the effect of taxing visuospatial WM. Bonferroni correction was used, meaning alpha was set to .025 for each comparison. In contrast to our prediction, vividness did not differ significantly between the EM and auditory task, $U = 1142.5$, $p = .944$. Furthermore, vividness was significantly lower in the fixation condition than in EM condition, $U = 608.5$, $p < .001$, which was in the opposite direction to our prediction. Therefore, it was not the case that the effect of EMs versus fixation was too small to detect using non-parametric analysis.

As vividness scores taken at 24 hr follow-up were normally distributed, the effect of task condition was analysed using a oneway ANOVA, which did not reveal a significant difference between conditions, $F(2, 125) = 1.328$, $p = .269$, $\eta p^2 = .021$. That is, the observed differences between task conditions during the experiment were no longer present after 24 hr.

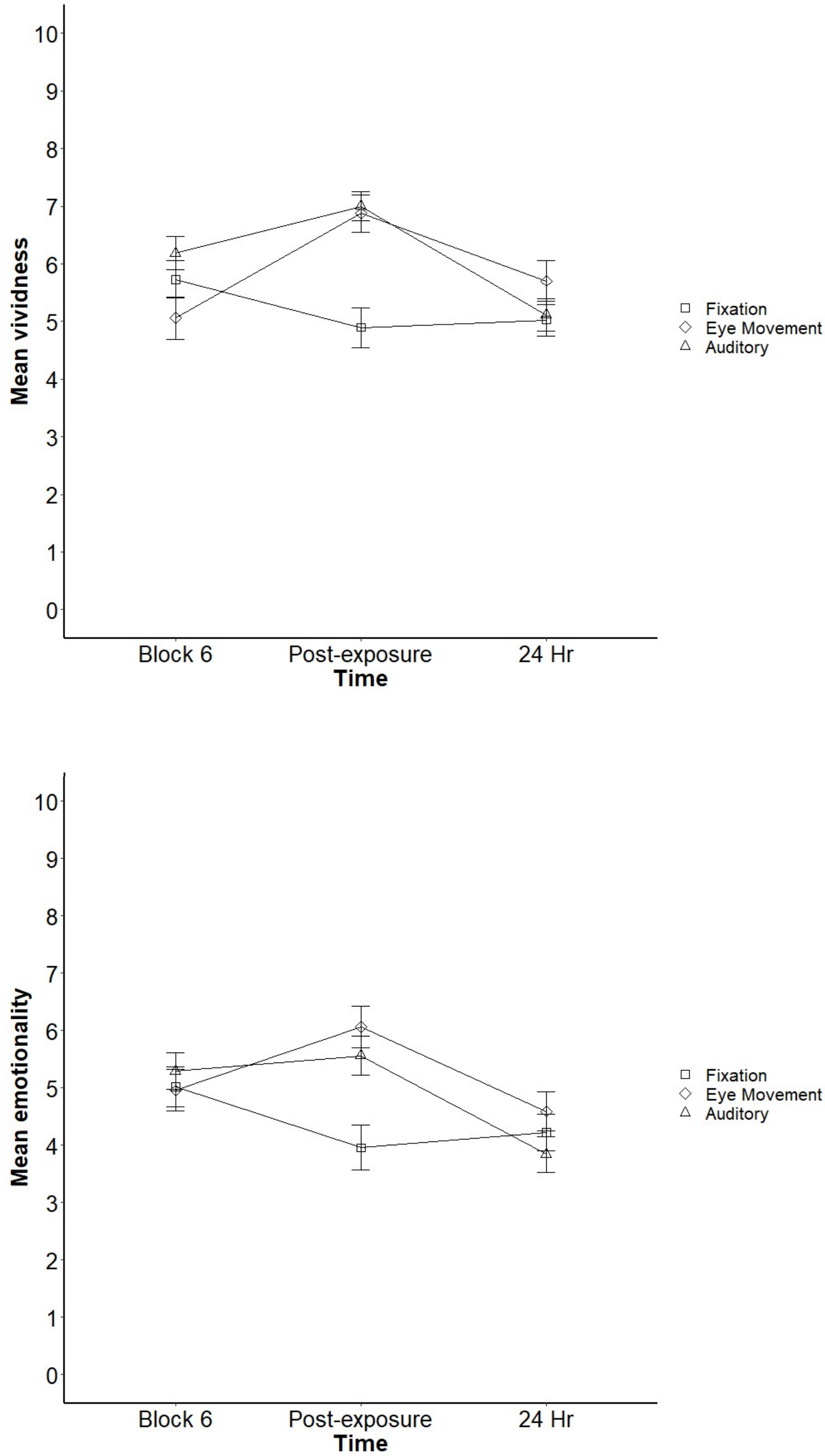

Figure 9: mean image vividness and emotionality immediately after the dual-tasking (block 6), after 20 s of imaginal exposure post-task (post-exposure), and at 24 hr follow-up (24 hr) in experiment 2b. Error bars represent standard error.

Emotionality

Figure 9 shows the condition mean for image emotionality at each point the image was rated. Histograms and Q-Q plots indicated that the distribution of emotionality ratings taken immediately after dual-tasking and at 24 hr follow-up were approximately normal. As for image emotionality following imaginal exposure post-task, the distribution of scores in the EM and auditory task conditions was negatively skewed, while scores in the fixation condition showed a bimodal distribution that was more positively skewed. As skewness varied, no single transformation would produce normally distributed data, therefore scores at this time point in the experiment were analysed using non-parametric tests.

A one-way on emotionality ratings taken immediately after the dual-task procedure failed to find a significant difference between task conditions, $F(2, 141) = 0.261$, $p = .770$, $\eta p^2 = .004$. For ratings taken after 20 s of imaginal exposure after dual-tasking, a Kruskal-Wallis H test revealed a significant effect of task condition, $H(2) = 16.388$, $p < .001$, $\varepsilon^2 = .115$. To investigate this interaction, planned comparisons were made using Mann-Whitney U tests between the EMs and fixation tasks, and between the EM and auditory tasks using a Bonferroni adjusted alpha of .025. These tests showed image emotionality was significantly lower in the fixation task condition than in the EM task condition, $U = 647$, $p < .001$. Image emotionality after imaginal exposure did not differ in the EM and auditory task conditions, $U = 992$, $p = .236$. There were no significant difference between task conditions when image emotionality was measured 24 hr after the dual-task procedure, $F(2, 125) = 1.217$, $p = .300$, $\eta p^2 = .019$.

Negative affect

Figure 10 shows the mean score for negative affect for each task condition at each point the image was rated. Histograms and QQ plots showed the distribution of negative affect scores at post-task, after imaginal exposure, and at 24 hr follow-up were positive skewed in more than one of the task conditions. The distributions of scores taken immediately after the dual-task procedure and at 24 hr follow-up were approximately normal after applying a square root transformation, therefore these adjusted scores were analysed using parametric tests. Scores taken after imaginal exposure at post-task were adjusted using square root, logarithmic and reciprocal transformations, however none of these methods produced normally distributed data. Therefore, negative affect scores at this time point were analysed using non-parametric tests.

A oneway ANOVA failed to find significant differences between task conditions in terms of negative affect ratings immediately after the dual-task procedure, $F(2, 141) = 1.691$, $p = .188$, $\eta p^2 = .023$, or ratings at 24 hr follow-up, $F(2, 125) = 0.095$, $p = .91$, $\eta p^2 = .002$. A Kruskal-Wallis H test found significant differences between task conditions when negative affect was rated after 20 s of imaginal exposure post-task, $H(2) = 13.372$, $p = .001$, $\varepsilon^2 = .094$. Dunn's tests using Bonferroni correction showed mean ranked negative affect scores were significantly lower in the fixation task condition compared to the EM condition, $p = .018$, and the auditory task condition, $p = .002$. Scores in the EM and auditory task conditions did not differ significantly, $p = 1$.

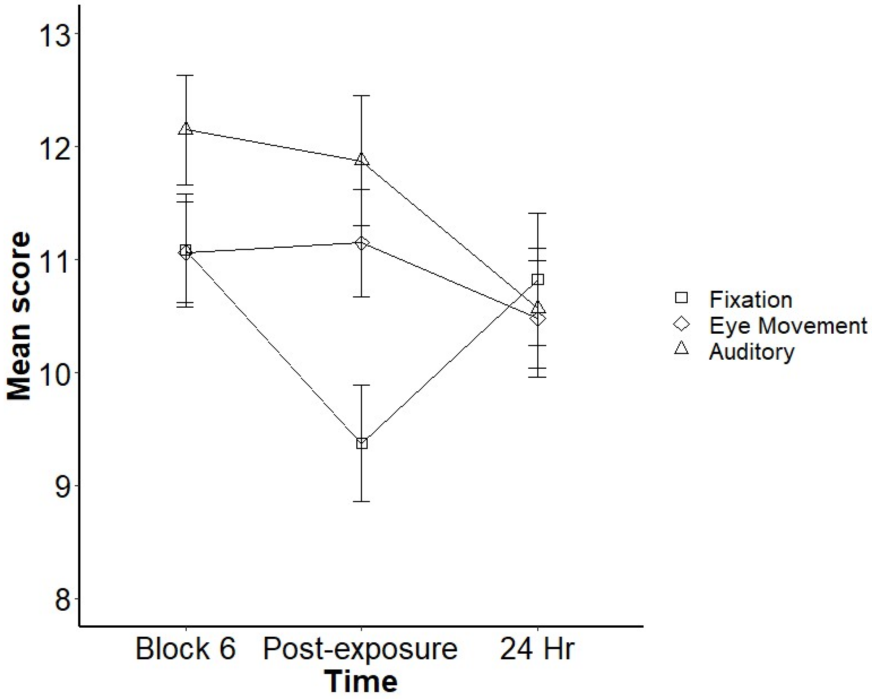

Figure 10: mean negative affect scores immediately after the dual-tasking (block 6), after 20 s of imaginal exposure post-task (post-exposure), and at 24 hr follow-up (24 hr) in experiment 2b. Error bars represent standard error.

Task difficulty, task pleasantness, retrieval difficulty and image intrusiveness

Table 4 summarises the mean scores in each task condition for our measures of task difficulty, pleasantness, the difficulty of retrieving the image during the dual-task, and the number of times the image intruded into awareness in the 24 hours following the dual-task procedure.

The distribution of task difficulty ratings was positively skewed in the fixation task condition according to histograms and Q-Q plots. Square root and logarithmic transformation of these scores failed to produce normally distributed data, therefore difficulty scores were analysed using non-parametric tests. A Kruskal-Wallis H test found task difficulty varied significantly between task conditions, H (2) = 11.395, p = .003, ε^2 = .08. Post-hoc Dunn's tests using Bonferroni correction revealed the fixation task was significantly less difficult than the EM task, p = .007, and the auditory task, p

= .016. In contrast, the EM and auditory tasks did not differ significantly in terms of subjective difficulty, $p = .1$.

The distribution of pleasantness scores was approximately normal in all task conditions. A oneway ANOVA indicated that task conditions did not differ significantly in terms of subjective pleasantness, $F(2, 141) = 1.019$, $p = .363$, $\eta p^2 = .014$.

As mentioned earlier, participants rated how difficult it was to retrieve the target image if they stopped focussing on it during the dual-task procedure. The distribution of ratings was positively skewed in the fixation condition and multimodal in the auditory task condition, therefore analysis was performed using non-parametric tests. A Kruskal-Wallis H test found the ability to retrieve the target image differed significantly between task conditions , H (2) = 12.71, $p = .002$, $\varepsilon^2 = .089$. Post-hoc Dunn's tests using Bonferroni correction indicated that participants found it easier to retrieve the target image during the fixation task compared to the EM task, $p = .002$, and the auditory task condition, $p = .031$. Ratings of image retrieval difficulty did not differ significantly between the EM and auditory task conditions, $p = .1$.

When participants were contacted 24 hr after the experiment, they were asked to state approximately how many times the target image had intruded into their awareness since performing the main dual-task procedure. Histograms and QQ plots showed the distribution of scores was positively skewed in all conditions. This was because most participants reported no/few intrusions of the image. Scores were transformed using square root and logarithmic transformations, but the resulting data continued to violate the assumption of normality. A Kruskal-Wallis H failed to find a significant difference between task conditions in terms of the mean ranked number of image intrusions in the 24 hr following the dual-task procedure, H (2) = 0.356, $p = .837$, $\varepsilon^2 = .003$.

Table 4: Condition means (and standard deviations) for ratings of task difficulty, task pleasantness, image retrieval difficulty during the dual-task, and number of image intrusions in the 24 hr after the dual-task procedure.

	Fixation	EM	Auditory
	M (SD)	M (SD)	M (SD)
Task difficulty	2.854 (2.666)	4.438 (2.396)	4.271 (2.648)
Task pleasantness	5.229 (2.345)	4.792 (2.798)	4.479 (2.593)
Retrieval difficulty	2.917 (2.508)	4.875 (2.765)	4.417 (3.002)
No. intrusions	1.45 (1.518)	1.568 (1.648)	1.386 (1.617)

4.2.3 Discussion of experiment 2b

The results of experiment 2b support the finding in experiment 1b that EMs did not affect the vividness and emotionality of distressing visual images more than the auditory task. This suggests the visuospatial WM load of the EM task had a negligible effect on images that were presumably stored in visuospatial WM during the dual-task procedure. Our results contradict previous studies that have found a larger effect of EMs on negative imagery compared to an auditory control task, which involved counting aloud from one (Andrade et al., 1997; Kemps & Tiggemann, 2007; Lilley et al., 2009). Unlike these studies, we did not find an effect of taxing the central executive on imagery. We found that the immediate effect of the EM and auditory tasks on image vividness and emotionality was not larger than the effect of the fixation task. This is surprising given that the EM and auditory tasks included additional components that were not present in the fixation task, namely the requirement to monitor and response to target letters. Presumably these additional elements mean the EM and auditory tasks create a larger general cognitive load than the fixation task. Our findings contradict previous studies which have found that taxing WM while imagining a negative autobiographical event reduces the vividness and emotionality of the image more than focusing on a stationary stimulus (Gunter & Bodner, 2008; Lilley et al., 2009; Smeets et al., 2012). Unlike experiment 1b, our failure to replicate the effect of taxing WM on imagery cannot be explained by practice effects. Again, we suggested that because all participants in experiment 1b practiced on the EM and auditory tasks during the STM procedure, they may have stopped paying attention to these tasks during the subsequent negative recall procedure, which would have reduced interference between the task and imagery. This explanation cannot account for the results of the current experiment, as the order of the STM and negative recall procedures was counterbalanced.

The results of experiment 2b also suggest that our decision to remove the baseline rating of imagery, which was used in experiment 1b, did not change the overall pattern of results. We reasoned that removing the baseline rating would reduce the possible effect of forgetting the initial rating on ratings taken at post-task. In the previous chapter, I argued that participants may provide ratings at post-task by trying to recall the rating given at baseline. Given our assumption that our experimental tasks are more cognitively demanding than the fixation task, we argued that the EM and auditory tasks in experiment 1b may have increased the likelihood that participants would forget the initial rating, leading to greater variation in scores in these task conditions and thus less power to detect the effect of task condition on imagery. By removing baseline ratings, participants in experiment 2b should have been more reliable at rating the effect of the task on their image because they would have been unable to reflect on previous ratings. The results of the current study suggest the inclusion of baseline ratings in experiment 1b was not in fact a problem: we replicated the finding in experiment 1b that EMs did not reduce image vividness more than the EM and auditory task. As we did not find any evidence that the effects of task condition were influenced by the inclusion of baseline measures, we decided to include baseline ratings in experiments 3-5 (chapters 5-7) to give a clearer sense about the change in image vividness and emotionality over time. If we had found that image vividness was lower after EMs than fixation, baseline ratings would show if this effect reflects a larger reduction in vividness in the EMs condition, or a larger increase in vividness in the fixation task condition. Measuring the effect of task conditions over time can therefore provide an insight into the underlying mechanism. In summary, our failure to find a reliable effect of EMs on negative imagery in experiments 1b and 2b, relative to the auditory and fixation tasks, suggests our results are not due to variation in post-task ratings as a result of forgetting previous ratings, or an effect of practicing the dual-tasks prior to the negative recall procedure.

We also failed to find a larger effect of EMs on image vividness and emotionality compared to the other task conditions when images were rated following 20 s of exposure to the image after dual-tasking, or when participants were emailed instructions to rate the image 24 hr after the experiment. Our results contradict the results of studies that have found an effect of EMs on negative imagery after a similar period of post-task image exposure (Leer et al., 2014; Van den Hout et al., 2001). Surprisingly, we found that image vividness and emotionality at this time point were lowest in the fixation task condition. This difference was caused by vividness and emotionality increasing over time in the EM and auditory task conditions, whereas ratings in the fixation condition continued to decrease from ratings taken immediately after the dual-task. A possible interpretation of these results is that participants were making comparisons to the image retrieved immediately after the dual-task procedure. Whereas the ratings immediately post-task can be considered a purer measure of task effects – participants did not have previous ratings on which to anchor their ratings – our subsequent instruction to rate the distressing image following imaginal exposure may have allowed participants to compare the current image to the image they had recalled moments earlier. This raises the question of why making comparisons to the previous image affected ratings in the

EM and auditory task conditions but not the fixation condition. A tentative explanation is that the EM and auditory tasks made images less vivid and emotional, but this difference was too small to detect given our use of a between-subjects design. Since ratings of vividness and emotionality are subjective, the interpretation of what a one point change in these ratings means may vary between individuals. This is a general limitation of our study and others that attempt to compare vividness and emotionality scores between-subjects. Having said this, we assume that it is possible to detect an effect of dual-tasking between-subjects, just that the difference between the ratings of different individuals may have to be larger to confidently say they represent meaningful differences in subjective experience. Assuming images were less vivid and emotional during the EM and auditory tasks than the fixation task, our results make sense if participants are rating imagery by making a comparison to the previous image. Assuming our experimental tasks interfere with imagery more than the fixation task, participants in the EM and auditory task conditions would have noticed a bigger difference between the imagine following imaginal-exposure and the degraded image recalled immediately after dual-tasking. As the fixation task should not be significantly more taxing than imaginal-exposure, the participants in this condition may have noticed relatively little difference between the image immediately after the dual-task procedure and after imaginal-exposure. We are not aware of any previous studies that have measured imagery both immediately after dual-tasking and after an additional period of imaginal exposure. This is arguably an advantage of the method used in the current experiment. This procedure was also used in experiments 3-5 (chapters 5-7, respectively) to demonstrate that any effect of EMs on imagery during dual-tasking may be erased by subsequent imaginal exposure.

There were no significant differences between task conditions when participants were contacted 24 hr after the dual-task procedure. That is to say the difference between the experimental and fixation conditions after the post-task imaginal exposure procedure was no longer present at follow-up. Continuing the argument above, it may have been difficult for participants to compare the image at follow-up to the previous image because of the delay, which may have encouraged them to rate their experience of the image at the time, as was the case immediately after the dual-task procedure. It is perhaps unsurprising that we failed to find an effect of EMs compared to the auditory task at 24 hr follow-up, as there was no evidence that these tasks affected imagery differently during the original experiment. In this sense, the results of experiment 2b do not offer any additional information about the long-term effects of dual-tasking on imagery, as this requires the image to be reconsolidated with the added effects of WM interference, which we were unable to find in this experiment or experiment 1b. Previous studies using a similar procedure to measure images at follow-up have found mixed evidence for the long-term effect of EMs: some have found a lasting effect of EMs on image vividness and emotionality after 24 hr (e.g. Schubert et al., 2011). NB the original procedure involved the full EMDR protocol), whereas others have failed to replicate the effects of EMs compared to a no-EM task when ratings were taken over the telephone 1 week later (Kavanagh et al., 2001; Lilley et al., 2009). Again, unlike these studies, we did not observe an added effect of EMs compared to our control tasks after dual-tasking. We used the same image rating procedure in experiment 3, which is described in the next chapter, as a way to test our assumption that an effect of EMs at follow-up would be observed using a more powerful within-subjects design.

In addition to testing the effect of EMs on negative imagery, experiment 2a compared the modality-specific WM loads of the concurrent tasks using a method that addressed the programming error on the verbal STM test in experiment 1a. In line with our predictions, we found clear evidence that EMs impaired performance of tests of visual STM more than the auditory task. Crucially, this result cannot be attributed to the general cognitive demands of the EM task, as the auditory task impaired performance on tests of verbal STM more than the EM task. To our knowledge, this is the first time the EM and auditory tasks used to interfere with negative imagery have been assessed to confirm they interfere with modality-specific WM stores. The advantage of knowing the extent to which tasks impair the different components of WM is that the relative effects of the tasks on image vividness and emotionality can be attributed more confidently to the modality-specific WM demands of the task. The results of experiment 1a confirm our assumption that the EM and auditory tasks selectively load on visuospatial and verbal WM, respectively, and therefore the EM task should have interfered to a greater extent with distressing images than our auditory task. An unexpected finding in experiment 2a was that compared to the fixation task, EMs impaired performance more on the verbal STM test, whereas the auditory task did not impair performance more than the fixation task on the visual STM test. This suggests that despite our efforts to match the EM and auditory tasks in terms of their general cognitive load, only the EM task interfered with the central executive processes that are required to accurately perform the STM tests. Accuracy on the verbal STM test, across task conditions, was lower than the visual memory test. Put differently, participants generally found it harder to recall the number strings on the verbal STM test than matrices on the visual STM test. Accurate performance on the verbal STM test required the participants to rehearse the sequence in which the numbers were presented. In contrast, the visual STM test did not require participants to rehearse information about the sequence of black squares, as the squares in each matrix were presented simultaneously. It is possible there was a greater need for participants to actively rehearse number strings than matrices, which may have placed greater demands on executive attention. In order to estimate the general cognitive load of the interference tasks, it requires that both tasks are compared in terms of their impact on similarly demanding tasks, or ideally the same task. Given our suspicion that the verbal STM test placed greater demands on the central executive, the results of experiment 2a cannot be used to draw conclusions about the extent to which our EM and auditory tasks load on central executive WM resources.

The results of experiment 2a offer more convincing evidence that our failure to find a larger effect of EMs on negative imagery in experiment 2b compared to other tasks was not because the EM task failed to load heavily enough on visuospatial WM. Crucially, our analysis of the effect of task condition on verbal STM test performance confirmed that the larger effect of EMs on visual STM performance in experiments 1a and 2a was not the result of general cognitive load alone. If the effect of EMs on visual STM performance was due to general cognitive load alone, then EMs should have also impaired performance on verbal STM tests more than other tasks. As the auditory task led to poorest performance on tests of verbal STM, we can conclude that the EM task impaired recall on the visual STM test because it loaded selectively on visuospatial WM resources. Furthermore, the majority of participants indicated that they performed the visual STM test using mental imagery. Therefore, we can assume that part of the reason EMs impaired recall on this test was due to interference with the rehearsal of the image of the matrix pattern in visuospatial WM.

4.3 Summary of experiment 2

It seems reasonable to assume that the EM task would have interfered with the visual details of the distressing image in visuospatial WM more than other tasks. The results of experiments 1 and 2 suggest that visuospatial WM interference does not selectively reduce vividness of emotional recollections. Rather, recollection vividness may be sensitive to overall cognitive load rather than modality-specific loads. Because we carefully matched the generic elements of the auditory and visual tasks, this experiment provides a stronger test of the impact of modality-specific processing than previous studies, where auditory and visuospatial tasks differed markedly in structure and response mode (Andrade et al., 1997; Lilley et al., 2009). We tentatively conclude that the effects of EMs on emotional memory recall observed in these previous studies are due to the general cognitive load imposed rather than the specific visuospatial elements of the task. This conclusion is more in line with the central account of EMDR proposed by Gunter and Bodner (2008) than the visuospatial WM hypothesis suggested by Andrade et al. (1997). However, readers will be unconvinced that experiments 1 and 2 offer support for a general resource account given our failure to observe a difference between the effects of the EM and fixation tasks on negative imagery. In experiment 3, we tested the possibility that our failure to detect the crucial effect of dual-tasking was due to our use of a between-subjects design, whereas most laboratory evidence for an effect of EMs comes from studies that compare EMs and control tasks within-subjects.

Before introducing experiment 3 in the next chapter, I will first address a question readers may have about whether our experimental tasks were overly taxing, given that in chapter 1 I highlight the effect of WM interference may follow an inverted-U relationship. Previous research suggests a concurrent task will have little impact on image vividness if the WM load of the task is too high (Engelhard et al., 2011). This is because if a task places very high demands on WM, it may leave too few attentional resources available for generating the distressing image. According to the WM account of EMDR, if the distressing image is not concurrently held in mind during the competing task, the image and task will not compete for WM resources, meaning the task will not reduce the vividness of the image. The EM task used in the current experiment may place greater demands on WM than the EM tasks used in previous studies, which typically involve looking at a moving stimulus such as circle (Gunter & Bodner, 2008) or the experiment's moving hand (Van den Hout et al., 2001). Participants in the current experiment were additionally required to detect when the moving letter had been replaced with a target letter. The requirement to actively monitor and respond to changes in the stimulus presumably places greater demands on WM than simply tracking the stimulus as it moves. Participants in the EM and auditory task conditions reported greater difficulty retrieving the target image if it had dropped from attention while dual-tasking compared to participants in the fixation task condition. These ratings were consistent with subjective ratings of task difficulty, which showed the EM and auditory tasks were harder than the fixation task. While this suggests the participants in the EM and auditory task conditions found it harder to divide their attention between the task and the image, it seems unlikely that our experimental tasks were so demanding of executive attention that they entirely prevented the retrieval of the target image – ratings of image retrieval were well below ceiling in the EM and auditory task conditions, suggesting participants in these conditions were able to regenerate the image during the dual-task procedure. It also seems unlikely that participants in the EM and auditory task conditions were paying more attention to the target image than the task, which one might anticipate if the task was so difficult that concurrently holding the image was not possible. Data regarding the responses to targets showed that of the twelve targets presented per task condition, the mean number of valid responses was $M = 10.708$ for the EM task and $M = 11.208$ for the auditory task. While it is possible the participants who performed the STM test procedure first would have known they could focus their full attention on the image after detecting the second target letter, and therefore appear to be performing the task, counterbalancing ensured that any effect of practice was averaged across all participants. In summary, while it is possible our experimental tasks placed higher demands on the central executive than tasks used in previous EM studies, there was little evidence to suggest that participants were unable to focus on the image while also performing the dual-task, meaning the image should have been susceptible to the WM interfere caused by the EM and auditory tasks.

5 Chapter 5: Comparing the effects of auditory and eye movement interference on negative recollection using a within-subjects design

Experiments 1b and 2b failed to replicate the findings of previous studies that show EMs reduce the vividness and emotionality of distressing imagery more than keeping both eyes stationary. Unlike experiments 1b and 2b, the majority of EM studies have used a within-subjects design, which generally has more statistical power than a between-subjects design. Within-subjects designs were used in early EMDR mechanism studies to control for individual variations in the use of rating scales (Andrade et al., 1997), and have been used in most of the subsequent literature showing an effect of EMs on image vividness and/or emotionality versus no-EMs (Kemps & Tiggemann, 2007; Lilley et al., 2009). The aim of experiment 3, described in this chapter, was therefore to compare the effect of the EMs task on negative imagery to the effect of the auditory and fixation tasks using a more powerful within-subjects design.

In experiment 3, we re-introduced the requirement for participants to rate the distressing image at baseline. As mentioned earlier, we decided to omit baseline ratings of imagery in experiment 2b due to our concern that participants may use the baseline rating as an anchor when giving subsequent ratings. Since experiment 3 used a within-subjects design, it was not possible to prevent participants from using the ratings in one condition as an anchor for the ratings given in a subsequent task condition. Furthermore, by including baseline ratings, it is possible to determine if the effect of dual-tasking is due to a decrease in ratings in the EM and auditory task conditions, or an increase in ratings in the control condition. This information is important when trying to establish the therapeutic mechanism of EMs in EMDR.

5.1 Experiment 3: comparing the effect of the interference tasks on distressing imagery

Our predictions were the same as in experiments 1B and 2B. We predicted that compared to the fixation and auditory tasks, performing the EMs task would result in less vivid and emotional recall when distressing images were rated immediately after the dual-task procedure, after a subsequent period of exposure to the image, and when the image was retrieved 24 hr later.

5.1.1 Participants

The closest resemblance to the current study is Van den Hout et al. (2011a). Similarly, to the current experiment, these authors used a non-clinical sample and compared the effect of EMs and auditory interference (tones) on negative imagery using a within-subjects. The duration of dual-task performance and the number of blocks of dual-tasking was similar to the current experiment. The study results revealed that EMs caused a greater reduction in image vividness compared to recall only (r = .42, or d = .93) and listening to alternating beeps through headphone (r = .27 or d = .56). Based on the most conservative of these effects (d = .56), an alpha of 0.016, and power of .8, we estimated that a total sample size of n = 31 would be required to find a significant difference between our EM and auditory tasks.

In order to counterbalance our three task conditions, we recruited 36 participants, consisting of psychology undergraduates (32 female; modal age was between 18-22 years) from Plymouth University. Participants took part in exchange for course credits. All participants were native English speakers.

5.1.2 Design and Procedure

The experiment was a within-subjects design adapted from Van den Hout et al. (2010). Participants generated three visual mental images of different negative autobiographical memories, following the same instructions used in the previous experiments. They gave each memory a short descriptive label and rated them in terms of emotionality. The experimenter used these initial emotionality ratings to allocate the images in a counterbalanced order to the three concurrent task conditions, which were also counterbalanced.

The concurrent tasks were same as those used in the previous experiments: central fixation, a visuospatial task, and an auditory task. Within task conditions, participants initially spent 20 s visualising the image, after which they completed the vividness, emotionality, and negative affect rating scales used in the previous experiments. Next, they held the image in mind for six 20 s intervals while simultaneously performing the concurrent task.

Immediately after the sixth task block, they rated the image using the same rating scales. These ratings were repeated again around 1 min after the final task block, after a 20 s period of visualisation. Participants were dismissed once all three tasks had been completed. Images was recalled in the same order approximately 24 hours later, outside of the original testing environment. Each image was visualised for 20 s, after which the participant completed the vividness, emotionality and negative affect rating scales using an online survey. Participants were also asked to estimate the number of time the distressing image had intruded into awareness since the dual-task procedure.

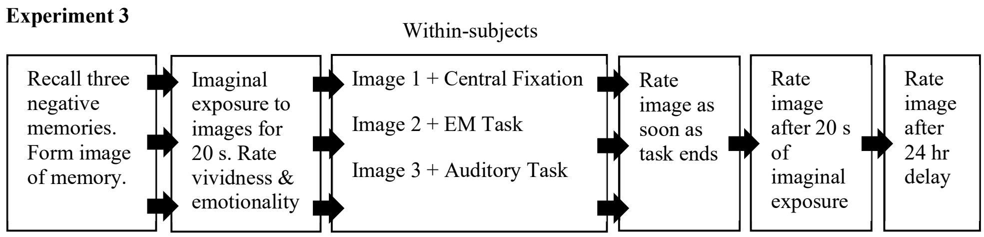

Figure 11: representation of the general procedure used in experiment 3.

5.1.3 Results

Four participants did not respond to the questionnaire sent at 24 hr follow-up. As the sample size estimate for experiment 3 was based on pre-post decrease in image vividness caused by EMs, it could be argued that our analysis of image vividness and emotionality at 24hr follow-up did not have sufficient power to detect an effect of EMs compared to the fixation task condition. However, given that the difference between task conditions at follow-up was small, it is unlikely that the small number of people who dropped out of the study would have significantly changed our conclusions.

Vividness

Figure 12 shows the mean change in vividness from baseline to immediately after the dual-task procedure, after subsequent exposure to the image and at 24 hr follow-up. Histograms and Q-Q plots showed the distribution of vividness scores immediately after the fixation task were negatively skewed. The distribution of scores continued to appear non-normal after being transformed using square and exponential transformations. We therefore used non-parametric tests to compare task conditions in terms of the change in vividness from baseline to immediately after the dual-task procedure. Aligned rank transformation was used to convert vividness scores, which were then analysed using a mixed 2 x 3 ANOVA. This analysis showed a significant main effect of task, $F(2, 70) = 6.612$, $p = .002$, and time, $F(1, 70) = 9.708$, $p = .002$, and the crucial interaction between time and task condition was also significant, $F(2, 70) = 7.258$, $p < .001$. To investigate this interaction, planned comparisons between the EM and fixation task, and the EM and auditory task were performed on the change in vividness from baseline. Wilcoxon signed-rank tests, with a Bonferroni correction showed that the EM task led to a significantly larger decrease in mean ranked vividness compared to the fixation task, $Z = -3.285$, $p = .001$. In contrast, the EM and auditory task did not differ in terms of the decrease in vividness from baseline, $Z = -0.135$, $p = .893$.

Histograms and Q-Q plots showed the distribution of vividness scores taken after 20 s of imaginal exposure post-task were negatively skewed in the fixation task condition. Scores still appeared non-normal after using square and exponential transformation. We therefore compared vividness scores using non-parametric tests. A Friedman test failed to find significant differences between task conditions, $\chi 2(2) = 1.784$, $p = .41$.

Histograms and Q-Q plots showed the distribution of vividness scores taken 24 hr after the dual-task procedure were normally distributed, therefore we analysed these scores using parametric tests. A repeated-measure ANOVA failed to find significant differences between task conditions, $F(2, 70)$, = 0.561, $p = .574$, $\eta p^2 = .018$.

In summary, the EM and auditory tasks caused a larger decrease in baseline vividness compared to the fixation task, but this difference was not significant after 20 s of recalling the image post-task, or 24 hr after the dual-task procedure.

Emotionality

Figure 12 shows the condition means for image emotionality at baseline, after both post-task image rating procedures and at 24 hr follow-up. Histograms and Q-Q plots showed that the emotionality scores taken at baseline and immediately after dual-tasking were normally distributed in all task conditions. These data were analysed using a 2 (Time: baseline; immediately post-task) x 3 (Task: fixation; EM; auditory) repeated-measures ANOVA. The main effect of task was not significant $F(2, 70) = 1.473$, $p = .236$, $\eta p^2 = .04$. The main effect of time was significant, $F(1, 35) = 11.796$, $p = .022$, $\eta p^2 = .252$, as was the interaction between time and task condition, $F(2, 70) = 3.427$, $p = .038$, $\eta p^2 = .089$. To investigate this interaction, the mean change in emotionality from baseline to immediately post-task was compared between the EM and fixation task, and the EM and auditory task using paired t-tests with Bonferroni correction. In contrast with our predictions, the EM task did not cause a significantly greater decrease in image vividness from baseline compared to the fixation task, $t(35)$, = 1.844, $p = .074$, or the auditory task, $t(35)$, = -0.188, p = .852. This suggested the interaction was significant because the auditory task caused a greater decrease in emotionality than the fixation task. This was confirmed using a separate t-test with Bonferroni correction for three comparisons (to reduce the risk of type 1 error), which showed a larger decrease in emotionality in the auditory versus fixation task condition, $t(35)$, = -2.606, p = .013.

Histograms and Q-Q plots showed that the emotionality scores taken post-task following 20 s of imaginal exposure were normally distributed in all task conditions. A repeated-measure ANOVA failed to find significant differences between task conditions, $F(2, 70)$, = 0.209, p = .812, ηp^2 = .006.

Histograms and Q-Q plots showed the distribution of emotionality scores taken 24 hr after the EM task was negatively skewed and bimodal. Transforming these scores failed to produce data that were normally distributed, therefore we compared emotionality scores at 24 hr follow-up using non-parametric tests. A Friedman test failed to find significant differences between task conditions, $\chi 2(2)$ = 0.349, p = .84. This null-result was not due to the use of non-parametric tests, as making the same comparisons using a repeated-measure ANOVA also failed to find significant differences between task conditions, $F(2, 70)$, = 0.288, p = .751, ηp^2 = .009.

In summary, while all tasks led to an immediate reduction in image emotionality from baseline, the effect of the EM task was not larger than that of the fixation task or auditory task. There was no effect of task condition on emotionality when participants recalled the image for 20 s at post-task, or at 24 hr follow-up.

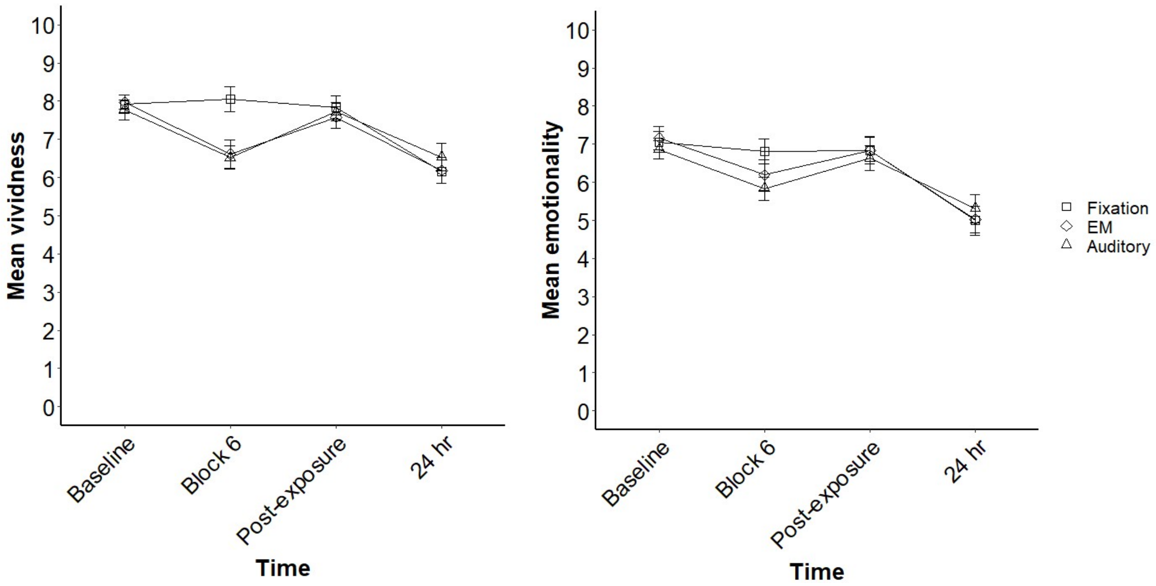

Figure 12: mean image vividness and emotionality at baseline, immediately after dual-tasking (block 6), after 20 s of imaginal exposure post-task (post-exposure), and at 24 hr follow-up in experiment 3. Error bars represent standard error.

Negative affect

Figure 13 shows the change in negative affect scores across the experiment and at follow-up in each task condition. Histograms and Q-Q plots showed that negative affect ratings were positively skewed at all four measurement points in all task conditions. The logarithm of these ratings were normally distributed, therefore we analysed the effect of task condition using these transformed ratings.

A 2x3 repeated-measures ANOVA using Huynh-Feldt estimates of sphericity (ε = .881) showed a significant decrease in negative affect from baseline to immediately after dual-tasking, $F(1, 35) = 10.22$, $p = .003$, $\eta p^2 = .226$. There was no main effect of task, $F(1.761, 61.651) = 0.656$, $p = .504$, $\eta p^2 = .018$, and the decrease in negative affect was not affected by task condition, $F(2, 70) = 2.533$, $p = .087$, $\eta p^2 = .067$.

Consistent with our analysis of vividness and emotionality, we separate repeated-measures ANOVAs were used to test the effect of task condition on negative affect ratings after 20 s of exposure to the image post-task, and at 24 hr follow-up. The results showed no effect of task condition on negative affect scores taken after imaginal-exposure, $F(2, 70)$, = 0.448, p = .641, ηp^2 = .013, or at 24 hr follow-up, $F(2, 70)$, = 1.022, p = .366, ηp^2 = .032. This shows the null-effect of task condition on ratings of negative affect was consistent across the experiment.

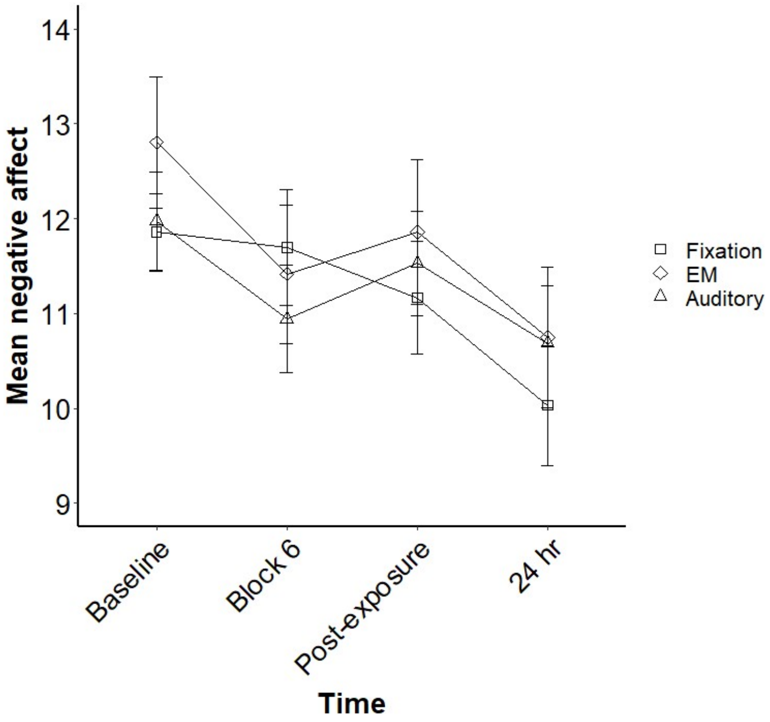

Figure 13: mean negative affect at baseline, immediately after dual-tasking (block 6), after 20 s of imaginal exposure post-task (post-exposure), and at 24 hr follow-up in experiment 3 (24 hr). Error bars represent standard error.

Task difficulty, task pleasantness, image retrieval difficulty and image intrusiveness

Table 5 provides the condition means for subjective ratings of task difficulty, pleasantness, difficulty of retrieving the image during the dual-task, and the mean number of times the distressing image intruded into awareness in the 24 hours following the dual-task procedure.

Histograms and Q-Q plots showed task difficulty scores were highly positively skewed in the fixation task condition, and moderately positively skewed in the auditory task condition. Square root and logarithmic transformations failed to produce scores that were normally distributed in all task conditions, therefore we compared the difficulty of each task using non-parametric tests. The results of a Friedman test showed that tasks differed significantly in terms of their subjective difficulty, $\chi 2(2) = 35.652$, $p < .001$. Post-hoc comparisons were performed using Wilcoxon signed-rank tests with Bonferroni correction, meaning alpha was .017 for each pairwise comparison. Results showed that the fixation task was easier than the EM, $Z(35) = -4.538$, $p < .001$, and auditory task $Z(35) = -2.905$, $p = .004$, while the auditory task was easier than the EM task, $Z(35) = -2.855$, $p = .004$.

Histograms and Q-Q plots showed that the distribution of scores for task pleasantness was multimodal in the fixation task condition, and was moderately positively skewed in the auditory task condition. Since there were multiple ways in which the data were non-normal, we used non-parametric tests to compare the pleasantness of the task conditions. A Friedman test showed that the dual-task conditions did not differ significantly in terms of subjective pleasantness, $\chi 2(2) = 5.791$, $p = .055$.

In terms of how difficult it was to regenerate the image while dual-tasking, difficulty ratings were highly positively skewed in the fixation task condition. The distribution of scores was not normal are performing square root and logarithmic transformation, therefore we based our analysis on non-parametric tests. The results of a Friedman test showed that image retrieval difficulty varied significantly between task conditions, $\chi 2(2) = 23.226$, $p < .001$. Post-hoc comparisons were performed using Wilcoxon signed-rank tests with Bonferroni correction, therefore alpha was .017. Results showed that compared to the fixation task, participants found it more difficult to retrieve the target image during EM task, $Z(35) = -3.982$, $p < .001$, and the auditory task, $Z(35) = -3.449$, $p = .001$. The difficulty of retrieving the image did not differ significantly between the EM and auditory tasks, $Z(35) = -866$, $p = .387$.

Histograms and QQ plots showed the distribution of scores was positively skewed in all conditions. This was because most participants reported no/few intrusions of the image. Square and logarithmic transformations failed to produce scores that were normally distributed, therefore the number of intrusions was compared between task conditions using non-parametric tests. The results of a Friedman test showed in the 24 hr following the dual-task procedure, there were no statistically significant differences between task conditions in terms of the number of times the image intruded into awareness, $\chi 2(2) = 2.218$, $p = .33$.

Table 5: Condition means (and standard deviations) for ratings of task difficulty, task pleasantness, image retrieval difficulty during the dual-task, and number of image intrusions in the 24 hr after the dual-task procedure.

	Fixation	EM	Auditory
	M (SD)	M (SD)	M (SD)
Task difficulty	1.722 (2.3)	4.694 (2.692)	3.278 (2.721)
Task pleasantness	3.833 (3.176)	4.917 (2.739)	3.722 (2.825)
Retrieval difficulty	2.5 (2.762)	5.583 (2.75)	5.028 (3.009)
No. intrusions	1.813 (2.278)	1.75 (2.109)	2.25 (2.553)

5.1.4 Discussion

Consistent with the findings of experiments 1b and 2b, experiment 3 failed to support our prediction that EMs would reduce the vividness and emotionality of distressing imagery more than performing an auditory task. We found that the EM and auditory tasks led to a similar decrease in image emotionality from baseline to immediately after the dual-task procedure. Our results do not support the finding in previous studies that EMs reduced image vividness and emotionality more than concurrent articulation (Andrade et al., 1997; Lilley et al., 2009). In these studies, the auditory task – counting upwards from one – may have imposed a smaller general cognitive load than the EM task. The larger effect of EMs in the studies by Andrade et al. and Lilley et al. may reflect the larger central executive demands of the EM task compared the auditory task. Therefore, the simplest explanation for why we failed to replicate the effect of EMs in these studies is that the auditory task used in the current experiment placed greater demands on the central executive. It seems unlikely that our study lacked the statistical power to detect a difference between the EMs and auditory task. First, experiment 3 used a more powerful within-subjects design. This was an improvement on experiments 1b and 2b, which used a between-subjects design, because there should have been less variation caused by the interpretation of our subjective measures of vividness and emotionality. Second, there was a trend whereby the auditory task caused a larger decrease in image emotionality than the EM task. This suggests that our finding that EMs had no greater impact on image emotionality than the auditory would have been the same even if the study had been more powerful. It seems more likely that we failed to find a larger effect of EMs on imagery compared to the auditory task because the modality-specific WM demands of the tasks contribute little to their effect on imagery. Our findings are more consistent with evidence that the effect of EMs and other dual-tasks on imagery are primarily caused by their general or executive WM demands (Gunter & Bodner, 2008; Kristjánsdóttir & Lee, 2011; Matthijssen et al., 2017).

The results of experiment 3 offer mixed evidence regarding the immediate effect of taxing WM on image vividness and emotionality. In contrast to experiments 1b and 2b, we found clear evidence that EMs caused a larger decrease in image vividness from baseline to immediately after the dual-task procedure than the fixation task. It is possible that the within-subjects design used in the current experiment had more power to detect the added effect of EMs than experiments 1b and 2b. Our findings support previous studies that have found an immediate effect of EMs on the vividness of imagery compared to keeping both eyes stationary (Andrade et al., 1997; Lilley et al., 2009). We also replicated the emotional benefit of taxing the central executive during recall: compared to the fixation task, performing the auditory task during recall caused a larger decrease in image emotionality from baseline to immediately after the dual-task. However, the general cognitive load of the EM did not cause a larger reduction in image emotionality compared to the fixation task, which is consistent with the results of experiment 2b. I highlighted in chapter 1 that the majority of laboratory studies have found an added effect of EMs on both the vividness and emotionality of images of negative autobiographical events (Gunter & Bodner, 2008; Kavanagh et al., 2001; Kemps & Tiggemann, 2007; Kristjánsdóttir & Lee, 2011; Leer et al., 2014; Van den Hout et al., 2001; van Schie et al., 2016; van Veen et al., 2016; van Veen et al., 2015). The results of experiments 3 support a smaller number of studies that have found an added effect of EMs on the vividness of negative recall but not its emotionality (Maxfield et al., 2008; Van den Hout et al., 2011b, experiment 2; Van den Hout et al., 2011a, experiment 4). It is unclear why emotionality was affected by the general cognitive demands of the auditory task but not the EM task. This result cannot be explained by differences in task pleasantness or negative affect, which did not differ between task conditions. It also seems unlikely that these results can be explained by the greater difficulty of the EM task compared to the auditory task, as the difference in subjective difficulty was small. In the next chapter I discuss the results of experiment 4, which suggest we could have observed an emotional benefit of EMs compared to fixation in the current experiment if participants had been instructed to rate the target image during the dual-task procedure. Furthermore, the repeated measurement of imagery in experiment 4 supports the assumption that image vividness and emotionality are closely related and are similarly affected by dual-tasking.

Our experiment extends previous research by showing that the immediate effects of dual-tasking on imagery are eliminated by engaging in a brief period of imaginal exposure alone. After completing the dual-task procedure, participants were instructed to focus on the distressing image for a further 20 s before re-rating the vividness and emotionality of the image. Ratings taken after imaginal-exposure did not differ between task conditions. It is possible this rapid recovery of the image reflects the shorter amount of time that participants were given to generate the image when instructed to rate the image immediately after the dual-task procedure. However, other studies have found a larger effect of EMs when images were rated after a similar period of imaginal-exposure at the end of the dual-task procedure (Leer et al., 2014; Van den Hout et al., 2001). It is unclear why our results should differ from previous findings simply because participants were additionally required to rate the image immediately after the dual-task procedure. One possible explanation is that we instructed participants to rate the image both immediately the dual-task and again after imaginal-exposure moments later – both approaches to measuring imagery have been used in previous studies, but not in combination. A tentative explanation is that ratings taken immediately after the task - when the effect of dual-tasking would have been most pronounced – made the recovery of the image at post-task more noticeable. I argued in the previous chapter that participants may attempt to compare the current version of their mental image with the most recently available version and give ratings that are adjusted to the degree these images differ. According to this argument, an image recalled after a period of imaginal-exposure may appear more vivid and emotional if the previous retrieval of the image was degraded by WM interference than if previous retrieval of the image was relatively unscathed by dual-tasking. Put differently, current judgements about the vividness of the image held on mind may be biased by how vivid the image was the last time it was retrieved, and this bias becomes larger to the degree the images are subjectively different. Assuming participants use the most recent version of the image as a comparator, older ratings such as those taken at baseline may not be taken into consideration when participants are asked to rate their current image. It follows that comparing baseline ratings taken on the third, fourth etc. occasion would give a misleading impression of how much the image has changed from baseline. Each rating taken may only give an accurate measure of how much the image has changed from the previous occasion the image was retrieved.

Lastly, we failed to find a difference between task conditions when participants recalled and rated the target image 24 hr after the original dual-task procedure. Our finding is consistent with evidence that performing EMs during negative recall has no added effect on future recollections of the same memory 24 hr (Littel, Kenemans, et al., 2017; van Veen et al., 2019) or one-week post-task (Kavanagh et al., 2001; Lilley et al., 2009). There are potentially important differences between our experience and those that have found a lasting effect of EMs on the vividness and/or emotionality at 24 hr (Leer et al., 2014) and one week follow-up (Gunter & Bodner, 2008; Schubert et al., 2011). For example, participants in Schubert et al. performed the full EMDR procedure, whereas participants in the current experiment merely performed EMs during negative recall. It is possible the long-term emotional benefit of EMs in their study reflects the contribution that EMs made to other therapeutic processes in EMDR, such as the generation of new connections between the target memory and new memories. In the studies by Gunter and Bodner (2008) and Leer et al. (2014), post-task and follow-up ratings were taken in the same location, whereas we did not control the environment in which follow-up ratings were given. Perhaps our procedure created variation in scores at follow-up due to environmental factors, such as noise and distractions, and this is why we failed to find a larger effect of EMs at follow-up. Perhaps a more straightforward explanation for why we did not observe an added effect of EM on imagery at follow-up was because there was no difference between the effect of the EMs and fixation tasks at the end of the experiment (after imaginal exposure). As mentioned earlier, we used a more sensitive procedure in experiment 4 in the hope this would establish a clearer effect of EMs after the dual-task procedure. The aim was then to test if the effect of EMs would be maintained at follow-up.

To return to the point made earlier that participants may be making comparisons between images, we suggest the tendency to rate an image by comparing it with a previous instance may help design a more sensitive measure of task effects on imagery. Repeated measurements of imagery during the dual-task phase of the study should provide a finer-grained and potentially more sensitive measure of the effects of tasks on vividness and emotionality. Previous studies have measured imagery after each of several blocks of dual-tasking and found reliable differences between EMs on image vividness and emotionality compared to keeping both eyes stationary (Kavanagh et al., 2001; Lilley et al., 2009). Given that experiments 1 and 2 failed to find reliable effects of task condition on image vividness and emotionality, experiment 4 was used to determine if the results presented in the current study would be reliably replicated when participants provided multiple ratings during the dual task phase. Furthermore, it was mentioned in the opening chapter that repeated measurements of imagery during the dual-task intervention provide more detailed information about the relationship between changes in vividness and emotionality. As such, the procedures used in experiment 4 provided a stronger test of the WM hypothesis, which predicts that changes in vividness and emotionality should be closely related and thus follow a similar trajectory over time.

6 Chapter 6: Comparing the impact of auditory and eye movement interference on repeated measurements of recollection

This chapter describes the methods and results of experiment 4. This experiment was essentially a replication of experiment 3, except participants rated the target image between each 20 s block of the dual-task procedure. This repeated-rating method was introduced by Kavanagh et al. (2001) to mimic the desensitisation phase of EMDR, and has also been used in other analogue EMDR research (Lilley et al., 2009; Smeets et al., 2012; van Veen et al., 2019). It is possible in these studies to see whether the pre-post change in image vividness and emotionality are an outlier, or if they are representative of the changes that are happening to the image throughout dual-task procedure. The aim of experiment 4 was therefore to see if we could replicate the effect of EMs immediately after the dual-task procedure, and to test the consistency of this effect by asking participants to repeatedly rate their image between blocks of dual-tasking. As discussed at the end of chapter 5, we hoped that the requirement to rate images repeatedly in the dual task phase would increase the sensitivity of the experiment to differences between conditions, as participants had more opportunity to deliberately compare a rating with the one preceding it.

6.1 Experiment 4: comparing the effect of the interference tasks on distressing imagery

Our predictions were the same as in experiments 1-3. We predicted that the EM task would cause a greater reduction in image vividness and emotionality that the fixation and auditory tasks. We predicted that this added effect of EMs would be observed when the image was recalled immediately after the dual-tasking, after exposure to the image moments after the dual-task procedure, and at 24 hr follow-up.

6.1.1 Participants and Procedure

A sample of 36 psychology undergraduates and members of the public (30 female; modal age was between 18-27 yr), consisting of psychology undergraduates and members of the public, were recruited from Plymouth University in exchange for payment. Most of the participants were native English speakers (86%).

The general procedure was the same as experiment 3, except participants were asked to provide additional ratings of image vividness and emotionality. Specifically, immediately after each 20 s block of the dual-task procedure, participants were asked to rate the image at that moment using the rating scales presented on the computer. After submitting their ratings, a message appeared on screen instructing participants to bring the image to mind. They then held this image in mind while completing the dual-task, after which they were asked to rate the image again. As in experiments 1-3, participants completed six blocks of dual-tasking in each task condition, therefore they rated the vividness and emotionality of the image six times between the start and end of the dual-task procedure. Due to a programming error, rating scales for negative affect were not presented immediately after the final dual-task block. This mean unlike experiments 2b and 3, negative affect was only recorded at baseline, after imaginal exposure at the end of the dual-task procedure, and at 24 hr follow-up.

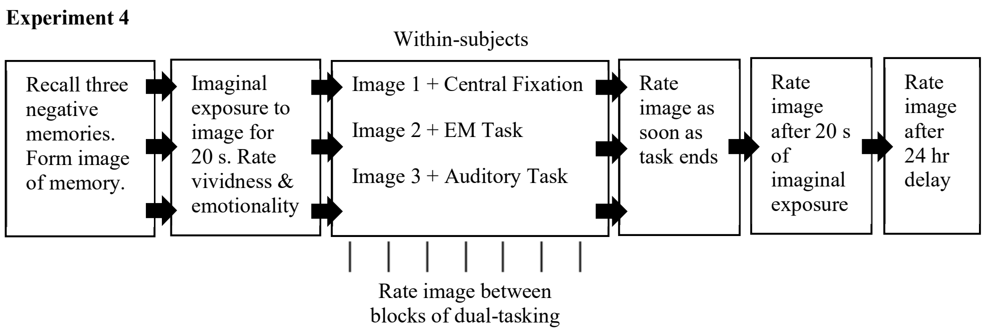

Figure 14: representation of the general procedure used in experiment 4.

6.1.2 Results

Vividness

Figure 15 shows vividness ratings at baseline, across blocks of the dual-task procedure, at post-task following imaginal exposure, and at 24 hr follow-up. We investigated the effect of task condition by comparing vividness at baseline and immediately after the dual-task procedure. As in previous experiments, vividness scores were non-normal in ways that meant scores could not be corrected for parametric analysis. We therefore used non-parametric tests investigate the predicted effect of EMs. Vividness scores at baseline and after block six of the dual-task were transformed using aligned rank transformation. These transformed scores were then analysed using a 2 (Time: baseline, immediately post-task) x 3 (task condition) repeated-measures ANOVA. Results showed a significant main effect of time, $F(1, 175) = 150.375$, $p < .001$, and task, $F(2, 175) = 10.6$, $p < .001$, and a significant interaction between time and task, $F(2, 175) = 12.916$, $p < .001$. This interaction was analysed by comparing task conditions in terms of the change in vividness from baseline. A Friedman test showed the change in vividness differed significantly between task conditions, $\chi 2(2) = 25.409$, $p < .001$. Post-hoc comparisons were performed using Wilcoxon signed-rank tests with Bonferroni correction (alpha was .025 for each comparison). As predicted, results showed that the EM task caused a significantly larger decrease in image vividness than the fixation task, $Z(35) = -4.447$, $p < .001$. The predicted effect of EMs compared to the auditory task was not significant, $Z(35) = -1.018$, $p = .308$.

To determine if the effect of EMs was consistent across time, we compared the effect of task condition across blocks of the dual-task procedure. Because of non-normality, non-parametric tests were used to analyse the change in vividness across blocks of the dual-task procedure. Scores after each task block were transformed using aligned rank transformation. These transformed scores were then analysed using 6 (time: blocks 1-6) x 3 (task condition) repeated measures ANOVA. Results showed a significant reduction in vividness over time, $F(5, 595) = 11.796$, $p < .001$, and a significant effect of task condition, $F(2, 595) = 114.599$, $p < .001$. The interaction between task condition and time was not significant, $F(10, 595) = 1.459$ $p = .151$, indicating the effect of task condition on vividness was consistent across blocks of the dual-task procedure.

We tested the effect of task condition after the dual-task procedure, following 20 s of imaginal exposure. As vividness scores were negatively skewed in the fixation and EM task conditions, square root transformation was performed which corrected for skewness. These transformed vividness scores were compared using a repeated-measures ANOVA, which showed a significant effect of task conditions, $F(2, 70)$, = 4.388, $p = .016$, $\eta p^2 = .111$. Planned comparisons were performed using paired-samples t-test. In line with our prediction, vividness was lower in the EM condition compared to the fixation task condition, $t(35) = -2.239$, $p = .032$, Cohen's dz = 0.373. Note, however, that this difference is not statistically significant with Bonferroni correction to alpha of .025 to account for running two post-hoc tests. The predicted effect of EMs compared to the auditory task was not significant, $t(35) = 0.452$, $p = .654$, Cohen's d_z = 0.07.

Vividness scores at 24 hr follow-up[3] were analysed using a repeated-measures ANOVA. This analysis failed to find a statistically significant effect of task condition on vividness 24 hr after the dual-task procedure, $F(2, 56)$, = 0.614, $p = .545$, $\eta p^2 = .021$.

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[3] Seven participants did not respond to the questionnaire sent at 24 hr follow-up.

Emotionality

Figure 15 shows emotionality ratings at baseline, across blocks of the dual-task procedure, at post-task following imaginal exposure, and at 24 hr follow-up. As emotionality scores were non-normal immediately after the dual-task procedure, we used non-parametric tests to investigate the prediction that EMs would cause the largest reduction in emotionality from baseline to immediately after the procedure. Scores at baseline and after the final block of dual-tasking were transformed using aligned rank transformation. These transformed scores were then analysed using a 2 (Time: baseline, immediately post-task) x 3 (task condition) repeated-measures ANOVA. Results showed a significant main effect of time, $F(1, 175) = 119.229$, $p < .001$, and task, $F(2, 175) = 13.665$, $p < .001$, and a significant interaction between task and time, $F(2, 175) = 9.965$, $p < .001$. This interaction was analysed by comparing task conditions in terms of the change in emotionality from baseline. A Friedman test showed the change in emotionality differed significantly between task conditions, $\chi 2(2) = 18.197$, $p < .001$. Planned comparisons were made using Wilcoxon signed-rank tests, with Bonferroni correction (alpha was .025 for each comparison). Results were consistent with our findings for vividness. As predicted, the decrease in emotionality from baseline was larger in the EM condition compared to the fixation condition, $Z(35) = -3.486$, $p < .001$. The predicted effect of EMs compared to the auditory task was not significant, $Z(35) = -0.193$, $p = .847$.

To determine if the effect of EMs was consistent across time, we compared the effect of task condition across blocks of the dual-task procedure. Because data were non-normal, the effect of task condition was analysed using non-parametric tests. The scores after each task block were transformed using aligned rank transformation. These transformed scores were analysed using a 6 (time: blocks 1-6) x 3 (task condition) repeated measures ANOVA. Results showed a significant reduction in emotionality over time, $F(5, 595) = 10.104$, $p < .001$, and a significant effect of task condition, $F(2, 595) = 94.63$, $p < .001$. The interaction between task and time was not significant, $F(10, 595) = 1.562$ $p = .114$, indicating the effect of task condition on emotionality was consistent across blocks of the dual-task procedure.

We tested the effect of task condition when images were rated following 20 s of imaginal exposure after the dual-task procedure. The effect of task condition was analysed using a repeated-measures ANOVA, which showed significant differences between task conditions, $F(2, 70)$, = 5.09, $p = .009$, $\eta p^2 = .127$. Planned comparisons were performed using paired-samples t-test. In line with our prediction, emotionality was lower in the EM condition compared to the fixation task condition, $t(35) = -2.324$, $p = .026$, Cohen's $d_z = 0.387$. Note, however, that this difference is not statistically significant with Bonferroni correction to alpha of .025 to account for running two post-hoc tests. The predicted effect of EMs compared to the auditory task was not significant, $t(35) = -0.827$, $p = .414$, Cohen's $d_z = 0.138$.

The effect of task condition on image emotionality at 24 hr follow-up was analysed using a repeated-measures ANOVA. As Mauchly's test indicated that the assumption of sphericity had been violated, $\chi 2(2) = 0.696$, $p = .007$, degrees of freedom were corrected using Huynh-Feldt estimates of sphericity ($\varepsilon = .802$). This analysis failed to find a statistically significant effect of task condition on emotionality 24 hr after the dual-task procedure, $F(1.605, 44.934)$, = 0.657, $p = .491$, $\eta p^2 = .023$.

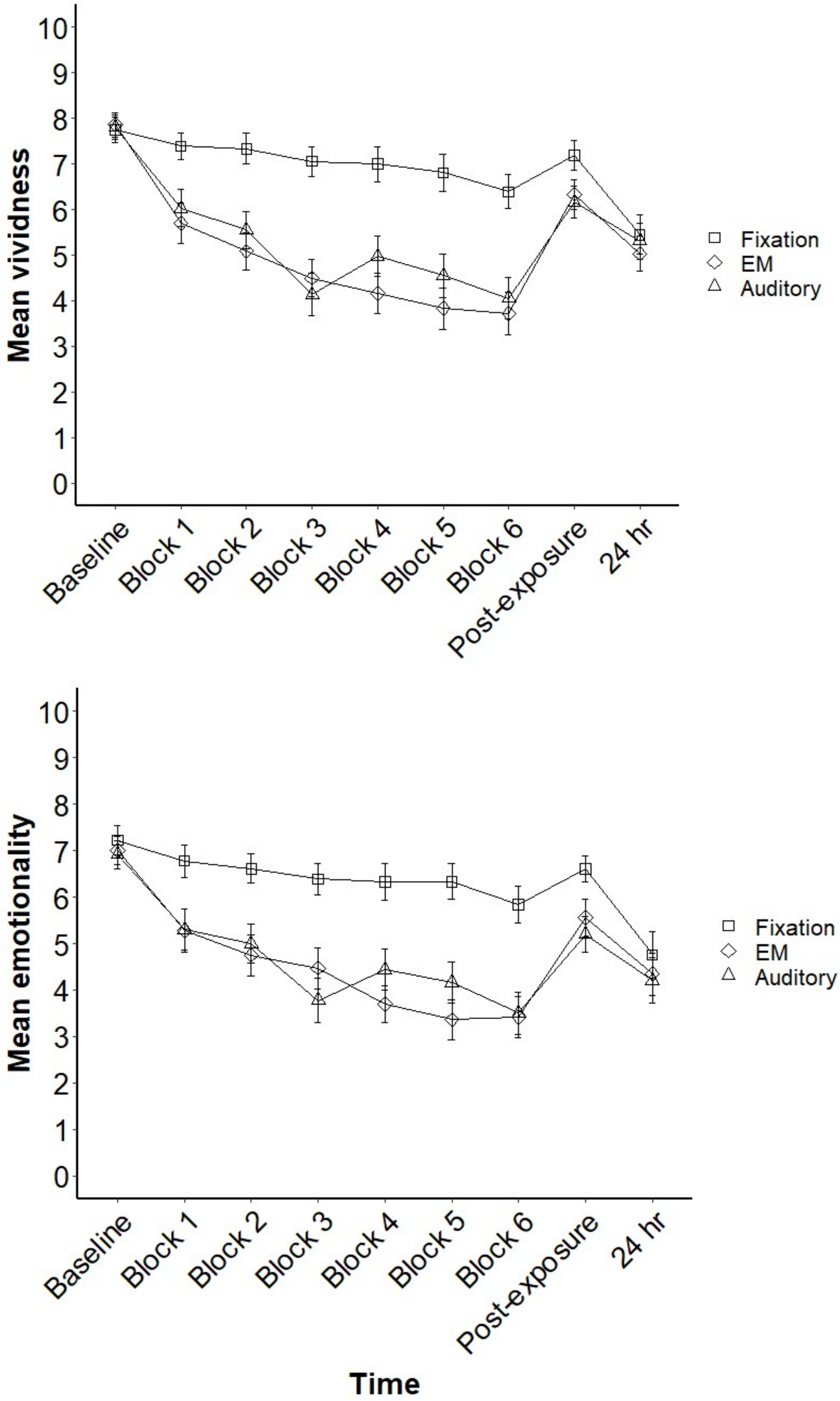

Figure 15: mean change in image vividness and emotionality across blocks of dual-tasking in experiment 4. Error bars represent standard error. Block 6 = immediately after dual-task. Post-exposure = 20 s of imaginal exposure post-task. 24 hr = one day post-task.

Negative affect

As mentioned earlier, due to a programming error, negative affect scores were not recorded immediately after the final block of the dual-task procedure. Figure 16 shows mean negative affect scores at baseline, at post-task following imaginal exposure, and at 24 hr follow-up. As some participants failed to provide ratings at 24 hr follow-up, it was not possible to test the interaction between task and our three measurement time points. Figure 16 shows that task conditions were closely matched at baseline in terms of mean negative affect score, meaning subsequent differences in ratings between task conditions could be attributed to the effect of task condition. We therefore separately compared the effect of task condition on scores taken after imaginal exposure, following the dual-task procedure, and at 24 hr follow-up.

Since the distribution of negative affect scores was non-normal, nonparametric tests were used to analyse the effect of task condition. Friedman's tests failed to find an effect of task condition on ratings of negative affect following imaginal exposure, $\chi^2(2) = 0.741$, $p = .69$, or at 24 hr follow-up, $\chi^2(2) = 0.19$, $p = .99$.

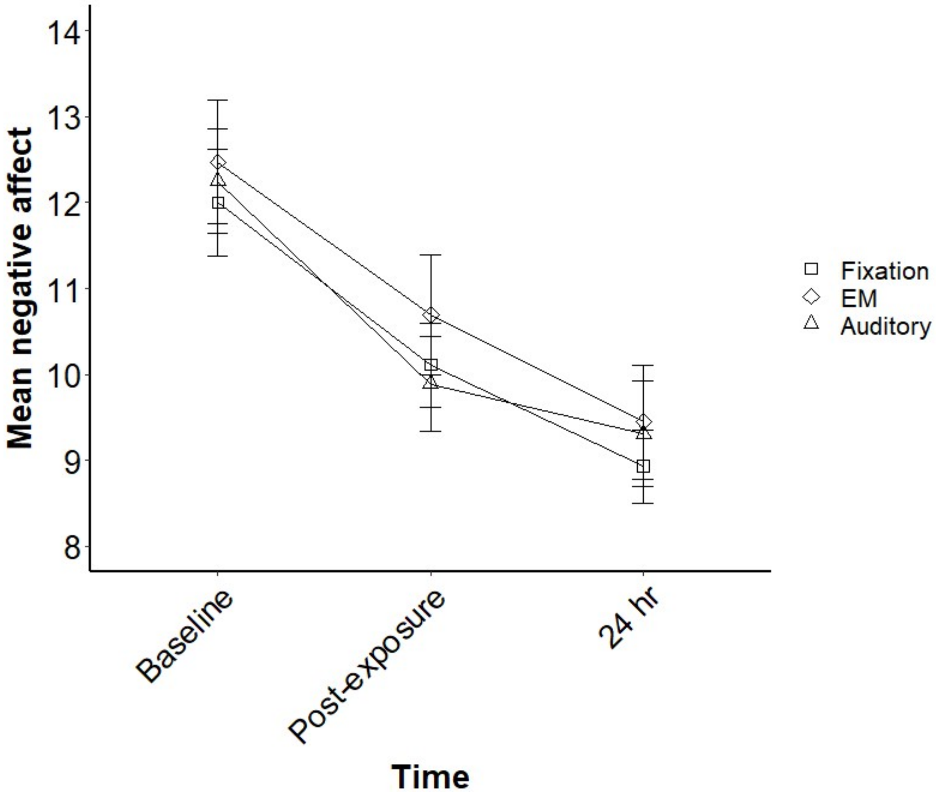

Figure 16: mean ratings of negative affect across experiment 4. Error bars represent standard error. Post-exposure = 20 s of imaginal exposure post-task. 24 hr = one day post-task.

Task difficulty, task pleasantness, image retrieval difficulty, and image intrusiveness

Table 6 shows the mean ratings for subjective task difficulty and pleasantness, the self-reported difficulty of retrieving the target image while performing the dual-task, and the number of times the image intruded into awareness between the experiment and 24 hr follow-up. Scores were non-normal in ways that meant scores could not be corrected for parametric analysis. Therefore, we compared the difficulty of the task conditions using non-parametric tests. A Friedman's test showed that tasks differed significantly in terms of self-rated difficulty, $\chi^2(2) = 9.807$, $p = .007$. Post-hoc comparisons using Wilcoxon signed-rank tests indicated the fixation task was easier than the EM task, $Z = -2.323$, $p = .02$, and the auditory task, $Z = -2.449$, $p = .014$. Note, however, that the difference between the EM and fixation task is not statistically significant with Bonferroni correction to alpha of .016, to account for running three post-hoc tests. The EM and auditory task did not differ in terms of subjective difficulty, $Z = -.0859$, $p = .391$.

Ratings of task pleasantness were normally distributed. A repeated-measures ANOVA indicates that task conditions differed significantly in terms of their subjective pleasantness. Pairwise comparisons using Bonferroni correction revealed that the auditory task was less pleasant than the fixation task, $p = .012$, and the EM task, $p = .008$. The EM and fixation tasks did not differ in terms of subjective pleasantness, $p = 1$.

Tasks conditions were compared in terms of the self-reported difficultly of retrieving the target image during the dual-task procedure. Because scores were non-normal in ways that meant they could not be corrected for parametric analysis, analysis were performed using non-parametric tests. A Friedman's test showed that ratings differed significantly between task conditions, $\chi^2(2) = 23.766$, $p < .001$. Planned comparisons using Wilcoxon signed-rank tests with Bonferroni correction (alpha was .017 for each comparison) showed that compared to the fixation task, participants found it more difficult to retrieve the target image during the EM task, $Z = -3.819$, $p < .001$, and the auditory task, $Z = -3.650$, $p < .001$. Image retrieval difficulty did not differ significantly between the EM and auditory task conditions, $Z = -0.207$, $p = .836$.

Table 6 shows the mean number of times the target image intruded into awareness in the 24 hr following the dual-task procedure. Histograms and Q-Q plots showed that the distribution of scores in all task conditions were highly positively skewed. Square root and logarithmic transformation did not correct for skewness and scores. As scores were non-normal, the effect of task condition on the number of image intrusions was analysed using non-parametric tests. A Friedman's test showed that the number of times the image intruded did not differ significantly between task condition, $\chi^2(2) = 1.210$, $p = .546$.

Table 6: Condition means (and standard deviations) for ratings of task difficulty, task pleasantness, image retrieval difficulty during the dual-task, and number of image intrusions in the 24 hr after the dual-task procedure.

	Fixation	EM	Auditory
	M (SD)	M (SD)	M (SD)
Task difficulty	2.583 (2.568)	3.861 (2.909)	3.4 (3.001)
Task pleasantness	5.25 (2.371)	5.194 (2.703)	4.114 (2.374)
Retrieval difficulty	3 (2.878)	5.75 (2.761)	5.886 (2.958)
No. intrusions	1.69 (2.089)	1.724 (2.051)	1.621 (2.227)

6.1.3 Discussion

In line with the results of experiments 1-3, the results of experiment 4 did not support our prediction that EM would have a larger effect on distressing visual imagery than the auditory task. We found that the EM and auditory tasks led to a similar decrease in image vividness and emotionality from baseline to immediately after the dual-task procedure. The use of repeated image ratings in the current experiment confirmed that the EM and auditory tasks caused a similar decrease in image ratings across blocks of the dual-task procedure. It seems unlikely that the similar decrease in ratings in the EM and auditory tasks in experiment 3 is an outlier, given the effect of these tasks was similar throughout the dual-task procedure. The results of experiments 3 and 4 reliably demonstrate that the additional visuospatial WM interference caused by the EM task did not contribute to a larger impact on distressing visual imagery. As discussed in the previous chapter, the findings of experiments 3 and 4 contradict previous studies in which EMs caused a larger decrease in the vividness and emotionality of negative images compared to an auditory control task (Andrade et al., 1997; Lilley et al., 2009). The current experiment is most similar to Lilley et al., who used repeated ratings of the image during the dual-task procedure. Again, the larger effect of EMs in their study may reflect difference in the general cognitive load of the tasks, as the auditory task – counting aloud - was relatively easy. It is likely that we failed to find a larger effect of EMs compared to the auditory task in experiments 1-4 because they were closely matched in terms of the response speed, type of decision, and response modality, which should have meant they placed similar demands on the central executive.

The results of experiment 4 provide clearer evidence than experiments 1-3 that EMs reduce the vividness and emotionality of imagery more than keeping both eyes stationary. Compared to the fixation task, we found that performing the EM task while focusing on the negative image caused a larger decrease in both vividness and emotionality from baseline to immediately after the dual-task procedure. The effect of EMs on emotionality, which was marginal in experiment 3, was now statistically significant in the current experiment. As the only difference between experiments 3 and 4 was the number of times the images was rated, it is possible this difference in methodology made our procedure more sensitive to the effect of EMs on image emotionality. Later in this chapter, I will offer a tentative explanation for why the apparent effects of dual-tasking on image emotionality may be larger when how the image is repeatedly rated. As I explained in the previous chapter, the WM model does not predict that EMs should reduce the emotionality of imagery. However, this expected if we consider that vivid images more accurately represent the experience of relieving the imagined event, which in the current experiment was a distressing autobiographical memory. That is to say the WM model predicts that EMs will reduce the emotionality of imagery by reducing its vividness. An improvement in experiment 4 compared to experiments 1-3 was that participants repeatedly rated the target image throughout the dual-task procedure. These repeated ratings, which are summarised in Figure 15, make it easier to see the close relationship between the changes in image vividness and emotionality caused by dual-tasking. While our experiment was not designed to determine the causal relationship between changes in image vividness and emotionality, our results offer reliable evidence that both of these aspects of imagery are affected more by our experimental tasks than the fixation task, presumably because the latter places smaller demands on the central executive. Our findings are consistent with other studies that have used repeated measurements of imagery, which demonstrate that EMs cause similar decreases in the vividness and emotionality of imagery (Kavanagh et al., 2001; Lilley et al., 2009; Smeets et al., 2012; van Veen et al., 2019).

The current experiment replicated our finding in experiment 3 that the effects of EMs on imagery are short-lived. In both experiments, participants were asked at the end of the dual-task procedure to spend around 20 s focussing on the image before re-rating its vividness and emotionality. Despite finding a clear effect of task condition immediately after the dual-task procedure, image ratings did not differ significantly between the EMs and fixation conditions after imaginal exposure. As Figure 15 shows, this was because there was a larger increase in image vividness and emotionality in the short period of time (approximately 2 min) between rating the image immediately after the dual-task and then again after imaginal exposure. As I explained in the previous chapter, it is possible that when participants rated the image after imaginal-exposure, they rated the extent to which the image had changed since the last time the image was retrieved i.e. immediately after the dual-task procedure. The difference made by removing the EM task during imaginal exposure is likely to have been more noticeable than that of removing the fixation, considering the EM task had a larger effect on imagery immediately after the dual-task procedure. It is possible that had we made it more difficult for participants to compare the image post-exposure to the image immediately after the dual-task, such as increasing the interval between ratings, this could have encouraged participants to rate the image according to how closely it represented re-experiencing the negative episode. Again, the results of experiments 3 and 4 are at odds with studies that have found an effect of EMs on negative imagery after dual-tasking, in which ratings of vividness and emotionality were taken after a period of imaginal exposure (Leer et al., 2014; Van den Hout et al., 2001). As these studies did not include an additional image rating immediately after the dual-task procedure, it may have been more difficult for participants to compare the image retrieved at post-task to the image at baseline. Our argument suggests that the effect of EMs in these studies may have been smaller or negligible if participants had also rated the image during or immediately after dual-tasking.

We also failed to find a larger effect of EMs compared the other task conditions when images were rated again 24 hr after the dual-task procedure. This was consistent with the results of experiments 2 and 3, which used an identical follow-up procedure. As I explained in the previous chapter, we were unable to test if EMs have a long-term effect on distressing imagery because there was limited evidence that images were affected by EMs prior to their reconsolidation – ratings taken after imaginal exposure, at the end of the experiment did not differ significantly between conditions. If reconsolidation of the memory trace includes the most recent effects of dual-tasking, it is first necessary to demonstrate an effect of EMs prior to reconsolidation, which we did not find in this experiment of experiments 1-3.

Compared to experiment 3, the EM and auditory tasks in the current experiment caused a larger decrease in vividness and emotionality from baseline to immediately after dual-tasking. A notable difference between these experiments was that participants in experiment 4 rated the target image after each 20 s block of the dual-task. It is unclear why the frequency of task ratings would influence the reported effect of dual-tasking, but we suspect that asking participants to repeatedly rate the image made it easier to make comparisons between the image being rated and the previous retrieval of the image. When participants are asked to rate the vividness and emotionality of a mental image on multiple occasions, such as before and after a period of dual-tasking, it is possible this rating is based on a comparison between the current image held in mind to the memory of what the image looked and felt like the last time it was retrieved. It is more likely that comparisons will be made if it is easy to recall the vividness and emotionality of the image the last time it was retrieved. Something that would improve recall and therefore facilitate comparisons between the current and previous image is reducing the delay between retrievals of the image. In contrast, the memory for the previous image has degraded because the memory has degraded over time, for example, it is less likely participants will rate the current image relative to the vividness and emotionality of the previous image. As the participants in Experiment 4 retrieved the image every 20 s during the dual-task procedure, it is possible they tended to rate each retrieval of the image in relation to the previous image because it was easy to recall the vividness and emotionality of the previous image. In contrast, participants in Experiment 3 rated the image at baseline and then the next rating was taken after 2 min of dual-tasking. Continuing our logic, participants in Experiment 3 may have found it more difficult to compare the image at post-task with the image at baseline, and instead rated the image at post-task in line with the wording on the rating scales – these asked how vivid and emotional the image was compared to the real life experience of the imagined episode. If participants were comparing two versions of the image when rating the current image, there were three possible ways they could respond: rate the current image as more, less or equally vivid/emotional compared to the previous image. Since participants in Experiment 4 were asked to rate the image six times during the dual-task procedure, they may have reduced their ratings each time because the current image was less vivid than the previous image, even if the change was very small. This would explain why the pre-post decrease in image vividness and emotionality caused by the EM and auditory tasks was larger in the current experiment that in Experiment 3. If the experiment were repeated, it is possible that asking participants to rate the image more frequently, such as every 10 s, would have resulted in an even larger self-reported decrease in image ratings from baseline to post-task in our experimental task conditions. In summary, instructing participants to rate the same image frequently may facilitate comparisons between the current and most recent retrieval of the image. It is possible that by using frequent ratings, the effect of dual-tasking measured post-task represents the sum of changes in vividness and emotionality from baseline, rather than representing the likeness between the image and actual experience when the image is rated at post-task. It follows that increasing the frequency of ratings may increase the apparent effect of dual-tasking on imagery.

As I have highlighted that participants may rate imagery based on their recall of the previous image, it is also possible that participants rate the effect of one dual-task compared to another when the study uses a within-subjects design, as was the case in experiments 3 and 4. Often EM studies have excluded participants if they report prior experience of EMDR, or an awareness about the effect of EMs on imagery. This is done to prevent results from being influenced by the expectation that EMs will interfere with imagery more than the control task, which often involves keeping both eyes stationary, or recall alone. While excluding participants may help to remove the effect of existing knowledge, it is still possible that when participants perform the EM and control procedure in a within-subjects design, they may figure out the expected task effects and respond accordingly. Counterbalancing overcomes this issue by averaging out the effect of expectation. To investigate the possibility that the effect of EMs in experiments 3 and 4 was influenced by participants comparing their experience between task conditions, we analysed the effect of task condition on the vividness of the first image that the participants recalled. The rationale was that when participants performed the dual-task procedure for the first time – while focussing on the first image – they would have no experience of performing the other tasks and therefore no way of comparing the experience of the task conditions when rating the target image. Figure 17 below show the change in image vividness from baseline to immediately after the dual-task procedure in experiments 3 and 4, where change scores are based on the first image the participants recalled while dual-tasking. The graphs suggest that compared to participants who performed the fixation task first, those who performed the EM task or auditory tasks first experienced a larger decrease in image vividness. Again, these participants had no experience of the other task conditions, meaning the numerically larger decrease in vividness caused by the EM and auditory tasks cannot be explained by expectation alone.

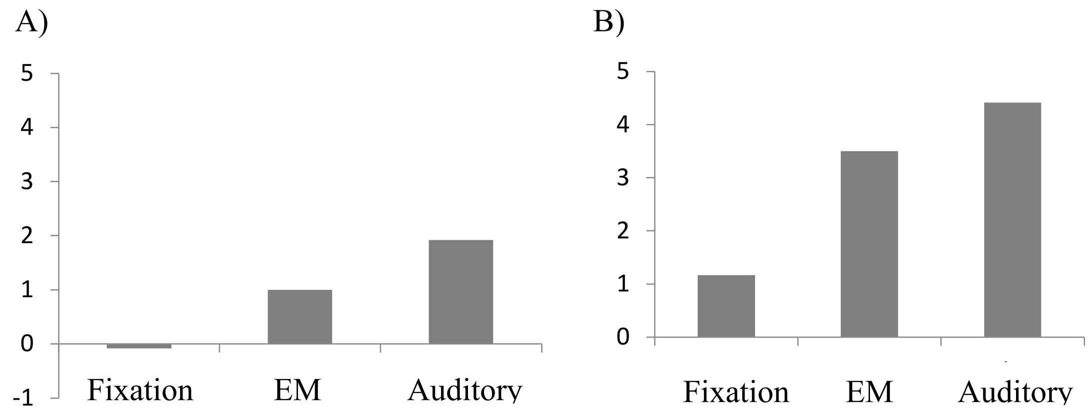

Figure 17: change in vividness from baseline to immediately after dual-tasking for the first image recalled in experiment 3 (A) and experiment 4 (B).

We suspect that the within-subjects design used in experiments 3 and 4 eliminated variation between participants in terms of their interpretation of the vividness and emotionality rating scales, and that this is why we found a clearer effect of EMs compared to fixation in these experiments than in experiments 1b and 2b, which used a between-subjects design. Again, previous studies using a between-subjects design have found a larger effect of EMs on negative imagery compared to fixation (Smeets et al., 2012; van Veen et al., 2016). As I discussed in chapter 3, these studies used repeated measurement of the image and a large number of dual-task trials. Given our observation that increasing the frequency of image ratings may enhance the effect of dual-tasking, we replicated the procedure of experiment 4 but used a between-subjects design. The results of this experiment are discussed in the following chapter.

7 Chapter 7: Comparing the impact of auditory and eye movement interference on repeated measurements of recollection using a between-subjects design

Experiments 1-4 offer mixed findings regarding the effect that taxing WM has on image vividness and emotionality. This lack of consistency may be partly down to differences between these experiments in terms of study design. Experiments 3 and 4 used a within-subjects design and showed a main effect of dual-tasking, whereas experiments 1b and 2b used a between-subjects design, which has less statistical power, and failed to find an effect of dual-tasking. That the dual-task procedure of experiments 1b and 3 were identical except for the study design further supports the argument that our failure to find an effect of dual-tasking in experiments 1 and 2 was due to the use of a between-subjects design.

The aim of experiment 5 was to test if the results of experiment 4 would be replicated if we repeated the experiment using a between-subjects design. Experiments 3 and 4 suggest the effect of dual-tasking is more apparent when participants repeatedly rate the target image. We reasoned that using the same procedure in experiment 5 would allow us to detect an effect of dual-tasking despite using a between-subjects design.

7.1 Experiment 5: comparing the effect of the interference tasks on distressing imagery

Our predictions were the same as in experiments 1-4: images recalled after concurrent EMs would be less vivid and emotional than images recalled after the auditory and fixation tasks.

7.1.1 Participants, Design and Procedure

Experiment 5 used a between-subjects design. A sample of 123 psychology undergraduates (103 female; modal age was between 18-22 yr) were recruited from

Plymouth University in exchange for course credits or money. Most participants were native English speakers (90%).

The procedure was essentially the same as experiment 4 except participants were randomly allocated to either the fixation, EMs or auditory task condition. Participants generated an image of based on a negative autobiographical memory and completed rating of vividness, emotionality and negative affect. They then held this image in mind while performing six 20 s blocks of dual-tasking, rating the vividness and emotionality of the image after each block. After the dual-task procedure, participants recalled the image and held it in mind for 20 s, then repeated the vividness, emotionality and negative affect ratings. Participants were emailed instructions 24 hr after the experiment to recall the same distressing image and then repeated the vividness, emotionality and negative affect ratings. The instructions also asked participants to estimate how often the image had intruded since the experiment.

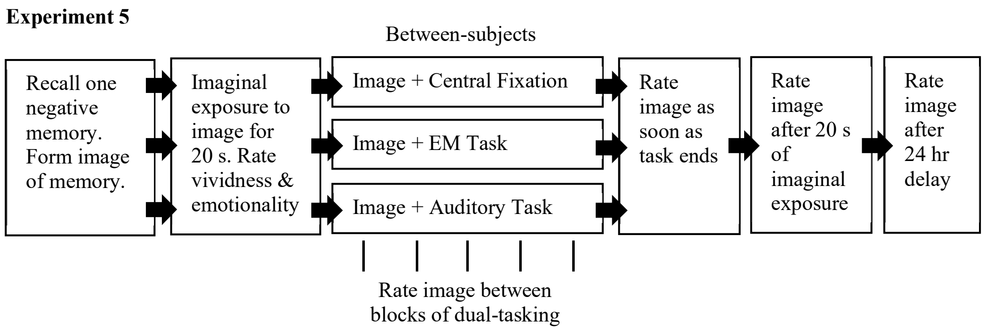

Figure 18: representation of the general procedure used in experiment 5.

7.1.2 Results

Vividness

Figure 19 shows vividness ratings at baseline, across blocks of the dual-task procedure, at post-task following imaginal exposure, and at 24 hr follow-up. We investigated the effect of task condition by comparing vividness at baseline and immediately after the final block of dual-tasking. As in previous experiments, vividness scores were not normally distributed for a variety of reasons, meaning it was not possible to correct all scores for parametric analysis. Due to non-normality, vividness scores were transformed using aligned rank transformation. These transformed scores were then analysed using a 2 (Time: baseline; after dual-task block 6) x 3 (task condition) mixed ANOVA, which showed a significant decrease in vividness over time, $F(1, 120) = 108.094$, $p < .001$, and significant effect of task, $F(2, 120) = 3.377$, $p = .037$, and crucially a significant interaction between time and task, $F(1, 120) = 6.155$, $p = .003$. This interaction was analysed by comparing task conditions in terms of the change in vividness from baseline. A Kruskal-Wallace H test showed the change in vividness varied significantly between task conditions, $H(2) = 8.688$, $p = .013$, $\varepsilon^2 = .071$. Planned comparisons were made using Mann-Whitney U with Bonferroni correction (alpha was .025 for each comparison). Results failed to find the predicted effect of EMs on vividness compared to the fixation task, $U = 697$, $p = .179$. In contrast with our predictions, the auditory task caused a marginally larger decrease in vividness than the EM task, $U = 625$, $p = .044$.

To check if the effects of dual-task condition were consistent over time, we compared the effect of task condition across blocks of the dual-task procedure. As vividness scores were moderately negatively skewed in the fixation condition, squared transformation was used but scores remained non-normal. Because of non-normality, vividness scores were transformed using aligned rank transformation. These transformed scores were analysed using a 6 (time: blocks 1-5) x 3 (task condition) mixed ANOVA. Results showed a significant reduction in vividness over time, $F(5, 600) = 24.915$, $p < .001$, and a significant effect of task condition, $F(2, 120) = 5.639$, $p = .005$. The interaction between task condition and time was not significant, $F(10, 600) = 1.674$, $p = .08$, indicating the effect of task condition on vividness was consistent across blocks of the dual-task procedure.

We tested the effect of task condition after the dual-task procedure, following 20 s of imaginal exposure. As vividness scores were negatively skewed in multiple condition, squared and exponential transformations were performed but failed to produce scores that could be analysed using parametric tests. A Kruskal-Wallace H test showed there was no statistically significant effect of task condition on image vividness following exposure to the image after the dual-task procedure, $H(2) = 1.503$, $p = .472$, $\varepsilon^2 = .01$.

Vividness scores taken 24 hr[4] after the dual-task procedure were normally distributed in all task conditions. A oneway ANOVA showed that there was no significant effect of task condition on image vividness at 24 hr follow-up, $F(2, 103) = 1.599$, $p = .207$, $\eta p^2 = .03$.

Emotionality

Figure 19 shows emotionality ratings at baseline, across blocks of the dual-task procedure, at post-task following imaginal exposure, and at 24 hr follow-up. Consistent with the analysis of vividness, the effect of task condition was tested by comparing emotionality at baseline and immediately after the final block of dual-tasking. Emotionality scores were non-normal for reasons that meant scores could not be transformed for parametric analysis. Emotionality scores were therefore transformed using aligned rank transformation and then analysed using a 2 (time: baseline; after dual-task block 6) x 3 (task condition) mixed ANOVA. This showed a significant decrease in emotionality over time, $F(1, 120) = 114.761$, $p < .001$, a significant effect of task condition, $F(2, 120) = 4.933$, $p = .008$, and a significant interaction between time

[4] Seventeen participants did not respond to the questionnaire sent at 24 hr follow-up. The remaining sample size at 24 hr follow-up was 36 for the fixation condition, 35 for the EM condition, and 35 for the auditory task condition.

and task condition, $F(2, 120) = 7.364$, $p < .001$. This interaction was analysed by comparing task conditions in terms of the change in emotionality from baseline. A Kruskal-Wallace H test showed the change in emotionality varied significantly between task conditions, $H(2) = 10.091$, $p = .006$, $\varepsilon^2 = .083$. Planned comparisons using Mann-Whitney U tests with Bonferroni correction (alpha was .025 for each comparison) showed that the predicted effect of EMs compared to the fixation task was not significant, $U = 820.5$, $p = .852$. In contrast with our predictions, and in line with our findings for vividness, emotionality decreased significantly more in the auditory task condition than in the EM condition, $U = 526.5$, $p = .003$.

To check if the effects of the dual-task were consistent over time, we compared the effect of task on emotionality across blocks of the dual-task procedure. Analysis was performed using non-parametric tests because emotionality ratings were non-normal for different reasons, meaning the data could not be transformed. Aligned rank transformation of the scores was performed and the aligned ranks were analysed using 6 (Time: blocks 1-6 of dual-task) x 3 (task condition) mixed ANOVA. This showed a significant decrease in emotionality over time, $F(5, 600) = 33.159$, $p < .001$, and a significant effect of task condition, $F(2, 120) = 5.751$, $p = .004$. Unlike our findings for vividness, there was a significant interaction between time and task, $F(10, 600) = 2.996$, $p = .001$, indicating the effect of task condition on emotionality varied across blocks of the dual-task procedure. Figure 19 indicates this interaction was due to a steeper decline in emotionality between blocks 1 and 2 in the auditory task condition compared to other conditions. There was otherwise little variation in the effect of task conditions across other blocks of the dual-task procedure.

We tested the effect of task condition after the dual-task procedure, following 20 s of imaginal exposure. Scores in the fixation and EM condition were moderately negatively skewed and continued to vary from normality after using squared transformation. Due to non-normality, the effect of task condition was compared using a Kruskal-Wallace H test, which showed image emotionality did not differ significantly between task conditions following exposure to the image after the dual-task procedure, $H(2) = 1.787$, $p = .409$.

Emotionality scores taken 24 hr after the dual-task procedure were normally distributed in all task conditions. A oneway ANOVA showed that there was no significant effect of task condition on image emotionality at 24 hr follow-up, $F(2, 103) = 3.002$, $p = .054$, $\eta p^2 = .055$. Note however that this effect was marginal. Figure 19 shows that emotionality at follow-up was lower in the EM and auditory task conditions compared to the fixation task condition.

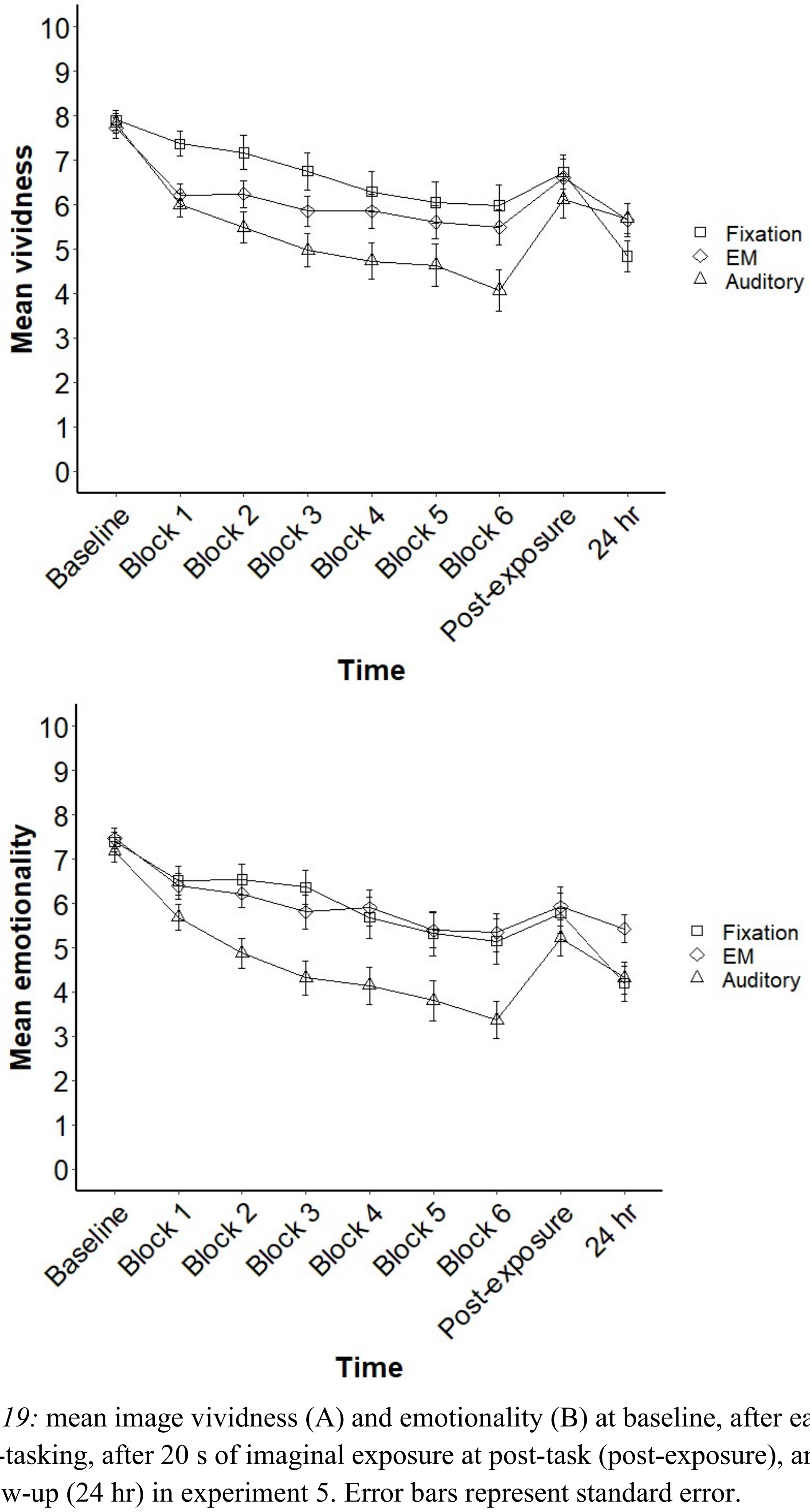

Figure 19: mean image vividness (A) and emotionality (B) at baseline, after each block of dual-tasking, after 20 s of imaginal exposure at post-task (post-exposure), and at 24 hr follow-up (24 hr) in experiment 5. Error bars represent standard error.

Negative affect

As with experiment 4, a programming error meant negative affect scores were not recorded immediately after the final block of the dual-task procedure. Figure 20 shows mean negative affect scores at baseline, at post-task following imaginal exposure, and at 24 hr follow-up. Figure 20 indicates that task conditions were closely matched at baseline, therefore we tested the effect of task condition at post-task and 24 hr follow-up.

Scores taken at post-task after imaginal exposure were positively skewed in all task conditions. After performing logarithmic transformation, the distributions of scores in all task conditions were normally distributed. These transformed scores were analysed using a oneway ANOVA, which showed negative affect scores did not differ significantly between task conditions at post-task, $F(2, 123) = 1.953$, $p = .146$, $\eta p^2 = .032$.

Scores taken at post-task after imaginal exposure were positively skewed in all task conditions. Scores met the assumption for parametric analysis after logarithmic transformation. A oneway ANOVA on these transformed scores failed to find a significant difference between task conditions at 24 hr follow-up.

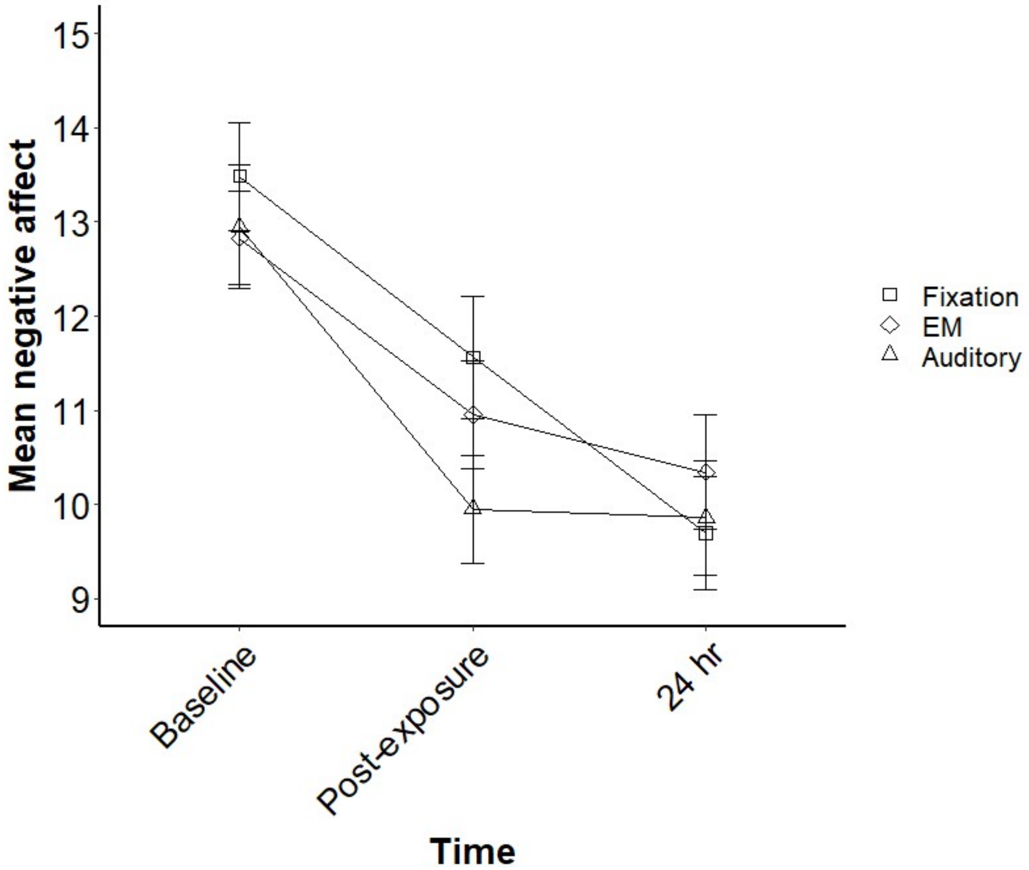

Figure 20: mean ratings of negative affect at baseline, after 20 s of imaginal exposure post-task (post-exposure), and at 24 hr follow-up (24 hr) in experiment 5. Error bars represent standard error.

Task difficulty, pleasantness, difficulty of image retrieval, and image intrusiveness

Ratings of task difficulty were non-normal for reasons that meant data could not be transformed for parametric analysis. A Kruskal Wallace H test showed that task conditions did not differ significantly in terms of self-rated difficulty, $H(2) = 1.495$, $p = .474$, $\varepsilon^2 = .012$.

Task pleasantness scores were approximately normally distributed. A oneway ANOVA showed that task conditions did not differ significantly in terms of self-rated pleasantness, $F(2) = 1.495$, $p = .474$, $\eta p^2 = .012$.

The difficulty of retrieving the image if it dropped from awareness during the task condition could not be transformed for parametric analysis because scores were non-normal for different reasons. A Kruskal Wallace H test showed that task conditions did not differ significantly in terms image retrieval difficulty, $H(2) = 1.013$, $p = .603$, $\varepsilon^2 = .008$.

Task conditions were compared in terms of the total number of times the target image had intruded into awareness in the 24 hr following the dual-task procedure. Scores were highly positively skewed but transforming the scores did not meet the assumption of normality for parametric tests. Due to non-normality, task conditions were compared using a Kruskal Wallace H test, which failed to find a significant effect of task condition on the intrusiveness of the distressing image, $H(2) = 0.393$, $p = .822$.

Table 7: Mean score for negative affect, task difficulty, task pleasantness, difficulty of image retrieval, and number of intrusions for each task condition.

	Fixation	EM	Auditory
	M (SD)	M (SD)	M (SD)
Task difficulty	3.878 (3.219)	3.659 (2.507)	3.073 (2.796)
Task pleasantness	5.39 (2.645)	4.342 (2.319)	5.171 (2.449)
Retrieval difficulty	4.488 (3.3)	5.073 (2.927)	5.219 (2.868)
No. intrusions	1.917 (1.574)	2.229 (1.957)	2.257 (2.063)

7.1.3 Discussion

The results of experiment 5 replicated the effect of WM interference in experiment 4, using a between-subjects design. We found that compared to the fixation task, performing the auditory task during recall caused a larger decrease in image vividness and emotionality from baseline to immediately after the task. This finding cannot be explained in terms of participants making comparisons between tasks, as they were only exposed to a single task condition. We can be more confident that the effect of the experimental tasks in experiments 3 and 4, which used a within-subjects design, were not inflated by participants comparing the effect of the task to the fixation task. Crucially, we found that the EM task did not cause a larger reduction in image vividness and emotionality than the auditory task, which supports the results of experiments 1-4. Taken together, experiments 1-5 offer reliable evidence that the visuospatial WM interference caused by the EMs task did not contribute to its effects on distressing imagery. Our findings contradict previous studies that claim to provide evidence that EMs reduce the vividness and emotionality of imagery by interfering with the storage of the image in visuospatial WM (Andrade et al., 1997; Lilley et al., 2009). As mentioned in previous chapters, the larger effect of the EM task in these studies may reflect the larger general cognitive load of the task compared to the auditory task, which involved counting upward from one. The simplest explanation for why we failed to replicate the effect of task modality was because the EM and auditory tasks in experiments 1-5 were more closely matched in terms of their general cognitive demands that the tasks used in previous studies. The results of experiments 1-5 are more consistent with a general resource account of EMDR, which states that dual-tasks like EMs interfere with distressing imagery primarily by taxing the central executive (Gunter & Bodner, 2008).

The results of experiment 5 offer less convincing support for a WM hypothesis than experiments 3 and 4 because EMs did not reduce in image vividness and emotionality more than the fixation task. This supports the results of experiment 1b and 2b, which failed to find a consistent effect of EMs on image vividness and emotionality compared to fixation. Our failure to find a reliable effect WM interference in experiments 1b, 2b and 5 may be down to the use of a between-subjects design. The aim of the current experiment was to test if the reliable effect of taxing WM in experiments 3 and 4 could be replicated using a between-subjects design. We can conclude that the general cognitive load of our experimental tasks had a more reliable effect on imagery when tasks were performed within-subjects (4 and 5) compared to between-subjects (1b, 2b and 5). Our observation that the effects of EMs are inconsistent when using a between-subjects design is supported by the existing literature. Of the studies that have used a between-subjects design, some have found an effect of EMs on the vividness and emotionality of negative autobiographical memories (Smeets et al., 2012; van Veen et al., 2016) but not positive autobiographical memories (Keller, Stevens, Lui, Murray, & Yaggie, 2014). Other studies, in which imagery was based on photos of neutral (Van den Hout et al., 2013) and emotional stimuli (Leer, Engelhard, Dibbets, et al., 2013) have found an added effect of EMs on imagery, whereas others using similar types of imagery have failed to replicate this effect (Leer, Engelhard, Dibbets, et al., 2013; Leer et al., 2017). Most of the laboratory studies that have used a within-subjects design, including experiments 3 and 4, have found an added effect of EMs on imagery (Andrade et al., 1997; de Jongh et al., 2013; Gunter & Bodner, 2008; Kavanagh et al., 2001; Kemps & Tiggemann, 2007; Kristjánsdóttir & Lee, 2011; Leer et al., 2014; Lilley et al., 2009; Van den Hout et al., 2014; Van den Hout et al., 2001), and relatively few have found no effect of EMs on image vividness or emotionality (Thomaes et al., 2016; van Schie et al., 2015). We suspect that where EM studies have used a between-subjects design, variation in subjective ratings of vividness and emotionality is increased by individual differences in the interpretation of what constitutes a vivid and emotional image. In contrast, the effect of individual differences is controlled in studies that have used a within-subjects design, which may explain why the differences between the effect of EMs and no-EMs in these studies is more reliable. An obvious way to address the potential limitations of using a between-subjects design in future EM studies would be to include objective measures, such as skin conductance, to corroborate the effects of EMs on image emotionality. Experiments 4 and 5 suggest a different way to increase the sensitivity of the study to the effects of WM interference would be to increase the frequency with which participants rate the target image during the dual-task procedure.

Instructing participants to rate the target between blocks of dual-tasking appeared to make our procedure more sensitive to the effects of taxing WM. The decreases in image vividness and emotionality caused by the auditory task was larger in experiment 5 than in experiment 1b, in which images were rated a baseline and post-task. We similarly found that the effect of our experimental tasks was larger in experiment 4, which used repeated ratings of the image, than in experiment 3. We suspect that repeatedly rating the target image makes it easier for participants to compare the image being rated to the image during the previous retrieval, and that this may enhance the overall effect of dual-tasking on the image.

The original rationale for including additional ratings of imagery during the dual-task procedure was to check if the effect of our task conditions on imagery was consistent over time. Put differently, repeated measurement of the image allowed us to check if the effect of task condition at post-task was representative of the change to the image during the dual-task procedure. In experiment 5, the effect of EMs was not consistent across blocks of the dual-task procedure. Figure 19 shows that EMs caused a larger decrease in image vividness compared to the fixation task after the initial block of dual-tasking, but the difference between the EM and fixation task was smaller across subsequent blocks of the dual-task procedure. Our findings are consistent with previous research using repeated measurement of imagery, which shows that EMs cause a large immediate decrease in image vividness that quickly plateaus as further blocks of dual-tasking are performed (Smeets et al., 2012). Smeets et al. explain that EMs and other cognitively demanding tasks may prevent the image from being actively refreshed in WM, which involves continuously switching attention to the image. When this refreshing process is disrupted by dual-tasking, it results in a sudden decrease in vividness. This explanation fits with our finding in experiments 4 and 5 that the EM and auditory tasks caused a large immediate decrease in vividness. However, it is unclear why the WM load of the auditory task continued to reduce image vividness and emotionality across blocks of dual-tasking, whereas the WM load of the EM task had a relatively small effect on ratings taken after the initial block. A possible explanation is that participants adapted to EM task more easily than the auditory task. That is to say the attentional load of the auditory task may have been sustained for longer than the EM task, perhaps because attention was captured more easily by sounds played though headphones than letters presented on the screen. This of course does not explain the results of experiment 4, which found that the EM and auditory tasks continued to have a larger effect on imagery throughout the dual-task procedure compared to the fixation task. Again, the only difference between experiments 4 and 5 was the study design. If participants adapted to the EM task more easily than the auditory task, it is unclear why this would have only happened when tasks were performed between-subjects but not within-subjects. An alternative explanation for the discrepancy between the effect of EMs, compared to fixation, in experiments 4 and 5 is not apparent at this time.

We found further evidence that the effects of taxing WM are short-lived and do not persist beyond the moments immediately after the dual-task procedure. Engaging in a brief period of exposure to the image at the end of the dual-task procedure eliminated the effect of the auditory task on image vividness and emotionality. This was due to a steeper increase in ratings in the auditory task condition than the other conditions. We also observed this sudden recovery in vividness and emotionality in experiment 4 and a tentative explanation was offered in the previous chapter. Briefly, the difference between the images recalled during imaginal exposure and during the auditory may have been more apparent than in the other task conditions. That is to say participants may have been rating the degree of difference between the current and previous version of the image, rather than rating the extent to which created a sense of reliving the recalled episode in the present moment. Previous studies have found an effect of WM interference on imagery even after subsequent exposure to the image (Leer et al., 2014; Van den Hout et al., 2001). As I explained in the previous chapter, participants in those studies did not rate the image immediately after the task, therefore they may have been less likely to think about and notice that the image held in mind was more vivid and emotional than during the dual-task procedure.

We also failed to find an effect of taxing WM on images recalled 24 hr after the dual-task procedure, which is consistent with the results of experiments 2-4. Previous studies have found that the immediate effects of taxing WM during negative recall are present at least 24 hr after the memory is reconsolidated (Gunter & Bodner, 2008; Leer et al., 2014; Schubert et al., 2011). However, one of these studies used the full EMDR procedure, which may have a more robust effect than our dual-task procedure, while the other studies invited participants back to the laboratory, which helps to reduce the effect of non-specific factors. The WM model does not itself predict a lasting effect of taxing WM on negative memories, however research on memory reconsolidation suggests that memories weaken by dual-tasking should be reconsolidated in a less vivid and emotional form, meaning future recollections should be similar less vivid and emotional. As we found the immediate effect of taxing WM on imagery was erased by subsequent imaginal exposure, the absence of dual-task effects at follow-up would be predicted assuming participants were retrieving the same memory trace. Perhaps a more important question is whether our experiment would have been able to elicit reconsolidation of dual-task effects, given that specific conditions need to be met for reconsolidation to occur.

Reviewed in greater detail elsewhere (Treanor et al., 2017), research suggests memories may only become labile within the first six hours after reactivation (Nader et al., 2000) and when recall occurs in the original encoding context (Hupbach et al., 2008), while older and stronger memories may be more resistant to updating (Eisenberg

& Dudai, 2004; Frankland et al., 2006) unless longer is spent reactivating the memory (Elsey & Kindt, 2017; Suzuki et al., 2004). There are also requirements that should be followed when designing studies to ensure the lasting effects of memory updating are due to reconsolidation (Elsey et al., 2018), such as ensuring updating only occurs when the memory is reactivated in conjunction with the experimental intervention, but not when these occur separately. We would argue that no study on the WM hypothesis of EMDR, including our own, address all of these boundary conditions. In experiments 1-5, we did not attempt to measure the age or strength of the memories on which the target image was based, meaning we cannot determine if these memories would have been susceptible to updating from dual-tasking. Furthermore, it is unclear from the literature what would have constituted a sufficient period of reactivation to ensure the memories participants recalled became labile, and therefore whether the brief period of exposure to the image at baseline was sufficient for these memories to become labile. While the dual-task procedure in experiments 2-5 was administered while the memory would have been in a labile state, and follow-up ratings were taken after a sufficiently long-delay, we did not include an additional control procedure in order to confirm that the effect of dual-tasking on imagery depended on concurrent activation of the memory. That is, we did not test if there was an interaction between dual-tasking and memory recall. Only one study on the mechanism of EMDR has tested this interaction (van Veen et al., 2016), however the effects of dual-tasking were not measured outside of the reconsolidation window, as testing the long-term effects of dual-tasking was not one of the study aims. Perhaps of greatest concern for studies in which the aim is to establish if dual-task effects are reconsolidated is the possible requirement for memories to be reactivated in the same encoding context. This makes it virtually impossible to demonstrate a lasting effect of dual-tasking on negative memories due to reconsolidation, as it is not feasible to conduct the procedure in the same spatial content as the original distress episode. It is possible that future research will suggest this boundary condition is not applicable to the types of negative autobiographical memories targeted in EM studies, since the evidence for context-specific updating is based on newly acquired memories within laboratory conditions. Alternatively, it may become apparent that certain conditions permit updating when reactivation of the memory occurs in a different context to the original negative event. As for the studies that arguably provide strongest evidence that the immediate effect of taxing WM on imagery are maintained at least a day after the intervention (Gunter & Bodner, 2008; Leer et al., 2014), a strict interpretation of the controls required to demonstrate reconsolidation would mean these studies do not prove the lasting effect of dual-taking was due to reconsolidation. This is because it was not demonstrated that the effects at follow-up were not present when the competing task was performed without memory recall (Elsey et al., 2018). It is worth noting that an equally strong body of evidence shows that the effects of WM interference on negative memories is short-lived and may not persist once the memory has been reconsolidated (Littel, Kenemans, et al., 2017; van Veen et al., 2019).

Taken together, the findings from experiments 1-5 suggest that dual-task effects on negative imagery are more likely attributable to the general or central executive WM load of the tasks, independent of the modality-specific WM load of the task. Furthermore, our experiments suggest the effect of taxing the central executive is fairly small, and requires repeated recall-and-rating blocks, ideally within-participants designs, for robust results. Nonetheless, reducing the vividness and emotionality of trauma recollections with concurrent tasks may be enough to generate therapeutic effects in EMDR. Experiment 6 proposes a mechanism by which dual-tasks may affect the therapeutic benefits of EMDR, and demonstrates a novel experimental method by which such effects can be studied in the laboratory.

8 Chapter 8: Investigating the possible effect of eye movement interference on the mind wandering component of EMDR

The results of Experiments 1-5 contradict Andrade et al. (1997) WM account of EMDR. We consistently found that performing EMs during the retrieval of a distressing visual mental image did not reduce the vividness and emotionality of the image more than performing a closely matched auditory task. If EMs reduce the vividness and emotionality of an image by interfering with its storage in visuospatial WM, the effect of EMs on image vividness and emotionality should have been larger than the effect of concurrent auditory interference, as the latter does not place demands on visuospatial WM. We believe these experiments offer more compelling evidence than many of the previous studies on the importance of modality-specific interference in EMDR. Firstly, participants in the current experiments formed distressing images that contained only visual information, which meant the resulting image should have been vulnerable to interfere from the concurrent EM task. Furthermore, we used objective measures of STM to confirm that the EM and auditory tasks selectively interfered with visuospatial and verbal STM respectively, meaning we can be confident the EM task would have been able to interfere with the image by loading on visuospatial WM. Crucially, the EM and auditory tasks in the current experiment were designed to place similar demands on the central executive. As I mentioned in chapter 1, some EM studies that claim to show an effect of taxing visuospatial WM have not adequately controlled for the general executive load, meaning the results may only reflect the impact of taxing the central executive. Where we observed an added effect of EMs compared to fixation, this was not greater than the effect of the auditory task, and there was some evidence that the effect of EMs was smaller than the effect of the auditory task. That is, our findings offer consistent evidence that EMs do not interfere with imagery as would be predicted if EMs derive part of their therapeutic effect from modality-specific interference. If EMs do not derive their effects by taxing visuospatial WM resources, it is possible the effect of EMs on the vividness and emotionality of imagery in EMDR is primarily caused by the demands placed on the central executive (Gunter & Bodner, 2008). I have mentioned in passing that designing dual-attention tasks that load heavily on the central executive might interfere with other important processes that occur in EMDR.

In this chapter, I consider how tasks that tax the central executive may affect the reprocessing of trauma memories in EMDR, where connections are formed between the trauma memory and information in existing memory networks. I first establish that the effects of concurrent WM load on emotional imagery may enhance associative memory processing once the dual-task is removed; but may have the opposite effect during dual-task performance. These predictions are then tested using a novel and clinically relevant method for studying memory reprocessing in laboratory settings. While preliminary results were inconclusive, the method described presents a timely development in the way we investigate how WM interference contributes to the therapeutic benefits in EMDR.

8.1 Background to experiment 6

8.1.1 EMDR facilitates reprocessing of trauma memories

It is assumed that the WM interference created by EMs in EMDR is therapeutic because it reduces the emotional intensity of recalling distressing events. While clients undoubtedly benefit from becoming desensitised to distressing memories, an early realisation was that EMDR also restructures how such memories are stored, which makes them less likely to cause disturbance in the future. This insight was behind the decision to rename Eye Movement Desensitisation as a way of acknowledging that its procedures simultaneously desensitise and reprocess distressing memories (Shapiro, 1991). Shapiro's Adaptive Information Processing (AIP) theory (Shapiro, 2001) subsequently emphasised the importance of EMDR's memory reprocessing effect, suggesting "the purpose of the entire eight-phase EMDR treatment is to facilitate accelerated information processing" (p. 89), to which EMs and other dual-attention stimuli are said to actively contribute. Given this emphasis, it is perhaps equally if not more important to consider how WM interference might contribute to the reprocessing of distressing memories in EMDR.

According to AIP theory, the process by which the brain normally encodes and stores episodes in memory is disrupted when a person experiences extreme distress. As a result, memories of particularly distressing events are stored in a similar form to when they were encoded, and differ from normal autobiographical memories in that they are disconnected from other knowledge and experiences. The consequence of this dysfunctional storage is that such memories appear and feel similar to the original event when recalled, and are resistant to change in spite of new adaptive learning. A similar view of memory pathology has been used to explain the aetiology and maintenance of PTSD (Brewin et al., 2010; Ehlers & Clark, 2000), where the vivid and fragmented storage of trauma memories is thought to increase their likelihood of occurring intrusively and as flashbacks. Both AIP and contemporary theories of PTSD suggest that EMDR, including its dual-task component, corrects dysfunctional storage of distressing memories by integrating them within a network of adaptive memories and knowledge.

It is assumed that the reprocessing of distressing/trauma memories in EMDR occurs when clients experience new recollections, images, and insights during and/or between sets of dual-task stimulation (Shapiro, 2001, 2018). Analogue studies on the mechanisms of EMDR usually instruct participants to keep the same memory in mind while performing a concurrent task. This is necessary to minimise the effect of non-specific factors. In contrast, EMDR therapists encourage clients to simply notice what happens to their thoughts, emotions and body while dual-tasking. Furthermore, in between sets of EMs, clients are instructed to simply observe whatever images, thoughts, emotions or sensations enter their awareness; usually they are then told to keep these new associations in mind during the next set of EMs. It is thought the non-directive approach used in EMDR allows clients to form connections between the original target memory and the new associations that spontaneously arise during and between sets of dual-tasking.

Clinical observations suggest that on approximately 40% of occasions, a client will report a change in what comes to mind from one set of EMs to the next (Shapiro, 2001). Common examples reported by clients are that one or more aspect of the trauma memory (image, thought, sensations, emotion) has become less salient. Shapiro refers to such changes in components of the original trauma memory as “single-memory processing effects” (p. 83). Additionally, and perhaps more crucially, clients also report what Shapiro refers to as “multi-memory associative processing” (p. 80), where entirely new memories and their associated components are experienced instead of the initial trauma memory. AIP theory suggests that successful reprocessing requires integrating the trauma memory with the new associations that are generated through EMDR. Shapiro states that both single- and multi-memory processing effects provide evidence the trauma memory is shifting toward an adaptive state. It may be the case that reducing the vividness of the trauma memory is important not just for emotional desensitisation, but also because a less vivid memory competes less with recall of other memories – concurrent WM interference during recall may make the trauma memory less attention grabbing, allowing variants and unrelated memories to be recalled alongside or instead of this memory once the concurrent WM load is removed.

8.1.2 How do EMs facilitate trauma reprocessing in EMDR?

Several potential mechanisms have been identified to explain how EMs may facilitate the associative memory processing effects that occur during EMDR. For example, it has been suggested that EMs increase the flexibility of attention by activating the orienting response – a reflexive shift in attention to potentially important information. This orienting response causes attention to be directed more readily toward new and unexpected information, as supported by evidence that EMs facilitate shifts in attention to cues presented in unexpected locations, and make unexpected sentence endings less surprising (Kuiken, Bears, Miall, & Smith, 2001). Another theory is that EMs facilitate the recall of autobiographical memories by promoting connections between and within cortices (Propper & Christman, 2008). This theory is supported by evidence that EMs improve the accuracy of recall for recently presented stimuli (e.g. Christman, Garvey, Propper, & Phaneuf, 2003, experiment 1) and autobiographical memories (Christman et al., 2003, experiment 2; Christman, Propper, & Brown, 2006). Various theories proposed and cite evidence that EMs induce neurobiological changes in areas associated with episodic memory to explain how EMs promote associative memory processing in EMDR (see Bergmann, 2010, for a review of theories and evidence). Proponents of such theories have argued that the WM model does not account for the multi-memory processing effects observed in EMDR (Leeds & Korn, 2012). While we would agree that no explanation currently exists, I will argue below that a combination of recent evidence from research on the WM load of imagery, and the literature on the role of WM in mind wandering, suggest a plausible mechanism by which taxing WM memory may facilitate memory reprocessing in EMDR.

It is possible that performing EMs aids the retrieval of trauma-related information during EMDR because the reduction in image vividness and emotionality reduces the grip of the trauma image on executive WM resources. According to this argument, the effects on imagery that are often observed under dual-task conditions may reduce the demands the image places on central executive resources, which in turn frees up resources for the retrieval of less salient trauma-related material once the secondary task has stopped. This view is consistent with the AIP account of EMDR, which suggests that dual-attention stimuli shift attention away from salient aspects of the trauma memory toward information stored in adaptive memory networks, allowing new connections to be forged (Shapiro, 2001). We hypothesise a cognitive mechanism by which this happens. Our hypothesis is that WM, specifically the central executive, plays an important role in the process of shifting attention to new information in EMDR. As such, we can use existing research on the WM model to make predictions about how the process of forming new associations in EMDR will be affected depending on the availability of executive resources. In the following paragraphs, I will present evidence that making EMs while holding a distressing image in mind reduces the cognitive load of the image when it is next retrieved into WM. I will then argue that the multi-memory processing effects that drive reprocessing in EMDR are similar to the process of mind wandering, which relies on central executive resources. Based on these two lines of evidence, I will then outline predictions based on our hypothesis about the effect of EMs on mind wandering in EMDR. Lastly, I will discuss how these predictions were tested using a clinically-relevant paradigm that is novel to the EMDR mechanism literature.

Research has demonstrated that mental images capture limited WM resources, particularly if the image is vivid and emotional. It is well established that the vividness of a mental image depends on the availability of WM resources. I have summarised supporting evidence in chapter 1 that shows that the vividness of imagery decreases when the WM resources needed to form the image are depleted (e.g. Baddeley and Andrade (2000). We can infer from this evidence that highly vivid images place greater demands on WM than images that are less vivid. Recent research also shows that images that are highly emotional place greater demands on WM than images that are neutral (Van den Hout et al., 2014). In this study, participants performed a RT task on its own, during recall of a neutral memory, or during recall of an emotional memory. The extent to which RT was slowed by concurrent recall compared to performing the RT task alone served as an objective measure of the WM load imposed by the memory. These authors found that recalling a neutral memory slowed RTs – and therefore taxed WM – and recalling a negative memory slowed RTs more than neutral memories, i.e. recall placed greater demands on WM when the memory was distressing. These findings were supported in a subsequent study (van Veen et al., 2016). If highly vivid and emotional imagery taxes WM resources, then reducing the image vividness and emotionality should also reduce the WM load of the image. Using the RT paradigm described earlier, van Veen et al. (2016) found evidence that distressing images which had been held in mind during EMs placed fewer demands on WM than images that had not been subject to dual-task interference. Crucially, the images that showed significant reduction in WM load were those that had shown a significant decrease in vividness and emotionality due to the effect of EMs. These studies suggest that, following sets of dual-task interference in EMDR, distressing imagery places fewer demands on WM because it is less vivid and emotional.

In EMDR, clients are asked to let go of whatever they are thinking about at the end of a set of dual-tasking and to just notice whatever comes to mind. Anecdotal accounts from EMDR sessions suggest that clients then report new images or memories which often emerge spontaneously into consciousness (Shapiro, 2001, 2018). This process by which clients spontaneously notice new information is similar to the process of mind-wandering (also referred to in the literature as task-unrelated thoughts, task-unrelated images, and zone outs). Smallwood and Schooler (2006) offer a concise description of mind wandering that resembles the multi-memory processing effects described by Shapiro: mind wandering is characterised by a shift in attention away from the primary task (in EMDR, recollecting the trauma memory) and toward unrelated internal information, such as thoughts, images, memories and feelings. In terms of the underlying cognitive processes, Smallwood and Schooler provide a comprehensive summary of empirical evidence that indicates mind wandering relies on the central executive. For example, studies have found that mind wandering happens less frequently when performing a concurrent task that is novel compared to a task has been practised (Teasdale et al., 1995, experiment 3). This is because practice reduces the central executive demands of the task. Furthermore, mind wandering impairs performance on tasks that involve executive processing, such as random number generation (Teasdale et al., 1995, experiment 4) and tasks that require response inhibition (Smallwood et al., 2004). In summary, there are phenomenological similarities between the experience of mind wandering and the associative processing that occurs during the desensitisation phase of EMDR. Given the evidence that the ability to mind wander depends on the availability of executive WM resources, it follows that the frequency of multi-memory reprocessing effects during EMDR will depend on the availability of executive resources. Since the effectiveness of EMDR is reportedly dependent on the occurrence of mind wandering to new trauma-related information (Shapiro, 2001), it is possible that improving cognitive capacity to mind wander will lead to better treatment outcomes. In contrast, factors that reduce the availability of central executive resources for mind wandering may inhibit the adaptive reprocessing of negative memories in EMDR.

Taken together, evidence from the WM and mind wandering literature suggest that focussing on a vivid and emotional image will result in less mind wandering because both imagery and mind wandering will compete for mutual, limited capacity central executive resources. Our hypothesis suggests that the effects of dual-tasking on image vividness and emotionality should reduce the executive WM load of the image when it is held in WM, which in turn should free up executive resources for mind wandering.

Our hypothesis assumes that the trauma image will sometimes remain in or intrude into WM when the EMDR therapist gives the instruction to mind wander, but that dual-tasking will reduce the extent to which the image takes up executive WM capacity. As I mentioned earlier, anecdotal reports suggest that during the desensitisation phase of EMDR, new associations are generated between sets of EMs approximately 40% of the time. Put differently, it is more common for no new associations to be generated after a set of dual-tasking. Presumably, when no new associations occur, this includes instances where the client continues to retrieve the target image. Persistence of the trauma image after sets of dual-tasking appears to be a common occurrence in EMDR, so much so that Shapiro named the phenomenon blocking and looping, and developed additional protocols for dealing with repeated retrieval of the trauma image and associated images, thoughts and feelings (Shapiro, 2001, 2018). Furthermore, trauma images are often characterised by those who experience them as being intrusive, persistent and difficult to control (Brewin et al., 2010). To frame this experience in terms of cognitive processing, clients sometimes find it difficult to shift their attention away from the trauma image when it is held in WM storage. In the context of EMDR, if the target image intrudes into awareness when the client is asked to let their mind wander, the reduced vividness and emotionality of the image should mean it places fewer demands on executive resources, which can instead be redirected to the retrieval of other related and unrelated images, thoughts and feelings. Our hypothesis is consistent with previous WM accounts of EMDR in that it assumes the decrease in image vividness and emotionality of imagery following concurrent EMs is at least partly caused by competition for mutual executive WM resources. What is novel about our hypothesis is that it provides an explanation for how the effects of dual-tasking on image vividness and emotionality may facilitate the generation of new associations between sets of dual-tasking in EMDR, which is apparently vital for the adaptive processing of trauma memories. Crucially, our hypothesis generates testable predictions about the effect of dual-tasking on mind wandering in the context of EMDR.

As our hypothesis assumes that the effectiveness of mind-wandering in EMDR depends on the availability of central executive WM resources, we can predict the factors that will influence the effectiveness of mind wandering, and thus the efficacy of EMDR. Our hypothesis is consistent with Shapiro's AIP model, in that it predicts that dual-tasking while focussing on a negative image should increase the likelihood of mind wandering once the dual-task is removed, relative to performing no task during recall. Our hypothesis differs from Shapiro's account in that we predict the central executive demands of the task and image will influence the likelihood of mind wandering. First, our hypothesis predicts that images that load heavily on central executive resources, because they are highly vivid and emotional, should impede mind wandering. It follows that reducing the vividness and emotionality, and therefore the central executive load of the image should facilitate mind wandering. The existing WM literature suggests tasks that cause larger reductions in image vividness and emotionality due to their higher central executive demands (Gunter & Bodner, 2008) should increase the likelihood of mind wandering once the dual-task is removed, relative to tasks that interfere little with imagery because they place fewer demands on the central executive. Individual differences in WM capacity, which effectively influence the amount of WM interference caused by the dual-task, are also expected to influence the likelihood of mind wandering during EMDR. Given the evidence that dual-tasks cause a larger reduction in image vividness and emotionality among participants with lower executive WM capacity (Engelhard, van Uijen, et al., 2010; Gunter & Bodner, 2008; Van den Hout et al., 2011b; Van den Hout et al., 2010), our hypothesis predicts a negative correlation between WM capacity and the increase in mind wandering caused by dual-tasking. Crucially, our hypothesis differs from Shapiro's AIP theory in that we would expect the central executive demands of dual-tasks, such as EMs, to suppress rather than facilitate mind wandering while the dual-task is being performed. Shapiro (2001) points out that EMs and other dual-task stimuli increase the occurrence of new associations during dual-task performance. This was based on anecdotal reports from clients during sessions of EMDR. Shapiro's observations clearly contradict our hypothesis, which is based on the WM literature, which predicts that mind-wandering should occur less frequently in EMDR when the client is concurrently performing EMs, given the executive WM load of EMs will leave fewer resources available for mind wandering. Furthermore, the likelihood of mind wandering during dual-task performance should be further reduced when the dual-task creates a higher central executive load, and if dual-task interference is increased because the individual performing the task has a lower central executive WM capacity (Smallwood & Schooler, 2006). Importantly, there have been no attempts to confirm, using controlled experiments, Shapiro's observation that EMs enhance mind wandering during and between sets of dual-tasking in EMDR. To address this gap in the literature, and test our predictions about the effect of dual-tasking on mind wandering, we developed a novel experimental method by which such effects can be studied in the laboratory.

To summarise, our updated WM model of EMDR is consistent with AIP theory and existing WM research, as it predicts dual-tasking during the recall of emotional memories will cause a reduction in the vividness and emotionality of the memory image – or to use Shapiro's terminology (Shapiro, 2001), will cause single-memory processing effects – compared to recall alone. Our hypothesis is novel in that it explains how the dual-tasks used in EMDR facilitate mind-wandering - or multi-memory processing effects, to use Shapiro's terminology. Our model states that the reduction in image vividness and emotionality caused by dual-tasking should reduce the central executive load of the image, which in turn should free up executive WM resources. Our prediction is consistent with AIP theory in that it predicts mind wandering should be more likely once the dual-task is removed; but differs in predicting that the central executive load of the dual-task will reduce, rather than increase the occurrence of mind wandering during task performance.

In order to test our hypothesis, it was necessary to establish a method for investigating if EMs affect mind wandering. This would provide a basis for testing the more nuanced predictions of our WM hypothesis. There are no previous examples of studies that have attempted to measure the sorts of memory processing effects that reportedly occur in EMDR. However, a helpful template is provided by the research on mind wandering I referred to earlier. In the following section, I will summarise existing methods that are used to study mind wandering and consider how these methods could be used to investigate mind wandering in the context of EMDR.

8.1.3 Measuring the effect of EMs on mind wandering in EMDR research

The methods used to measure mind wandering in previous research and their associated limitations have been described in detail elsewhere (Smallwood & Schooler, 2006). To summarise, the most common of these methods fall into two categories. One method, known as thought probing, involves prompting participants – using an audible tone, for example - at various points in a task to report whether their thoughts prior to the probe were related or unrelated to the task. Responses are often provided verbally - participants describe the content of their thoughts – which are then classified by the experimenter (e.g. Smallwood, Obonsawin, & Heim, 2003) using a set criteria (Smallwood, Obonsawin, & Reid, 2002). Alternatively, participants are trained to identify task-unrelated thoughts, and then press a button to indicate if a task unrelated thought had occurred prior to the probe (Giambra, 1995). A second common method is self-report, whereby participants are given a definition of a task-unrelated thoughts and are then monitor their thoughts, recording whether task-unrelated thinking occurred within a certain period of time (Antrobus, 1968). The dependent variable in most mind wandering studies is the number of task related and unrelated thoughts reported during a set period of time, the duration of which varies considerably between different studies.

It would be relatively straightforward to use the probe and self-report methods to study the impact of EMs and other dual-attention stimuli used in EMDR, such as binaural tones. This research could show if mind wandering is affected by the types of dual-attention tasks used in EMDR, and varying in the executive demands of the tasks, for example, could be used to test our hypothesis about the mechanisms by which these tasks effect mind wandering. The issue with simply replicating previous mind wandering studies is that mind wandering in these studies is defined as thoughts that are unrelated to task being performed. In contrast, mind wandering in EMDR should be defined in terms of thoughts that are unrelated to the trauma memory. To investigate mind wandering in EMDR, a novel method is required to detect variations of the target memory (i.e. thoughts related to the negative episode) and novel memories (i.e. thoughts unrelated to the negative episode). Such a method would offer relevant insights into the single and multi-memory processing effects that reportedly occur in EMDR. The remainder of this chapter describes an experiment that used a novel procedure to study mind wandering in a way that is relevant to EMDR, and that was used as a preliminary test of our hypothesis about the effects of dual-tasking on mind wandering. Predictions are also made about the factors that should influence the effect of dual-tasking on mind wandering; while these predictions were not investigated in the current experiment, they are included to illustrate the utility of the procedure for investigating our hypothesis about the mechanism by which dual-tasks facilitate mind wandering in EMDR.

Shapiro refers to two types of memory processing in EMDR: multi-memory processing effects, which are akin to mind wandering; and single-memory processing effects, such as changes in image vividness and emotionality. One way to measure the frequency of mind wandering and other changes to the target image is to ask participants to report whenever they experience such changes occur. As mentioned earlier, this method is referred to as self-report in the mind wandering literature. Prior to the experiment, participants could be given an example of mind wandering (e.g. an image that bears no resemblance to the target image) and then asked to indicate when they experience mind wandering, or other changes to the target image. Probes could be presented at fixed intervals during the experiment, to which participants respond by indicating if they experienced mind wandering, or a change to the target image prior to the probe (Giambra, 1995). An advantage of this self-report method is that mind wandering, or other changes to the target image can be measured with high temporal sensitivity. This reduces the likelihood that participants will forget if they had experienced mind wandering or changes to the target image, which may occur if they are asked to report their experience after a delay. A related point is that participants are likely to report instances of mind wandering because they are monitoring and responding to changes in thought content in real time. An important limitation of using self-report to measure mind wandering is that monitoring and responding to instances of mind wandering may interrupt subsequent mind wandering (Giambra, 1995). Consequently, participants may stop noticing new associations or changes to the target image because they are distracted by the requirement to report these changes. A more general limitation of the self-report method is that participants may report mind wandering more frequently because they are paying greater attention to whether mind wandering has occurred (Smallwood & Schooler, 2006). This may lead to incorrect conclusions about mind wandering in EMDR. For example, it may appear that mind wandering occurs more frequently during sets of EMs than occurs in therapy, or it may that EMs interfere less with mind wandering than might be expected based on WM theory. Those readers familiar with EMDR will know that it is common practice in the preparation phase of EMDR for therapists to inform clients that they will likely notice new thoughts, images, and emotions arising during and between sets of dual-tasking. In other words, clients are led to expect that new associations will come to mind in EMDR. This does not necessarily make the self-report method appropriate for investigating mind wandering in controlled analogue studies, as the goal of such studies is to minimise the impact of extraneous variables such as the effect of expectation. The thought probe method described earlier is another option for measuring mind wandering in EMDR, which addresses some of the limitations of self-report.

An extension of previous analogue research on EMDR would be to ask participants what they are thinking about after a set of EMs and to record whether what they report is an example of mind wandering, or a change to the target image. These questions are semantically identical to those use to categorise task unrelated thoughts (Smallwood et al., 2003). Additionally, the participant could be asked whether mind wandering and/or changes to the target image occurred during the preceding set of dual-tasking. Thought probing can offer more detail about the content of participant's thoughts than self-report, as the latter is usually restricted to binary judgements about whether or not mind wandering had occurred. Additionally, thought probing is less likely to result in expectation effects, as participants are not asked to monitor when mind wandering occurs. A limitation of thought probing is that by asking participants to describe their thoughts, it is likely that the detail of the responses will vary due to individual differences in the ability to articulate thought content. Consequently, it could be difficult to categorise the responses of participants who struggle to describe their thoughts. A solution to this issue is to have participants judge for themselves whether their thoughts during/after dual-tasking were the same as or different to the target image. Another advantage of thought probing is that it offers greater control over the timing of sampling, meaning it is possible to avoid interfering with mind wandering. Probing thoughts infrequently is likely to increase the chance that participants will forget some instances of mind and the details of any changes to the target image, meaning important information may be missed. Using more frequent probes should therefore reduce the risk of omissions and errors that are due to forgetting. However, probes should not be so frequent that they interrupt mind wandering. Giambra (1995) suggests using one probe every 15-30 seconds to avoid interrupting mind wandering, while also placing a sensible upper limit on the number of associations that can be reported – a cut-off prevents too much variation between participants. Fittingly, the standard EMDR protocol is for clients to perform 24 s sets of EMs, followed by the instruction to let go of the target image and report whatever comes to mind (Shapiro, 2001, 2018). This means thought probes can be used in a way that reflects the procedures used in EMDR. It is also worth noting that most studies on the WM theory of EMDR use 24 s sets of dual-tasking (Van den Hout et al., 2011b), although they differ from EMDR in that participants are not asked to let their mind wander after dual-tasking; participants bring the target image to mind before performing the next set of dual-tasking. This structure is used as an analogue of EMDR, but only allows the effect of EMs on the target image to be measured. Adding a thought probe after each block of EMs should allow testing of our prediction that the effect of EMs on imagery 'releases' WM to allow mind wandering once the dual-task demands are removed.

To summarise, using thought probes after 24 s blocks of recall with concurrent EMs is an appropriate way to assess mind wandering between sets of dual-tasking. Additionally, the same method can be used to investigate if mind wandering and/or changes to the target image have occurred during dual-tasking. This method is less likely to cause expectation effects than self-reported mind wandering, it closely reflects the structure of EMDR, and it can easily be included as part of the usual experimental procedures used to study the mechanisms of EMDR. Crucially, this method can be used to test our hypothesis about the mechanism by which EMs facilitate mind wandering/reprocessing of the trauma memory in EMDR. Again, we hypothesise that mind wandering should be supressed by EMs during concurrent retrieval of the trauma image, while the effect of dual-tasking on the trauma image should enhance mind wandering once the dual-task has ended. We anticipate that future WM studies using thought probes to assess mind wandering will provide further insights into the role WM plays in the reprocessing of trauma memories in EMDR.

The following section describes the general procedures of experiment 6, which we believe is the first attempt to investigate the effects of EMs on the types of mind wandering that occur in EMDR. The aim is to summarise the similarities and differences between the procedures used, and those that are used in EMDR and previous EM studies.

8.2 Overview of experiment 6

8.2.1 Summary of the procedure used in Experiment 6

Experiment 6 was similar in a number of ways to previous EM studies. At the start of each set of EMs, participants were instructed to retrieve the same distressing image, which we refer to as the target image. Participants then held this image in mind for around 24 s while making concurrent EMs. Participants completed several sets of recall with concurrent EMs. Before and after the dual-task procedure, participants rated the vividness and emotionality of the target image. Crucially, the procedure of experiment 6 was dissimilar to previous EM studies because participants were instructed to stop focussing on the target image and to let their mind wander after performing a set of EMs; most analogue studies omit this instruction and simply ask participants to retrieve the target image. This change allowed us to measure the frequency of mind wandering and changes to the target image, and provided a better analogue of EMDR practice.

There remains some notable differences between experiment 6 and the procedures used in EMDR. These differences are summarised below.

Memory and image selection

Selection of the negative memory was based on instructions used by Andrade et al. (1997), which is the standard procedure for generating vivid and emotional memories in laboratory settings. These instructions are necessarily brief and ask the participant to generate a negative image based on a distressing autobiographical event. In contrast, memory selection in EMDR is the result of extensive history taking and questioning by the therapist. Furthermore, the types of memories selected in EMDR are often intrusive trauma memories, nightmares, and flashbacks, whereas the instructions used in experiment 6 are assumed to generate "healthy" or non-traumatic, but still distressing autobiographical memories.

Measurement of changes to imagery only

In EMDR, clients are encouraged in the initial phases of therapy to identify multiple salient aspects of the trauma memory (a verbal thought, image, physical sensations), which are then held in mind simultaneously and monitored for changes during the dual-tasking phase of therapy. Shapiro (2001) describes how each aspect of the trauma memory may change during the EM phase of therapy. Experiment 7 was concerned only with mind wandering in relation to the target image. Participants in Experiment 7 formed an image of a negative memory, held this image in mind while making EMs, and then reported if the target image had changed or entirely new images had come to mind. There were a couple of reasons why we decided to investigate only the image component of the negative memory. First, it was simpler to differentiate if responses to thought probing were an example of entirely new information (i.e. mind wandering), or a change to the target image. This was partly driven by the time constraints for running Experiment 7, as it was not feasible to measure changes to all aspects of the target memory using our thought probing method. Measuring the effect of EMs on imagery is also justified on the grounds that most EMDR research has focussed on negative imagery, including most of the research on the WM hypothesis of EMDR. Furthermore, imagery is the most dominant aspect of trauma memories (Ehlers et al., 2002) and images generate more emotion than thoughts about the same negative event (Holmes & Mathews, 2005). While this method could be adapted to measure mind wandering to thoughts, physical sensations and other memories, researching mental imagery it is arguably equally if not more relevant to understanding the mechanisms behind the effectiveness of EMDR.

Repeated targeting of the same image rather than new associations

In EMDR, if a client notices a new mental image after a set of EMs, they are asked to hold this image in mind during the next set of EMs (Shapiro, 2001, 2018). In contrast, participants in experiment 6 were asked to begin each block of EMs by retrieving the same image of the distressing memory. There were two related reasons for this decision. First, if EMs facilitate mind wandering in EMDR by reducing the vividness, emotionality and thus attentional load of the target image, these effects should be greater when the same image is repeatedly degraded by EMs, versus holding the image in mind on only a few sets of EMs (Leer et al., 2014). Furthermore, without the instruction to bring the target image to mind, the participant's attention may wander to new images that place few demands on attentional resources because they are not vivid or emotional. If this were to happen after the initial set of EMs, our ability to detect an effect of EMs on these images may be diminished compared to target image.

Order of procedures

The order of the procedures in Experiment 6 was slightly different than in EMDR. Participants in Experiment 6 were informed shortly before the dual-task procedure of the possibility that their mind may wander. In EMDR, this information is usually provided in the earlier preparation phase of therapy – long before the dual-task procedure. We decided to use a different order on the grounds that participants might forget information about the dual-task procedure if it was presented early in the experiment. If participants forgot that they were allowed to focus on mental images unrelated to the original target, this could significantly change their approach to the dual-task procedure; they may try to keep the target image in mind, which would reduce the likelihood of mind wandering. By providing key instructions in a timely manner, the procedure reduces the likelihood that our results would be affected by individual differences in understanding of the procedure.

8.2.2 Summary and predictions of experiment 6

While there were potentially important differences between the procedure of experiment 6 and that used in EMDR, we do not feel these differences make our study so different that the methods offer no insight into the effect of EMs on memory reprocessing in EMDR. Again, the main impetus for developing the procedure described in experiment 6 was to more accurately recreate the desensitisation phase of EMDR within a laboratory setting, the compromises that made our procedure different to EMDR were necessary to ensure experimental control. Future replications could bring the remaining procedures more closely in line with other phases of EMDR, but this is unlikely to improve the external validity of the study outcomes.

Shapiro has suggested that dual-attention tasks in EMDR may facilitate the reprocessing of trauma memories by eliciting single and multi-component memory effects. The former involves changes to one or more aspects of the trauma memory, such as a reduction in vividness and emotional intensity, while the latter involves the retrieval of new trauma-related information. Research on mind wandering suggests the process of generating new information, such as new images and thoughts related to the trauma memory, relies on limited central executive resources. Therefore, EMs and other dual-attention tasks that load on the central executive should reduce the number of new associations experienced during sets dual-tasking. In contrast, evidence from the WM mechanism literature suggest new associations should be generated more frequently after a trauma image that has been held in mind during EMs, versus one that has been held in mind without EMs. The reason is twofold: the image that has been held in mind during concurrent EMs will be reconsolidated in a less vivid and emotional state, and therefore is less likely to capture attention during subsequent mind wandering; secondly, if attention does drift to the trauma image, as it is less vivid and emotional following concurrent EMs, it will place fewer demands on executive resources, meaning attention can more easily drift to new information.

We predicted that compared to recall alone, recall with EMs will increase the likelihood of changes to the target image (e.g. reduced vividness and emotionality) during and after dual-tasking. Crucially, we predicted mind wandering will be less likely during the EMs task than during the fixation task, but will be more likely after the EM task than after the fixation task.

8.3 Experiment 6: testing the effect of EMs on mind wandering

8.3.1 Participants

A sample of 64 psychology undergraduates (male = 10, female = 50, mean age = 23 yr) from the University of Plymouth was recruited in exchange for course credits. Four participants were excluded from the final data analysis because their data was incomplete (n = 2) or they failed to comply with instructions (n =2).

8.3.2 Materials and design

Stimuli for the experiment were created in Microsoft PowerPoint (2016) and were displayed on a 23-inch computer monitor (1920x1080 resolution). Participants were randomly assigned to one of two dual-attention task conditions: EMs or fixation. In both task conditions, a solid black circle (1 cm wide) was presented on a light grey background; the participant was asked to focus on this dot while holding in mind the image of a negative autobiographical memory. In the EM condition, the circle first appeared on the right side of the screen and then oscillated from right to left (at a distance of 43 cm) in a smooth horizontal motion, making one cycle per second for 24 s. In the fixation condition the dot was presented and remained stationary in the centre of the screen for 24 s. In total, participants performed twelve 24 s blocks of image retrieval plus EMs/fixation. Each block was separated by a period of mind wandering and measurement of changes to the negative image (see below for full procedure).

8.3.3 Procedure

A detailed description of each phase of the procedure is given in the following sections. The script containing the instructions for the experiment is available in Appendix (B). The instructions for selecting a distressing image were adapted from instructions that have been used frequently by Engelhard and colleagues to generate distressing imagery within analogue EMDR studies (e.g. van Veen et al., 2015). Our script was unique with respect to instructions regarding how participants should perform the dual-task procedure (see sections two and three of the procedure). These instructions were adapted from the standard EMDR protocol, which is described in detail by Shapiro (2001, pp. 91-153). We tried to use similar wording to the EMDR protocol, such as the instructions to let the image go between sets of dual-tasking, and when informing participants how the target image may change throughout the procedure.

Section one: practice trial and image selection

To familiarise the participant with the dual-attention task, they performed a short practice block in which the stimulus was presented for 5 s.

For the memory selection procedure, the participant was given a few minutes to recall one incident that had made them fearful, anxious, or distressed, and that was still unpleasant for them to think about in the here and now. They were then asked to rate how unpleasant it was to recall this memory from zero (not unpleasant at all) to ten (extremely unpleasant). If this rating was less than four, the participant was asked to recall a more unpleasant memory. If they struggled to do so, a list of examples (e.g. being involved in an accident) was provided.

For the image selection procedure, the participant first described their memory of how the negative event unfolded, meanwhile the experimenter listened and gave occasional feedback such as "okay" and "I see". Once they had reached a natural conclusion, the participant was asked to visualise the incident in their mind and form an image of the most unpleasant part of the event. The experimenter used the analogy of playing the memory like a movie, freezing it at the point it was most unpleasant (similar instructions are used to facilitate image selection in EMDR: ten Broeke & de Jongh, 2015; van Veen et al., 2015). Once the participant confirmed they had an image in mind, they were asked to rated its vividness and emotionality from 0 (no image at all; neutral, respectively) to 10 (as vivid and as clear as read life; as bad as if it were happening). These ratings served as a baseline. It was explained to the participant that this particular image of the incident would be referred to as the 'target image' throughout the experiment.

Section two: preparation

The experimenter explained that during the main procedure (section three) the participant should "let whatever happens, happen" and to "just notice what happens, without trying to influence it or judge whether it should be happening or not". They elaborated "the target image may change in appearance, or it may just stay the same. Sometimes new images may come to mind that are related or unrelated to the memory; equally, you may experience only the target image". These instructions were adapted from the preparation phase of EMDR (Shapiro, 2001), and were used in much the same way to permit uninhibited processing of the target. Whereas instructions in EMDR go as far as suggesting that the person will be unable to hold the target image in mind, the instructions for the experiment were deliberately neutral with respect to potential outcomes so as not to influence the participant.

Section three: dual-task procedure

Participants brought the target image to mind and then looked at a dot presented in the middle of the computer screen. They were instructed to keep their head still and to focus on the dot until it disappeared. The experimenter then initiated the task by clicking the computer mouse, at which point the dot either remained in the centre of the screen (central fixation) or moved from right to left (eye movement) for 24 s. The experimenter sat to the side of the participant during the task and occasionally checked the participant's eyes to determine if they were focused on the dot.

At the end of each block of dual-tasking, the participant was asked to let go of whatever they were thinking about at that moment and to allow their mind to go blank. After a short pause (around 5 s), and crucially to replicate EMDR protocol, the experimenter then asked "what image comes to mind now". This indicated to the participant that they should describe in a short sentence the first mental image they experienced. If it was reported that no image had come to mind, the participant was encouraged to take as long as they needed until they noticed an image[5].

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[5] If no mental image was initially reported by the participant after a block of dual-tasking, this response was recorded by the experimenter and the participant was encouraged to take as long as they needed, but to report and then describe the first image they noticed. First, this was why there was no written statement to report no image, since participants reported this verbally. Secondly, our procedure differs slightly from EMDR protocol in that clients are not usually asked to wait until an image comes to mind after the dual-task, rather they are asked to bring the target image to mind if no other information emerges. The decision to change this procedure was to avoid floor effects, as it was anticipated participants may report no new images without giving sufficient time for an image to emerge. This and other differences between the current procedure and EMDR are addressed in the discussion when considering potential limitations of the experiment.

Participants were asked to select from a list of written statements (Appendix C) which statement they felt most accurately described the image they had experienced after the dual-task. The wording of these statements (see below) was based on the Shapiro (2001) description of the single and multi-memory processing effects that often occur in EMDR, but only made reference to the imagery (Shapiro suggests thoughts and physical sensations may also change, in addition to imagery) as this was the focus of the current experiment.

The experimenter asked “which of the following best describes the first image that came to mind *after* the task”, and the participant was asked to “select either A, B, or C from the list of statements below”:

A. I got an image of the negative incident. It was identical in every way to the target image I had in mind at the start of the task.
B. I got an image of the negative incident, but aspects of the image were different to the target image I had in mind at the start of the task (e.g. it is more/less detailed, closer/further away, quieter/louder, it relates to a different part of the incident).
C. I got an image of something other than the negative incident.

Next, changes to the target image during the dual-task procedure were recorded using a second list of statements. These were semantically identical to the first list but were worded slightly differently and contained an additional option for reporting that no images had been experienced during the task. The experimenter asked “which of the following statements best describes your experience *during* the task”. (NB the experimenter clarified this referred to the period while the stationary/moving dot was presented). Participants were told they could “select one or more of the following statements”:

A. I experienced the target image. All aspects of the image stayed the same while it was in my mind.
B. I experienced the target image, but aspects of the image changed while it was in my mind (e.g. it became more/less detailed, closer/further away, quieter/louder, it changed to a different part of the incident).
C. I experienced at least one mental image of something other than the negative incident.
D. I did not experience any mental images.[6]

If participants selected statements that were contradictory (for statements about the image during the task, this would be ‘A’ and ‘B’, or ‘D’ and any other statement), the experimenter highlighted this discrepancy to the participant and then asked them to briefly describe their experience in their own words, before selecting again which statement/s they felt were most appropriate (the procedure did not continue until participants had given non-conflicting responses).

Once responses were recorded, the participant faced the screen (which was blank) and was asked again to bring to retrieve the target image and to indicate by saying “yes” when this image had come to mind. They then re-rated the vividness and emotionality of the image, after which they were told to stop focussing on the image. The experimenter explained that the participant would be asked to repeat the dual-task procedure and reiterated that they should just notice whatever happens without trying to influence or judge their experience. At this point the main procedure (section three) was

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[6] While we did not make predictions about the participants’ responses to statements A or D, these statements were included because one of our aims was to develop a new procedure that could be used to research the sort of memory reprocessing that occurs in EMDR. Statements A and D would indicate the absence of memory reprocessing, which would predict poorer therapeutic outcomes. It therefore seemed pertinent to include these statements so they can be used in future research on EMDR, as it may be of interest to the research question to know if there has been no change to the target image.

repeated. Participants repeated section three of the experiment twelve times without any breaks. The full experimental procedure took approximately 35-45 min.

8.3.4 Results

8.3.4.1 Image classification

Images were classified into three categories using the response statements described earlier. The first category was an image of the distressing episode that was perceptually identical to the target image as it appeared prior to the most recent block of dual-tasking[7]. We refer to this type of image as the ‘target image’. The second category were images that depicted the distressing event but that were not identical to the target image. An example would be a less vivid version of the target image, or an image depicting an earlier or later part of the same negative episode. Hereafter, we refer to images in this category as ‘episode-related images’ or ERIs. The third category were images that depicted something other than the negative episode. While such images could be of anything, an example would be an image depicting part of a different negative episode. We refer to images in this category hereafter as ‘episode-unrelated images’ or EUIs (NB. although we use the term unrelated for simplicity, these images could be related to the distressing episode in terms of their meaning or the emotions they elicit, for example). A fourth category represented occasions where no image was reported. As mentioned earlier, participants were asked to select a category for the period during and after the dual-task. This meant for each time period, each category of image could be

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[7] Responses after each task block were in relation to the target image as it appeared prior to that block, not the image as it appeared at the beginning of the experiment. This was to ensure that if the target image had changed in appearance on the first block, but then did not undergo further changes on subsequent blocks, the results would reflect that the image had only changed once during the experiment. If images had been rated in relation to baseline, a change to the image only after block one could have been rated as a change (relative to baseline) after every task block. This latter criterion would lead to the incorrect conclusion that the image had changed after every task block.

reported a maximum of twelve times, since there were this many dual-task blocks in the experiment.

8.3.4.2 Statistical analysis procedure

We calculated the proportion of experiment blocks on which each type of image was reported. For example, if an EUI was reported after six of the experiment blocks in the EM condition, the proportion of times this type of image occurred after dual-tasking was .50, given that there were 12 blocks in each task condition. The reason for calculating scores as proportions was to allow for comparisons with future studies using a different number of experiment blocks. Since proportion scores do not meet the assumptions for parametric analysis, we converted our data using Arcsine transformation. To test our predictions, we compared task conditions in terms of EUIs and ERIs - we did not analyse scores for the 'no image' and 'target image' categories. Figure 21 provides the mean proportion of blocks on which EUIs and ERIs were reported during and after the EM and fixation tasks.

8.3.4.3 Main analysis: effect of EMs on EUIs

We analysed the effect of task condition on the likelihood of participants experiencing EUIs during and after dual-tasking. We predicted that EUIs would be more likely to occur after the EM task than after the fixation task, whereas EUIs would be more likely to occur during the fixation task than the EM task. A 2 (Task: EM; fixation) x 2 (Time: during; after dual-tasking) mixed ANOVA revealed that EUIs were more likely to occur after dual-tasking than during dual-tasking, $F(1, 58) = 12.742$, $p = .001$, $\eta p^2 = .18$. Note however EUIs still occurred during some blocks of dual-tasking, despite our instruction to focus on the target image during the task. There was no significant main effect of task condition, $F(1, 58) = 0.004$, $p = .949$, $\eta p^2 < .001$, and the crucial interaction between time and task was not significant, $F(1, 58) = 0.527$, $p = .471$,

ηp^2 = .009, which indicates that the EM task did not affect the likelihood that participants would experience EUIs during or after dual-tasking, compared to the fixation task.

8.3.4.4 Main analysis: effect of EMs on ERIs

We analysed the effect of task condition on the likelihood that participants would experience ERIs during and after dual-tasking. We predicted that ERIs would be more likely during and after the EM task compared to the fixation task. A 2 (Task: EM; fixation) x 2 (Time: during; after dual-tasking) mixed ANOVA revealed that ERIs were more likely to occur during dual-tasking than after dual-tasking, $F(1, 58) = 4.442$, $p = .039$, $\eta p^2 = .071$. In contrast to our predictions, the main effect of task condition was not significant, $F(1, 58) = 0.861$, $p = .357$, $\eta p^2 = .015$. There was no significant interaction between task and time, $F(1, 58) = 0.842$, $p = .363$, $\eta p^2 = .014$, which indicates that EMs did not affect the likelihood that participants would experience ERIs during or after dual-tasking, compared to the fixation task.

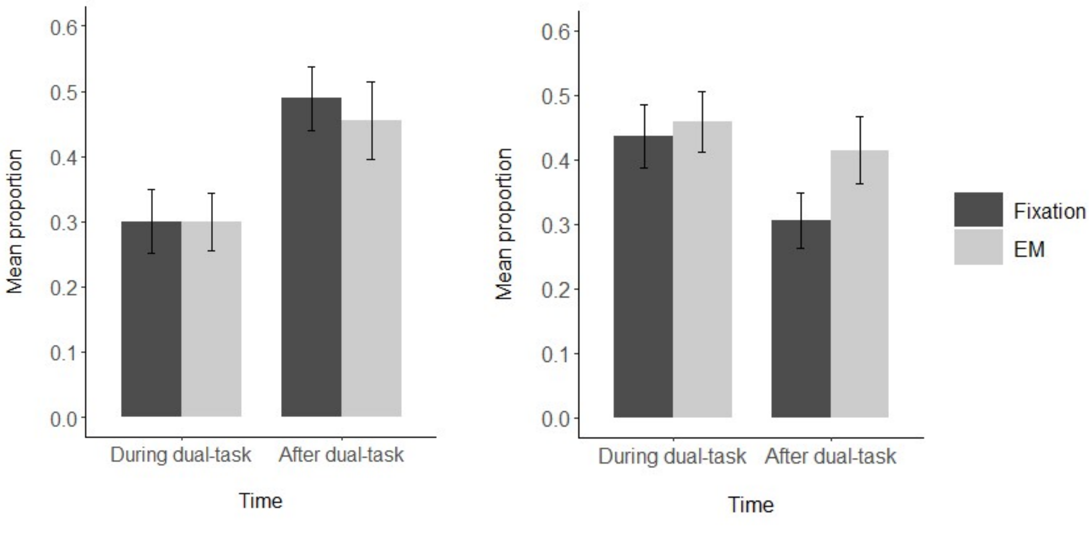


Figure 21: mean proportion of EUIs (A) and ERIs (B) reported during and after the EM and fixation tasks in experiment 6. Errors bars represent standard error.

8.3.4.5 Main analysis: effect of EMs on image vividness and emotionality

Figure 22 shows the mean change in target image vividness and emotionality across blocks of the experiment. Consistent with the analysis of experiments 1-5, we investigated the effect of task condition on image vividness and emotionality by comparing ratings at baseline and immediately after the final block of dual-tasking. Vividness and emotionality scores were not normally distributed for a variety of reasons, meaning it was not possible to transform all scores for parametric analysis. Due to non-normality, vividness and emotionality scores were transformed using aligned rank transformation.

For vividness, a 2 (time: baseline; post-task) x 2 (task condition) mixed ANOVA showed a significant reduction from baseline, $F(1, 58) = 57.692$, $p < .001$, and a significant effect of task condition, $F(1, 58) = 4.706$, $p = .0342$. In contrast with our prediction, the decrease in vividness did not differ significantly between task conditions, $F(1, 58) = 3.017$, $p = .088$; however, results were in the predicted direction – EMs caused a larger mean decrease in vividness than fixation (see Figure 22).

The analysis of emotionality showed no differential effect of task. A 2 (time) x 3 (task condition) mixed ANOVA showed a significant reduction in emotionality from baseline, $F(1, 58) = 113.54$, $p < .001$, and a significant main effect of task condition, $F(1, 58) = 4.388$, $p = .041$, but the decrease in emotionality did not differ significantly between task conditions, $F(1, 58) = 0.001$, $p = .969$.

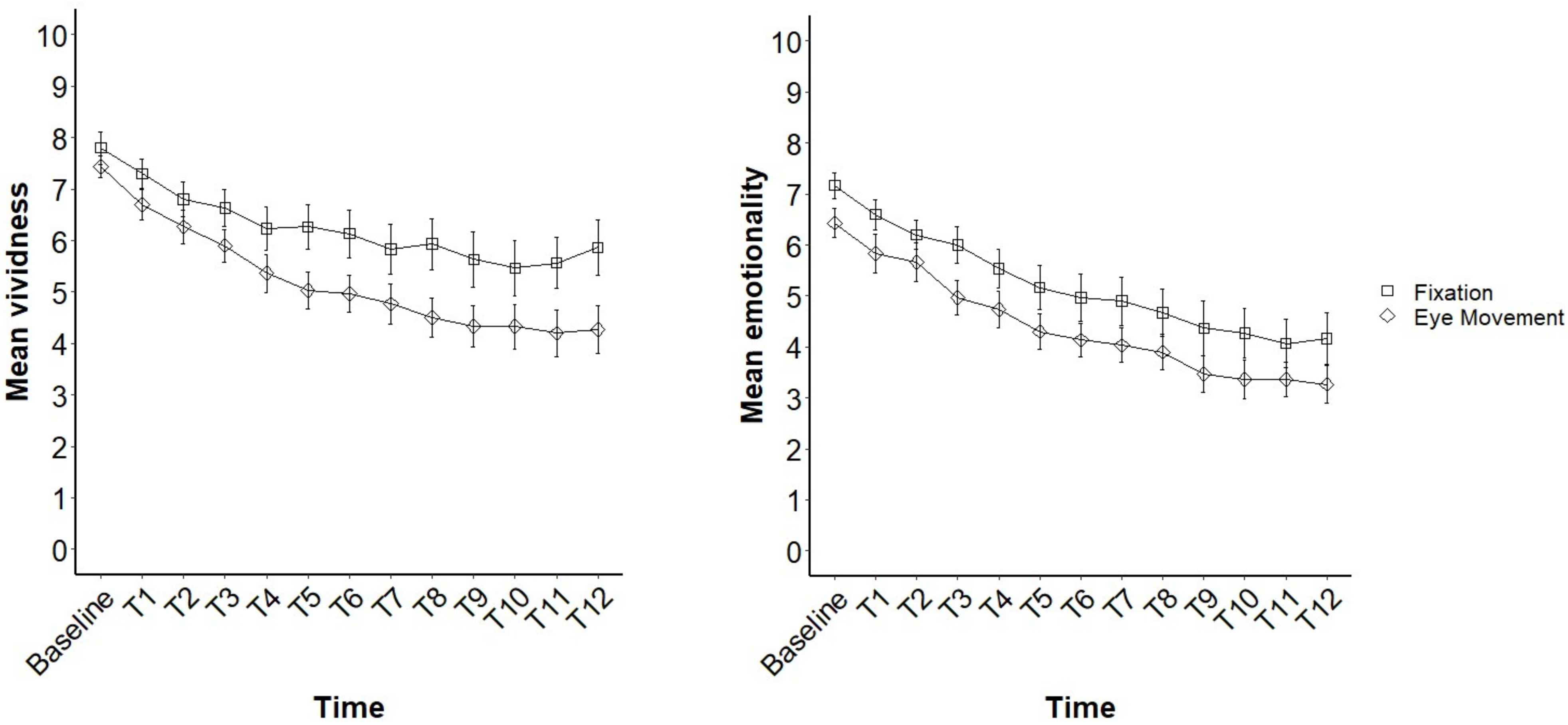


Figure 22: mean change in vividness and emotionality from baseline across blocks of experiment 6. Error bars represent standard error.

8.3.4.6 Exploratory analysis: effect of baseline image emotionality on reprocessing

It is possible that we did not find an effect of EMs on mind wandering after dual-tasking because participants who selected less emotional images found it easier to mind wander. According to our hypothesis, mind wandering is more likely when the target image is less emotional and therefore captures fewer attentional resources. We assume that EMs facilitate mind wandering by reducing the emotionality and therefore the attentional load of the image. We might therefore expect a smaller effect of EMs on mind wandering when images are already less emotional. If the baseline image produces little or no emotion, mind wandering may occur so easily that the added effect of EMs appears negligible. To investigate whether the occurrence of mind wandering after dual-tasking was affected by baseline image emotionality, participants in each condition were split into two groups based on whether their baseline emotionality score was above or below the median for the condition. Figure 23 below summarises the proportion of participants on each task block who reported each type of image.

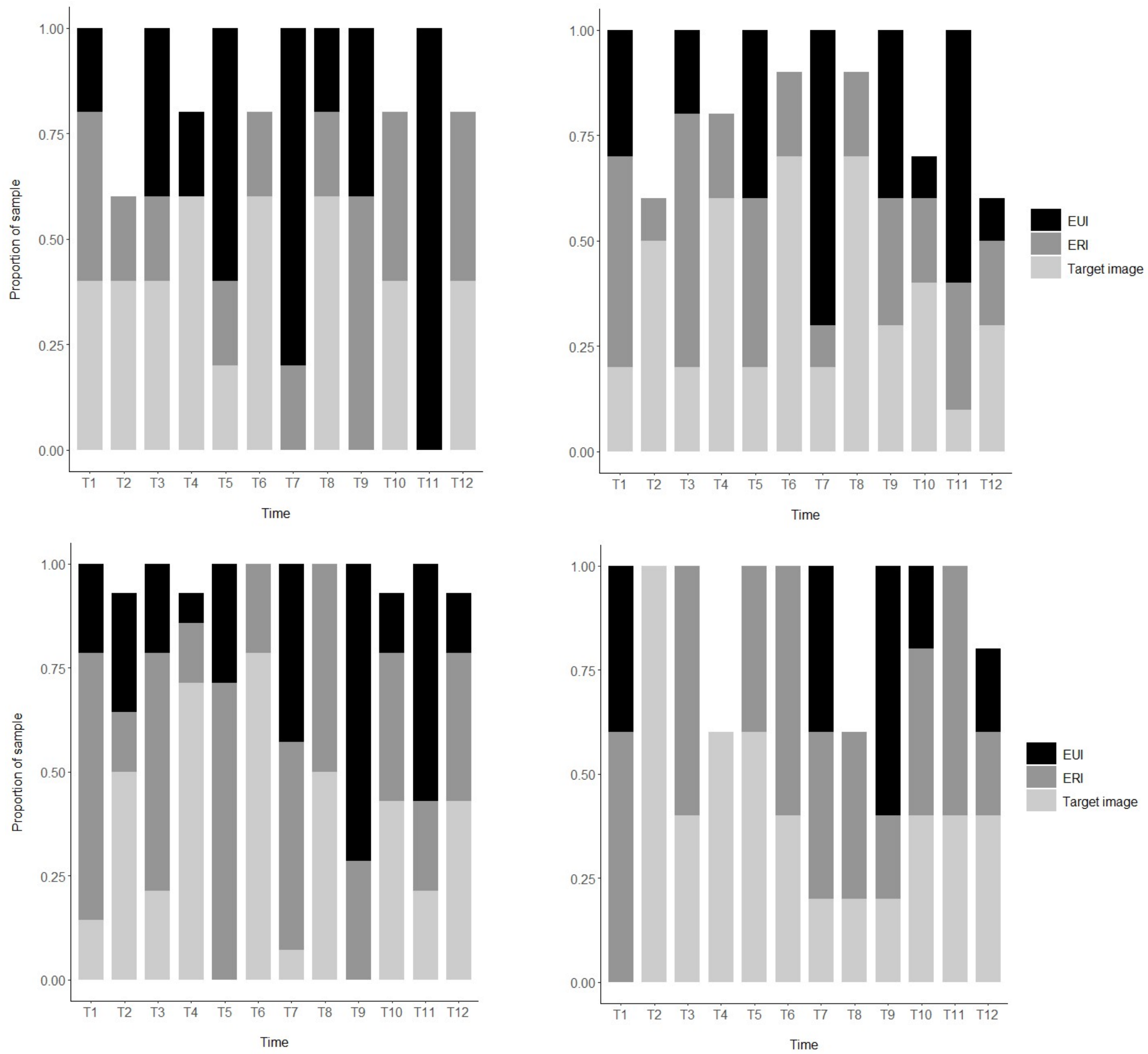


Figure 23: change in the proportion of participants reporting each image category after each block of the dual-task, split by task condition (fixation: top; EM: bottom) and baseline image emotionality (left: low emotion; right: high emotion).

The first thing to note from Figure 23 is that the type of image reported after the dual-task varied over time in both conditions, regardless of baseline image emotionality. This shows that our procedure was sensitive enough to detect when mind wandering (ERUs) and changes to the target image (ERIs) had occurred. As for the effect of image emotionality on mind wandering, when the target image created little emotion at baseline, this did not lead to a ceiling effect - participants did not report mind wandering after every block of dual-tasking. Put differently, holding a less emotional image in mind at baseline did not make it so easy to mind wander that we would have been unable to detect an effect of EMs on mind wandering. Rather, participants who selected less emotional images appeared to be more susceptible to the effect of EMs. There appeared to be an increase in the proportion of EUIs over time in both task conditions, both for high and low emotionality groups. This is consistent with the decrease in image vividness and emotionality over time, in both task conditions, which would have increased capacity for mind wandering.

8.3.5 Discussion

Summary of the aims and findings of experiment 6

It was been suggested that EMs may facilitate memory reprocessing in EMDR by improving the recall of non-traumatic episodic memories (Propper & Christman, 2008), by increasing the flexibility of attention (Kuiken et al., 2001), or by inducing neurobiological changes that help to integrate the trauma memory with adaptive information stored in other memory networks (Bergmann, 2010; Stickgold, 2002). Earlier, I argued that recent evidence regarding the effects of WM loads on negative imagery suggests a different mechanism of action. I proposed that EMs and other dual-attention stimuli used in EMDR may reduce the extent to which the trauma image captures attention, which in turn frees up executive WM resources for mind wandering between sets of dual-tasking, when the client is asked to 'just notice what comes to mind'.

Experiment 6 had two aims. The first was to establish a protocol that could be used to test, in a laboratory setting, if EMs facilitate the types of single and multi-memory processing effects that clients report during EMDR. The second was to test an initial set of predictions made by our WM hypothesis about the effect of EMs on ERIs and EUIs during and between sets of dual-tasking while holding a negative image in mind. Based on evidence that mind wandering requires central executive WM resources, and the finding that EMs reduce the central executive WM load of emotional imagery, we predicted that mind wandering would be more likely after recall with concurrent EMs compared to recall while keeping both eyes stationary. This prediction was consistent with AIP model (Shapiro, 2001). In contrast to AIP theory, we predicted that mind wandering would be less likely during recall with EMs than during concurrent fixation because EMs should reduce the availability of executive WM resources for mind wandering.

Our procedure was similar to previous analogue EMDR studies, in that healthy participants were instructed to recall a negative autobiographical memory, to form a vivid and emotional image of the memory while performing EMs or no EMs, and rated the vividness and emotionality of the image before and after sets of dual-tasking. While previous EM studies have included a period of mind wandering between sets of dual-tasking (Devilly, Spence, & Rapee, 1998), our study was unique in that we measured if the thoughts participants reported were an example of mind wandering or a variant of the target image. Specifically, participants were asked to report whether the first image that came to mind when asked to mind wander was identical to the target image, a variant of the target image (ERI), or a novel image of something other than the negative episode (EUI). Additionally, we also asked participants to indicate if ERIs and EUIs occurred during the preceding dual-task procedure, in order to test our predictions about the effect of dual-task interference on mind wandering. The definitions of ERIs and EUIs were respectively based on the description of single and multi-memory processing effects that reportedly occur in EMDR (Shapiro, 1991, 2001, 2018), and was consistent with the definition of task related and unrelated thoughts often used in research on mind wandering (Smallwood et al., 2002). By basing our EUIs and ERIs categories on the definition of single and multi-memory processing effects in EMDR, respectively, the results of the current experiment shed light on the way reprocessing in therapy may be affected by EMs.

In contrast to our hypothesis, we failed to find an effect of EMs on the likelihood of observing ERIs and EUIs during or after dual-tasking. We also failed to replicate the finding in previous studies that EMs cause a larger decrease in image vividness and emotionality than the fixation task (e.g. Gunter & Bodner, 2008; Lilley et al., 2009; Smeets et al., 2012); however, numerically the results were in the expected direction. Interestingly, we observed a trend whereby the number of participants reporting EUIs between-sets of EMs increased over time, and the vividness and emotionality of the image decreased over time. Although we did not analyse the correlation between changes to the target image and the occurrence of EUIs, the observed trends are consistent with our hypothesis that mind wandering should occur more readily as the vividness and emotionality, and therefore the WM load of the target image decreases. It is possible that had we found a larger effect of EMs on image vividness and emotionality, we may have found clearer evidence for our prediction that ERIs and EUIs would occur more frequently after recall with concurrent EMs, compared to recall with fixation. We tentatively explored the possibility that the reason we failed to find an effect of EMs was because the participants who selected images with lower emotionality found it easy to mind wander. However, inspection of the data did not reveal any trends that would suggest less emotional images led to ceiling effects in terms of mind wandering. That is, image emotionality at baseline was not the reason we failed to find the predicted effect of EMs on mind wandering between sets of dual-tasking. In the following sections I consider reasons why we may have failed to find the predicted effects of EMs on the target imagery and mind wandering.

Our failure to find a statistically significant effect of EMs on image vividness and emotionality compared to fixation is consistent with experiments 1b, 2b, and 5, which also used a between-subjects design. I have argued that the results of experiments 1-5, and the results of previous laboratory EM studies, suggest that the additional impact that EMs have on image vividness and emotionality is less reliable when using a between-subjects design. This is most likely due to the increased variation caused by individual differences in the interpretation of the subjective vividness and emotionality of the image. It is worth noting that EMs caused a numerically larger decrease in image vividness compared to fixation in the current experiment, although this effect was not statistically significant using our current study design. It is possible that had we used a more powerful within-subjects design, we may have found a clearer effect of EMs on image vividness and emotionality. However, it is unclear if increasing the power of the study would have changed our finding regarding the effect of EMs on ERIs and EUIs, as there was little evidence that these measures were effected differently by the EM and fixation tasks. There were limitations in experiment 6 that may explain why we failed to replicate the observation that EMs facilitate ERIs and EUIs in EMDR. These limitations are discussed below and solutions are offered for future research purposes. After addressing the limitations of the study, I discuss the strengths of the methods and describe how they can be used to test our novel hypothesis about the WM mechanisms of EMDR.

Limitations of experiment 6 and potential solutions

As mentioned earlier, there were potentially important difference between current experiment procedure and EMDR therapy. Whereas in EMDR treatment focuses on reprocessing intrusive trauma memories, nightmares and flashbacks, those generated using the current approach were assumed to be normal distressing autobiographical memories. It may be that the memories generated within EMDR respond differently to dual-tasking than those generated using the standard laboratory protocol. That is, the results generated using the procedure of experiment 6 may not help us to understand the effect of EMs on the reprocessing of trauma memories in EMDR. Trauma memories, unlike those based on normal life events, are thought to be disconnected from existing experiences and knowledge (Brewin et al., 2010; Ehlers & Clark, 2000; Shapiro, 2001, 2018). This suggests they are less likely to result in new associations following dual-tasking. Furthermore, it has been reported that trauma memories are richer in sensory detail than memories of non-traumatic life events (Rubin, Feldman, & Beckham, 2004), which may increase their cognitive load and make them even less likely to result in the generation of new memory associations when visualised. This may explain why the addition of EMs is necessary to enhance connectivity of trauma memories in EMDR, while autobiographical memories in the current study already contained sufficient connections to other information for the benefit of EMs to make little difference. Repeating the experiment with people who have experienced trauma, as in previous EM studies (e.g. Lilley et al., 2009) would provide further clarity.

Another notable difference between experiment 6 and EMDR was that participants were asked to return to the target image before performing the next set of dual-tasking. One of the primary aims of the experiment was to test the effect of EMs on memory reprocessing in a way that would closely resemble EMDR. It may seem strange then to only combine the target image with EMs rather than replicating EMDR protocol, in which images that emerge between blocks of EMs become the new target. To recap, if our hypothesis is correct and mind wandering is increased in EMDR due to reductions in the WM load of the image, we decided that instructing participants to focus on the same image on consecutive blocks of EMs would cause a larger reduction in the WM load of the image, therefore increasing our ability to detect an effect of EMs on mind wandering. Furthermore, if participants had been asked to start each block of EMs focussed on the image that came to mind during mind wandering, this may increase variation in the WM load of the image being targeted, making it more difficult to test our hypothesis. Of course the current method does not indicate whether EMs facilitate the kinds of chaining between associations that occurs in EMDR, where the client moves from one image/thought/memory to the next. This could be easily be tested by asking participants to focus on whatever comes to mind at the end of one blocks of dual-tasking during the next block of dual-tasking. The contents of the participant's thoughts during and after sets of dual-tasking could then be measured using the same general procedure described in experiment 6, however instructions about what the participants should focus on during dual-tasking, and the examples of ERIs and EUIs used to categorise the type of memory processing would need to be updated accordingly. Such a procedure would more closely resemble EMDR. As I will explain later in this chapter, updating the procedure of experiment 6 to target thoughts, beliefs and images associated with, or unrelated to a negative/traumatic memory would still allow researchers to test our hypothesis about the contribution of WM interference to the reprocessing of memories in EMDR.

It is possible that our definition of reprocessing in terms of ERIs and EUIs were reductionist and therefore failed to detect an effect of EMs on other types of reprocessing that occur in EMDR. By asking participants to select from a set of predetermined statements, rather than using their own words, the range of potential responses to the dual-task procedure was necessarily restricted. Furthermore, our definition of mind wandering and changes to the negative memory was defined only in terms of changes in imagery, which is just one of the memory components that reportedly change as a result of dual-tasking in EMDR. Participants may have noticed other changes to the negative memory that were not adequately described by the statements provided, thus forcing them to select the next best option. As such, the procedure gives a reductionist view of what might happen to distressing memories when combined with dual-task performance in EMDR. It seemed sensible in the first instance to establish that EMs could facilitate changes to and retrieval of new images, given that the majority of EM studies have demonstrate an effect of EMs on the vividness and emotionality of mental imagery. Furthermore, our hypothesis about the mechanism by which dual-tasking facilitates shifts in attention to new associations in EMDR was based on evidence regarding the effects of EMs on imagery. While our method was sensitive to ERIs and EUIs – we detected variations in these measures between participants and over time - these measures arguably fail to capture other important changes to the distressing memory that occur in EMDR. In the standard protocol for EMDR, in addition to focussing on the mental image of the trauma, clients make EMs while focusing on a maladaptive self-referencing belief (e.g., "I am not lovable"), an opposing adaptive belief (e.g., "I am lovable"), as well as emotions and physical sensations associated with the trauma memory (Shapiro, 2001, p. 430), all of which can transform or be replaced by novel insights, feelings and images. One way to address this limitation would be to provide participants with response statements that could capture changes to other aspects of the negative memory i.e. thoughts, physical sensations and emotions. Participants could be asked to focus only on the image associated with the memory during dual-tasking, in order to provide greater control over the potential mechanism of action. Alternatively, if the primary aim of the experiment is to establish that EMs generate the sorts of memory reprocessing that occurs in EMDR, participants could be asked to focus on different components of the negative memory while dual-tasking (Shapiro, 2001, 2018), which would more closely resemble the protocol used in EMDR. As I mentioned earlier, participants in mind wandering studies are often asked to describe their thought content when probed and then these descriptions are then categorised as related or unrelated to the task by multiple experimenters, and checked for reliability. While participants in experiment 6 were asked to describe what came to mind after the dual-task procedure, these descriptions were not used to categorise ERIs and EUIs and we instead relied on participant's self-report using the response statements. Moreover, participants were asked to only provide a brief description in the interest of time, therefore the qualitative data in experiment 6 lacked detail about the participant's subjective experience. In future research, participants could be asked for more detailed descriptions of their cognitions and emotions when probed, and these descriptions could be categorised by multiple experimenters in terms of whether they represent a change to one or more elements of the negative memory. As I describe below, such a method would overcome the challenges we faced in attempting to create a definition of ERIs and EUIs for participants that would reliably lead to the desired interpretation.

There was some evidence that our measurement of ERIs and EUIs may have lacked internal consistency. Response statements - which participants used to indicate changes to the image – were proof read by several individuals prior to the experiment and feedback was used to adapt the statements so that they were understandable and their intended meaning was apparent. Nevertheless, several participants in experiment 6 selected mutually exclusive statements regarding the images experienced during the task. For example, several participants reported that they had experienced no change to the target image during the preceding block of dual-tasking and also that features of the image had changed (ERI). The experimenter made participants aware when incongruent statements had been selected and these were resolved. However, the fact that some participants gave incongruent responses at all suggests the wording of our response statements may not have been clear enough for all participants to reliably appreciate the differences between the statements. We also found that several participants reported an image of something other than the negative episode (EUI) after the dual-task, but their description of this image suggested it was an earlier or later part of the same negative event (ERI). As our goal was to measure the occurrence of mind wandering, it was important that participants could differentiate between statements in order to report when they had experienced no change to the target image, a variant of the same event image (ERI), or an image of something other than the negative event (EUI) – the latter was used to measure of mind wandering. If the intended meaning of our response statements was not clear, individual differences in the interpretation of these statements could have introduced noise within the data. Significant variation would explain our failure to find the predicted effect of EMs on these measures, and our failure to observe an interaction between the types of image (ERIs or EUI) and when the image occurred (during or after dual-tasking). It seems unlikely that we failed to detect an effect of EMs on ERIs and EUIs due to variation in the comprehension of our response statements. Anecdotally, only a small number of participants selected incongruent statements, or gave descriptions of imagery that seemed incongruent with the image category selected. If the experiment were to be replicated, participants could be trained prior to the experiment to identify examples of ERIs and EUIs, which would help to reduce variability in interpretation. As mentioned earlier, this training method has been advocated for helping participants to distinguish task related and unrelated thoughts in mind wandering research (Smallwood & Schooler, 2006). Alternatively, participants could be asked to provide detailed descriptions of the images experienced during and after the dual-task procedure; these descriptions could be used by multiple experimenters to categorise the type of image and compared to the image category selected by the participant to check reliability. Unfortunately, it was not possible to categorise the participants' descriptions of the image retrieved after the task in order to check if these descriptions matched the image category selected. This was because the experimenter asked for a brief description of the image in order to match the protocol used in EMDR – some descriptions were too brief or too vague to accurately interpret the type of image experienced. Furthermore, participants were not asked to describe the types of image during the dual-task procedure, therefore there was no way to check the reliability of their interpretation of the statements regarding the types of imagery experienced while performing the EM or fixation task.

It could be argued that our definition of ERIs was not valid. The response statement that participants used to report an ERI described several possible changes to the image, including a shift to an earlier or later part of the negative episode. Our rationale for including shifts within the same event as an example of an ERI was that this type of change to the target is defined by Shapiro as a single-memory processing effect. We used the description of single-memory processing effects to define our ERI category. However, it is possible that for the image to change to an earlier/later part of the event, attention needs to be shifted away from the target image. That is, when participants reported an ERI because the image had changed to a different part of the same image, it could be argued that we should have instead categorised this type of image as an example of mind wandering (an EUI), or perhaps a separate image category altogether. Assuming central executive WM resources are required when the target image changes to an earlier or later part of the same episodic memory, we would predict that EMs would reduce the likelihood of these changes in imagery while performing EMs for the same reason mind wandering (EUIs) should be less likely during EMs – EMs will reduce the availability of central executive resources for mind wandering. If our definition of ERIs in experiment 6 was invalid and captured instances of mind wandering, our experiment was not capable differentiating the effect of EMs on mind wandering and changes to the target image. The consequence is that experiment 6 may not have been capable of detecting differential effects of dual-tasking on ERIs and EUIs, which we predicted would be the case during dual-tasking. Our concerns about the validity of our image categories do not extend to the definition of EUIs, which was less equivocal - we can be confident that experiment 6 was capable of detecting an effect of EMs on mind wandering. Put differently, the procedure used in experiment 6 provides a valid means of investigating how EMs contribute to mind wandering EMDR. It may be helpful in future research to separately record changes in sensory quality of the image and images of earlier or later parts of the same negative episodic memory. As mentioned earlier, participants could be asked for more detailed descriptions of the images they experienced, which could be categorised by the experimenter. Alternatively, the response statements pertaining to ERIs used in experiment 6 could be replaced with two separate statements to distinguish images of different points in the same memory and images of the same moment that have changed in perceptual quality. Our prediction remains the same about the effect of EMs on ERIs due to changes in sensory quality of image (e.g. appearing more distant, or changing colour) – EMs should increase ERIs more than fixation given that the WM interference caused by EMs will reduce image vividness. Future research may elucidate whether the ERI response category described in our procedure should be reviewed.

Although not a limitation of our procedure, another possible reason that EMs did not facilitate mind wandering in experiment 6 is that EMs do not facilitate mind wandering in EMDR. Our experiment makes an important contribution to EMDR research, as it is the first attempt to establish that EMs contribute to the kinds of memory reprocessing effects that are thought to move trauma memories into a more adaptive state. Put differently, it has never been established that EMs contribute to the adaptive reprocessing of negative memories in EMDR, more than concurrently focusing on a stationary stimulus. While it is apparently a well-established phenomenon that EMs contribute to mind wandering in EMDR, this has not been confirmed through controlled experiment. Perhaps concurrent fixation during recall is equally effective to EMs in terms of stimulating the adaptive reprocessing of memories. The possibility that the dual focus of attention involved in EMs is sufficient to generate adaptive reprocessing of trauma memories is supported by evidence that the therapeutic outcomes of EMDR do not differ if the client concurrently focuses on a stationary stimulus during recall of the trauma memory, versus performing EMs during recall (Sack et al., 2016). Assuming the therapeutic effects of EMDR are caused by the adaptive reprocessing of the trauma memory with new associations that emerge during dual-tasking, the existing evidence about the active component of EMs suggests that fixation during recall should be sufficient to generate the types of single and multi-memory processing effects that we measured in experiment 6. In other words, perhaps we should have predicted equal effects of the EM and fixation task on EUIs in experiment 6. As I argued in chapter 1, an important gap in the EMDR mechanism literature could be addressed through meta-analysis of existing component studies to determine if the outcomes of therapy differ when the trauma memory is recalled with EMs, with dual-focus of attention, or without dual-focus of attention. Until such research is available, the procedure used in experiment 6 offers a complimentary method for investigating the component/s of EMs and other dual-tasks that facilitate the types of memory reprocessing effects behind the effectiveness of EMDR. Future replications of our experiment could include an additional control task in which the target image is recalled without dual-tasking, such as eye closure or unfocussed looking (for examples of studies using these controls, see Sack et al., 2016). We would expected mind wandering to be enhanced most by EMs, followed by fixation, and least by recall without concurrent demands on executive attention.

Strengths of the procedure used in experiment 6 and its utility for future research

Experiment 6 provides a novel method that can be used in laboratory studies to investigate if EMs and other dual-tasks facilitate memory reprocessing in EMDR. While there were differences between our procedure and the procedure used in EMDR, as I explained earlier, the procedure provides a useful analogy for studying the single and multi-memory processing effects that are considered evidence of reprocessing in

EMDR, which we refer to as ERIs and EUIs, respectively. There are several indicators that our procedure was sensitive to these memory processing effects. First, most participants were able to understand and follow instructions to engage in mind wandering, which were adapted from the standard EMDR protocol for the EM stage of therapy. Second, participants were able to use the response statements to indicate when they had experienced ERIs and EUIs. Furthermore, Figure 23 shows there was variation in the type of image category selected during and after the dual-task was different. This indicates that our procedure had sufficient temporal sensitivity to detect the presence or absence of reprocessing at different points in the procedure, as well as the type of reprocessing. As I highlighted in the predictions for experiment 6, there were theoretical reasons to expect that the effect of EMs on reprocessing would be different during and after dual-tasking. Therefore, the temporal sensitivity of our procedure also permits specific predictions about the timing of memory processing effects to be studied. Perhaps more importantly, the ability to measure the participants' experience during and after the dual-task means it is possible to test if reprocessing follows the same timeline as in EMDR (Shapiro, 2001, 2018). For example, researchers could experimentally test the observation that once mind wandering starts in EMDR, it tends to continue for several blocks of dual-tasking.

Another strength of our novel procedure is that it can be used to test predictions about the mechanisms by which EMs contribute to reprocessing in EMDR. According to our updated WM model of EMDR, we would expect factors that cause larger reductions in the WM load of the target image to enhance the retrieval of ERIs and EUIs. For example, increasing the central executive demands of the dual-task performed during recall may increase mind wandering when the dual-task is removed. Conversely, factors that reduce the impact of dual-tasking on the target image are expected to impede mind wandering and therefore reduce therapeutic outcomes. For example, recalling images that are highly vivid and emotional, having larger executive WM capacity, and engaging in extensive practice of the dual-task – which will reduce its executive WM demands - should all reduce the effect of dual-tasking on the WM load of the image, and therefore reduce the likelihood of ERIs and EUIs between sets of dual-tasking. We would also predict that the occurrence of EUIs will be associated with poorer performance on tasks that require mutual central executive WM resources. The EM and auditory tasks used in experiments 1-5 could be used to test this prediction - the number of valid responses to target letters should be lower during blocks of dual-tasking in which EUIs occurred.

Our procedure could also be used to test if the WM interference caused by EMs facilitates changes to and the retrieval of other memory components targeted in EMDR: thoughts, beliefs, physical sensations. Changes in the validity of these beliefs (i.e., how true the client thinks the belief is at the present moment) are a key indication that the trauma is being reprocessed, and therefore inform the therapist if further sets of dual-tasking is required (Shapiro, 2018). Dual-tasking is also combined with new associations that arise during reprocessing, which can take the form of short verbal statements the client mentally rehearses. Few studies have looked at the effects of dual-tasking on such thoughts; most research has focussed on the imagery component of negative memories. The studies by Baddeley and Andrade (2000) and Kemps and Tiggemann (2007) have shown that it is possible to interfere with the vividness of verbal imagery using tasks that tax the central executive. Other studies have found mixed evidence regarding effects of dual-tasking on the clarity and validity of beliefs. For example, Maxfield et al. (2008) found evidence that performing EMs during the recall of negative memories reduces the clarity of verbal thoughts associated with the memory, although effects may have been influenced by effects of EMs on the memory image, as both the image and thought were held in WM simultaneously. Other studies have found that performing EMs while focussing on a self-referential belief does not affect the perceived validity of the belief, although the beliefs targeted in these studies concerned stable personality traits and therefore may have been resistant to the limited amount of dual-tasking performed (Matthijssen & van den Hout, 2016; Matthijssen & Van den Hout, 2016). These studies indicate that concrete thoughts, about specific autobiographical events, may be more susceptible to disruption by concurrent WM interference. WM theory suggests dual-tasks could reduce the clarity of verbal thoughts/beliefs by disrupting the central executive processes involved in maintaining the contents of verbal WM (Baddeley, 1986). Although we are not aware of any evidence that less clear verbal thoughts reduce placed fewer demands on WM, this would follow if these thoughts are disrupted by reduction in central executive resources by dual-tasking. It follows that dual-tasking while focusing on the thoughts and beliefs associated with emotional memories should reduce the quality of these beliefs and may consequently free up attention resources for retrieving novel thoughts and beliefs, which requires the availability of central executive resources (Smallwood & Schooler, 2006; Teasdale et al., 1995). Future research could investigate if dual-tasking facilitates the adaptive processing of thoughts and beliefs that are targeted during EMDR. Our procedure serves as a useful template for future investigation. The wording of the image selection instructions and response statements could be easily modified to study the effects of dual-tasks on changes to verbal cognitions and the generation of novel verbal cognition. From a WM perspective, the effects of dual-tasking on verbal cognitions should depend on the same factors that influence the occurrence of ERIs and EUIs, such as the central executive demands of the competing task and individual differences in executive functioning. Such evidence would suggest a WM mechanism plays a role in the reprocessing of more than just the image component of the trauma memory in EMDR.

Lastly, the procedure used in experiment 6 can also be used to investigate other theories about how EMs and other dual-attention tasks facilitate memory reprocessing in EMDR. Described earlier and reviewed elsewhere (Bergmann, 2010; Gunter, & Bodner, 2009), these theories can be summarised according to the fundamental mechanism of action, such as enhanced inter-hemispheric communication (Propper & Christman, 2008) and initiation of an orienting response (Kuiken et al., 2001). While the predictions of experiment 6 were based on our theory about the role WM may play in reprocessing trauma memories in EMDR, these predictions were about the standard EM protocol used in EMDR, namely how mind wandering should be effected differently during and after sets of EMs. That is to say we did not manipulate the standard EM procedure in order to test our theory. We would recommend that future mechanism research should attempt to retain the elements of the procedure necessary for establishing if dual-tasking effects mind wandering: the selection of a negative memory; focusing on components of the memory while dual-tasking; the instruction to mind wander; and the measurement of EUIs and ERIs. However, the procedure could be adapted to include continuous monitoring of physiological and neurobiological changes associated with the underlying mechanism of action. For example, if EMs facilitate reprocessing by increasing communication between hemispheres, then experiment blocks on which EUIs and ERIs occur should be correlated with measures that indicate improved hemispheric communication (Bergmann, 2010), and these measures should be affected when dual-tasking is performed during negative recall compared to recall alone. Previous studies demonstrate how measures such as skin conductance (Barrowcliff et al., 2004) and EEG (Keller et al., 2014; Sack, Lempa, Steinmetz, Lamprecht, & Hofmann, 2008) can be incorporated into EM procedures similar to that used in experiment 6. Ideally, multiple theories could be tested within the same experiment in order to establish which offers the strongest explanation for the effects of EMs on ERIs and EUIs.

8.4 Conclusion of chapter 6

Experiment 6 offers a useful method for studying if EMs facilitate the types of single and multi-memory processing effects that reportedly occur in EMDR. These memory processing effects are considered crucial evidence during EMDR that the trauma memory is being connected with adaptive information stored in disparate memory networks. The importance of memory reprocessing is based on Shapiro's AIP theory and is consistent with cognitive models of the causes of PTSD, which state that PTSD symptoms are maintained because the trauma memory is stored differently to normal autobiographical memories, which causes the memory to become resistant to updating with new information and maintains the memory in a state where it becomes easily activated by associated sensory information, creating a vivid sense of reliving the trauma in the present moment. The benefit of EMs and other dual-tasks in EMDR is reportedly to facilitate the generation of variants of and novel associations to the trauma memory, which then become connected with the trauma memory, allowing it to be connected with other semantic and episodic information – the memory becomes stored similarly to healthy autobiographical memories.

Despite the reported importance of EMs for generating new associations during EMDR, there have been no attempts to confirm that EMs increase the generation of new associations to the trauma memory as is suggested by Shapiro's AIP theory. Experiment 6 offers an important controlled test of the claim that EMs facilitate the retrieval of related and novel mental images associated with distressing autobiographical memories. While our novel procedure differs to EMDR in several ways, such as targeting of non-trauma memories and focussing on one component of the distressing memory – the image – our procedure closely resembles key aspects of the EMDR protocol and crucially, provides the necessary experimental control. The procedure described in experiment 6 can easily be adapted address the potential limitations highlighted, and provides a template for testing if EMs facilitate the generation of other memory components, namely thoughts, beliefs, emotions and physical sensations. Given evidence that dual-focus of attention, rather than bilateral stimulation is the active component of EMs in EMDR, our procedure can be used to investigate the active component that drives the effects of EMs and other dual-attention tasks on the single and multi-memory processing effects in EMDR. Such research could be used to supplement much needed meta-analytic research on the effectiveness of EMDR with and without these active components.

Several theories have been proposed to explain the underlying mechanisms by which EMs contribute to the reprocessing of memories in EMDR. Shapiro's AIP account of EMDR proposed that EMs contribute toe memory reprocessing, but was not meant to explain how EMs help to alter and create novel associations to the trauma memory. Previous accounts about the contribution of WM to EMDR have only been used to explain how EMs and other dual-attention tasks cause single memory processing effects, namely reductions in the vividness and emotionality of the trauma image (relatively few studies looking at the effects of dual-tasks on the thought and belief component of the trauma memory). The WM hypothesis has been criticised for not explaining how the WM interference caused by dual-tasking facilitates the connection of the trauma memory with new adaptive information. Perhaps partly because of the perceived limitations of WM theory, other theories have been proposed to explain how EMs facilitate the spontaneous retrieval of new information that occurs during the desensitisation phase of EMDR. I propose an alternative hypothesis, based on WM theory and evidence from research recent EM studies, which offers an explanation for how the WM demands of dual tasks can facilitate the reprocessing of trauma memories in EMDR. Importantly, this explanation not only accounts for the observation that EMs generate novel mental images in EMDR, but can also account for the shifts in attention to novel thoughts, beliefs and physical sensations when clients are asked to let their mind wander between sets of dual-tasking. This novel WM theory brings together associated evidence from previous research on WM hypothesis of EMDR and evidence from research on the role that WM plays in mind wandering. Described earlier, this theory states that the WM interference caused by dual-tasking during negative recall reduces the central executive WM load of the trauma image, which in turn frees up central executive resources for mind wandering to novel information after the dual-task is removed.

Future research, using the procedure described in experiment 6, could investigate the factors that our WM theory predicts will influence the effectiveness of dual-tasking on memory reprocessing, such as the WM load of the concurrent task and individual differences in WM capacity. Specific predictions about how these factors will affect the likelihood of observing single and multi-memory processing effects are in more detail outlined earlier. These predicts are derived from existing research on the effects that dual-tasking has on the vividness, emotionality and WM load of emotional mental imagery. Briefly, our theory assumes that the factors that make dual-tasking more effective for reducing the vividness and emotionality of the target image will increase the likelihood of mind wandering immediately after the dual-task is removed. An advantage of our method is that it will be familiar to researchers who have tested the effects of EMs on image vividness and emotionality, and therefore it can be adopted easily to replicate previous EM studies while also testing predictions about the effects of dual-tasking on mind wandering. It is likely that other researchers will appreciate other potential uses for the method. In addition to the suggestions for future research outlined earlier, it would be interesting to investigate if the reconsolidation of the target image in a less vivid and emotional state effects the likelihood of mind wandering when the image is retrieved outside of the consolidation window. The conditions required to demonstrate reconsolidation have been discussed in earlier chapters. Our theory would predict that mind wandering will continue to occur more readily in the EM condition compared to recall alone when the procedure is repeated after a 24 hr delay. Evidence that the immediate effects of EMs on image vividness and emotionality facilitate mind wandering after reconsolidation would suggest a mechanism by which the benefits of one session of EMDR are carried forward to the next. This and other important predictions mentioned earlier can be investigated through simple adaptations to the procedure described in experiment 6.

9 Chapter 9: General Discussion

9.1 What do the current findings reveal about the WM model of EMDR?

9.1.1 Challenging the modality-specific WM hypothesis.

Debate exists about whether EMs and other dual-tasks reduce the vividness and emotionality of imagery by selectively interfering with ability to hold the image in modality-specific WM stores. More precisely, it has been suggested that the therapeutic effect of EMs in emotional imagery is caused in part because EMs disrupt the ability to store the visual components of the image within visuospatial WM (Andrade et al., 1997). This debate is relevant to EMDR, as clients can perform EMs or listen to binaural tones during recall of the trauma memory, where the latter should selectively interfere with the verbal or auditory aspects of imagery, according to WM theory. Guidance on how dual-attention tasks should be delivered in EMDR makes reference to WM theory and research (Beer et al., 2011; Shapiro, 2018), however I highlighted in chapter 1 the lack of well-controlled evidence regarding the important of task modality in EMDR. Evidence for the hypothesis that the benefits of dual-tasking in EMDR should be increased by matching the modality of the task and image is mixed and several of the studies that claim to offer supporting evidence have important methodological limitations, raising doubts about the validity of the study conclusions. Clarifying the WM systems involved during the dual-task phases of EMDR is important, as an interaction between task and image modality suggests that EMs and binaural tones should be more beneficial when the target image contains primarily visual and auditory information, respectively. Furthermore, if the subsystems of WM contribute to their effects on imagery, new tasks could be developed that interfere effectively with imagery because they load heavily on visuospatial or auditory WM, but leave central executive resources available for engaging in mind wandering between sets of dual-tasking, which is assumed to connect the trauma memory with adaptive information stored other memory networks (Shapiro, 1991, 2018).

To address the need for further well-controlled evidence regarding the modality-specific interference hypothesis of EMDR, experiments 1-5 tested the effects of EMs on the vividness and emotionality of imagery compared to an auditory task that was designed to place similar demands on central executive WM resources, thereby controlling for the general cognitive load of the dual-tasks on imagery. To address the limitations of previous studies (Andrade et al., 1997; Lilley et al., 2009), the EM and auditory tasks used in experiments 1-5 were matched in terms of the multiple elements that may affect the general or executive WM load of the tasks, namely response speed, type of decision, and response modality. Furthermore, our experiments improved on previous studies (Gunter & Bodner, 2008; Van den Hout et al., 2011b) by instructing participants to form images that contained only visual aspects of the negative memory, thereby ensuring images would be susceptible to the visuospatial WM load of the EM task. A final improvement in our study compared to previous EMDR mechanism studies was that we confirmed, using objective measures of visual and verbal WM that our EM and auditory tasks selectively interfered with the visuospatial sketchpad and phonological loop, respectively.

In line with research demonstrating a therapeutic effect of EMs on negative autobiographical memories (Barrowcliff et al., 2004; Gunter & Bodner, 2008; Hornsveld et al., 2010; Kavanagh et al., 2001; Kemps & Tiggemann, 2007; Kristjánsdóttir & Lee, 2011; Leer et al., 2014; Van den Hout et al., 2014; Van den Hout et al., 2001; van Schie et al., 2016; van Veen et al., 2016; van Veen et al., 2015) experiments 3 and 4 found that performing EMs while holding in mind the image of a distressing autobiographical memory caused a larger reduction in the vividness and emotionality of the image compared to fixation. We believe these findings support the assertion that EMs interfere with imagery because holding an image vividly in mind and performing a concurrent task both rely on limited central executive WM resources (Andrade et al., 1997; Gunter & Bodner, 2008).

Crucially, the results of experiments 1-5 consistently showed that EMs did not reduce the vividness and emotionality of distressing visual imagery more than the auditory task. These findings suggest the visuospatial load of the EM task did not contribute significantly to its effects on imagery. Furthermore, we found some evidence that the auditory task was more effective at interfering with the imagery than the EM task (experiment 3 and 5), suggesting our failure to find a larger effect of EMs was not simply down to a lack of statistical power. Our findings support previous studies that have failed to find an additional effect of matching the modality of the concurrent task to the sensory modality of the image in terms of reductions in image vividness and emotionality (Kristjánsdóttir & Lee, 2011; Matthijssen et al., 2017; Mertens et al., 2020). It is likely that our results failed to replicate the larger effect of EMs in some previous studies (Andrade et al., 1997; Lilley et al., 2009) because the EM and auditory tasks in experiments 1-5 were more closely matched, whereas the effect of EMs in previous studies may reflect the larger executive WM load imposed by this task compared to the auditory task. We can be confident that our results do not reflect this same limitation. Had EMs outperformed the auditory task, we may have been inclined to provide objective evidence about the extent to which these tasks load the central executive, as a larger effect of EMs could arguably be caused by greater executive demands unless proven otherwise. That EMs were consistently no more effective than auditory interference provides a strong challenge to the modality hypothesis, even without objective evidence about the executive demands of the task. We did not find any consistent evidence to suggest that the auditory task was more difficult, less pleasant, or impaired the retrieval of the target image more than the EM task. It seems unlikely then that the larger effect of EMs was offset by non-specific factors specific to the auditory task – our data suggest that for all intents and purposes, our EM and auditory tasks did not differ in ways that would explain why they had a similar effect on imagery. Our findings seem more consistent with the suggestion that the general or executive WM load of EMs and other dual-attention tasks can account for the therapeutic effect on imagery more so than their demands on modality-specific WM resources (Gunter & Bodner, 2008). The implication for EMDR is that reducing the vividness and emotionality of trauma recollections with concurrent tasks may be enough to generate therapeutic effects, without needing to adapt these tasks or deliver them in a way that would increase the overlap with the modality of the target image. Our results suggest that research efforts may better directed toward adapting tasks so that they sufficiently interfere with the central executive to reduce image vividness and emotionality. However, as previous research on mind wandering suggests (see experiment 6, chapter 8), therapists may need to consider the possible trade-off between increasing the central executive demands of tasks to interfere with imagery and reducing the likelihood of participants generating new associations to the trauma memory while performing the concurrent task.

9.1.2 Toward a more comprehensive WM model of EMDR

Experiment 6 had to aims. The first was to establish a protocol that could be used in a laboratory setting to test if EMs facilitate mind wandering toward novel images. We argued that mind wandering is the same process Shapiro describes as multi-memory processing effects in EMDR (Shapiro, 2001, 2018), whereby the client retrieves new associations to the trauma memory. It is thought such effects help to reprocess the trauma memory by connecting it with information stored in other memory networks. While the reprocessing of memories is considered to be vital to the effectiveness of EMDR according to AIP theory (Shapiro, 2001, 2018), previous research has focussed on establishing that EMs desensitise the trauma memory – no study has looked at whether EMs facilitate reprocessing of the trauma memory. Experiment 6 addressed this gap in the literature. Specifically, the method was used to test if EMs contribute to the types of single and multi-memory processing effects that are used as evidence of reprocessing in EMDR. We referred to these effects as ERIs and EUIs, respectively (see chapter 8). We were able to demonstrate that the procedure used in experiment 6 was sensitive to ERIs and EUIs, and therefore that it can be used to detect the type of mind wandering that occurs in EMDR. We failed to find an evidence for the claim that EMs facilitate mind wandering compared to the same procedure without EMs. However, we also did not replicate the effect of EMs on image vividness and emotionality, which we argue is required to find an effect of EMs on mind wandering. Limitations in the design may explain why we failed to find the predicted effects of EMs. These limitations can be addressed in future research by including additional control tasks without dual-focus of attention, which may be an active component of EMs, and by measuring changes to more than just the imagery component of the memory, such as beliefs and physical sensation that also appear to change in EMDR. While there were some difference between the methods used in experiment 6 and the procedures of EMDR, our novel procedure provides a foundation for future studies of the contribution that EMs make to the reprocessing of trauma memories.

The second aim of chapter 7 was to provide an initial test of a novel theory about the role of WM in the reprocessing of trauma memories in EMDR. This hypothesis brings together related evidence from recent developments in the EMDR mechanism literature and existing evidence on the WM processes involved in mind wandering. Our hypothesis states that the central executive is involved when clients mind wander (i.e. report multi-memory processing effects, or EUIs) between sets of EMs in EMDR, and explains that EMs facilitate mind wandering by reducing the central executive load of the trauma image. Experiment 6 failed to find support for the predictions of our hypothesis – EMs did not supress mind wandering during dual-tasking more than no EMs, nor did EMs facilitate mind wandering when the task was removed. Again, limitations in the study design and procedure may explain our findings. We suggested several changes that may provide a stronger test of our hypothesis, such as using more opened ended questions to gather more detail about the effects of the EM task on the participants' imagery. This and other recommendations we make for future research may help to establish if and how EMs contribute to memory reprocessing in EMDR.

Our updated WM hypothesis (chapter 8) represents a potentially important step forward in establishing how the dual-tasking component of EMDR promotes recovery from PTSD. This is because we offer an explanation of how the immediate effects of WM interference on imagery – decreases in vividness and emotionality – contribute to the adaptive reprocessing of trauma memories in EMDR. Previous WM accounts (Andrade et al., 1997; Gunter & Bodner, 2008) have only sought to explain the immediate, or desensitising effect of EMs on emotional imagery. Importantly, our hypothesis is consistent with the application of EMDR to conditions other than PTSD in which unprocessed memories are thought to be a maintaining factor (e.g. Brown & Shapiro, 2006; de Jongh, van den Oord, & ten Broeke, 2002; Perlini et al., 2020; Shapiro, 2002). As Shapiro (2001) highlights in AIP theory, it is assumed that EMDR works for conditions beyond PTSD in which unprocessed childhood memories are a factor, and that EMs help to reprocess such memories. We would argue that EMs and other dual-attention tasks will facilitate mind wandering and therefore memory reprocessing so long as image associated with the memory places demands on central executive WM resources.

Another advantage of our hypothesis compared to previous WM accounts of EMDR is that it does not rely on the assumption that EMs must cause a decrease in the emotionality of the trauma image in order to have a therapeutic effect. As I highlighted in chapter 1, although the majority of EM studies have found effects on both image vividness and emotionality, some studies including our own (experiment 1b and 3) have found an added effect of concurrent EMs on vividness or emotionality, but not both. These findings raise questions about how closely associated the effects of EMs are on vividness and emotionality. The reason a close relationship has been considered important is that it is assumed the emotional benefit of EMs in EMDR is caused by a reduction in the vividness of the image in WM. Finding evidence that EMs reduce image vividness without emotionality would therefore raise questions about how changes in vividness alone can produce therapeutic effects, while an effect of EMs on emotionality but not vividness raises questions about whether WM interference is the underlying mechanism of action. We would argue that there strongest evidence changes in vividness causes changes in emotionality is provided by previous studies (Smeets et al., 2012), therefore the primary limitation of previous WM accounts of EMDR is in explaining the therapeutic benefit of EMs when only the vividness of the image is reduced. Previous explanations that may account for the benefit of holding a less vivid image in mind is that doing so may increase perceived mastery over the recollection of the distressing image (Oren, & Solomon, R. , 2012), foster acceptance of the image in its degraded form (Gunter & Bodner, 2008), and create a sense of psychological distance from the image held in WM (Gunter & Bodner, 2009). Our WM hypothesis equally does not assume that reductions in vividness caused by EMs must be accompanied by reductions in emotion in order to facilitate memory reprocessing in EMDR. However, an advantage of hypothesis is that it is more parsimonious, as we assume only the involvement of the central executive of WM to explain both the immediate effects of EMs on imagery and also how these effects translate to the therapeutic processes involved in EMDR. By explaining both the desensitisation and reprocessing effects of EMs according to WM theory, our hypothesis makes specific predictions about the factors that will influence the effectiveness of the dual-task component of EMDR, such as the central executive demands of the competing task. Furthermore, our hypothesis that the central executive plays a key role in both desensitisation and reprocessing in EMDR means existing WM research on this component for EMDR offers a set of well-established methods that can be used to test the predictions of our theory. Future research could make simple corrections to the procedure of experiment 6, such as measuring the executive functioning of participants or increasing the executive load of the dual-task, in order to test our theory about the role of the central executive in memory reprocessing. If our hypothesis garners support from such research, a direction for future research may be to investigate how EMs facilitate mind wandering to other memory components, such as new beliefs and physical sensations, and how EMs facilitate other parts of therapy that target positive and future imagery.

As I explained in chapter 8, our hypothesis predicts that verbal cognitions associated with unprocessed memories will also be degraded by concurrently taxing the central executive. This is because the central executive is presumably involved when clients are asked to hold the trauma-related thought/belief in mind alongside the trauma image when dual-tasking in EMDR. Emerging research suggests EMs reduce the clarity of such thoughts. It may be that this reduction in clarity and associated reductions in the emotionality of the though reduces the WM load of the thought, just as reductions in vividness and emotionality reduce the WM demands of imagery (van Veen et al., 2016). To be explicit, EMs and other dual-attention tasks should reduce the WM load of cognitions that place demands on central executive WM resources, and in doing so should free up executive resources for attention to wander to new images and thoughts. As it is assumed that changes to any component of the memory and retrieval of any new cognitions demonstrates successful reprocessing (Shapiro, 2001), we would argue that our WM hypothesis can explain the reprocessing of memories beyond just changes to the imagery component of the trauma memory. This prediction will of course need to be qualified by further research, starting with investigating if WM interference reduces the WM load of verbal cognitions.

Evidence that EMs reduce the vividness and emotionality of imagery for future events (Engelhard et al., 2012; Engelhard, van den Hout, et al., 2010) and positive memories (Barrowcliff et al., 2004; Engelhard, van Uijen, et al., 2010; Hornsveld et al., 2011; Kemps & Tiggemann, 2007; Van den Hout et al., 2001) is also relevant to the later stages of EMDR. Specifically, clients perform EMs while focusing on future templates: the client mentally rehearses managing challenging future scenarios (Shapiro, 2018). Moreover, positive or 'safe place' imagery, which clients use as a relaxation tool, is also combined with EMs in the preparation phase of therapy (Shapiro, 2001, p.125). EMs are reported to facilitate the processing and emotional benefits of positive material in EMDR (Shapiro, 2018). That EMs have an emotional blunting effect on positive images in laboratory studies has led to debate about whether using EMs with positive material may have adverse effects on treatment outcomes (Hornsveld et al., 2012; Hornsveld et al., 2011; Leeds & Korn, 2012). Reconciling this debate depends on how the role of WM interference in EMDR is interpreted. According to our hypothesis, the degrading effects of EMs on positive and negative imagery should similarly facilitate mind wandering to new images and thoughts, which would in turn promote the formation of new adaptive connections. Note that our hypothesis does not explain how mind wandering from future templates and positive imagery will promote recovery from EMDR, but it does resolve the counterintuitive effect that degrading positive imagery has therapeutic effects in EMDR. A natural step for future research would be to test if the effect of EMs on the WM load of negative imagery (van Veen et al., 2016) could be replicated with positive mental imagery.

9.2 How does this thesis contribute to existing research on the WM model of EMDR?

9.2.1 The potential impact of image rating procedures

The effect of EMs on imagery may depend on the number of times participants are asked to rate the target image. Visual inspection of the current data indicates that the EM and auditory task caused a larger pre-post decrease in image vividness and emotionality when participants rated the target image after each block of dual-tasking (experiments 4 and 5) than when ratings were taken at baseline and post-task (experiments 3 and 1, respectively). Each pair of experiments (4 and 3, 5 and 1) were otherwise identical, which suggests interference effects were enhanced by the requirement to repeatedly rate the target image.

Van den Hout et al. (2001) suggested that when participants are asked to rate their image in its current form after performing a concurrent task, they may instead reflect on how the image appeared and felt *during* the task. This argument was put forward to explain why the effects of performing EMs during recall were still present when the memory was recalled after the EM task had ended. Their argument highlights an important point about the WM hypothesis which appears to have been ignored in subsequent EM studies - WM theory only predicts reductions in vividness and emotionality during the concurrent task. If the effect of taxing WM on imagery is restricted to the period when the dual-task is being performed, image vividness and emotionality should recover after the dual-task is removed. The explanation provided by Van den Hout et al. (2001) suggests that imagery only appears to be less vivid following concurrent EMs because participants are reporting that the image was degraded while the EM task was being performed.

The larger dual-task effects in our experiments when participants rated the image multiple times is consistent with the suggestion that participants give ratings that are biased by previous image judgements. However, these data cannot simply be explained by participants rating the effect of EMs on the image during the task. If EM effects were restricted to the dual-task period, and subsequently recover after the task, the decrease in image ratings from baseline should have been similar after each block of dual-tasking. However, we found that ratings on image vividness and emotionality decreased with each subsequent rating (see experiments 4 and 5). Rather than recovering to baseline after each block of dual-tasking, the effects of dual-tasking on imagery in our experiments carried over from one period of recall to the next, only returning to baseline levels when participants were given longer to recall the memory (around 20 s post-task). Our findings our consistent with previous findings that the effects of dual-tasking carryover when the image is rated after each block of dual-tasking (Kavanagh et al., 2001; Lilley et al., 2009).

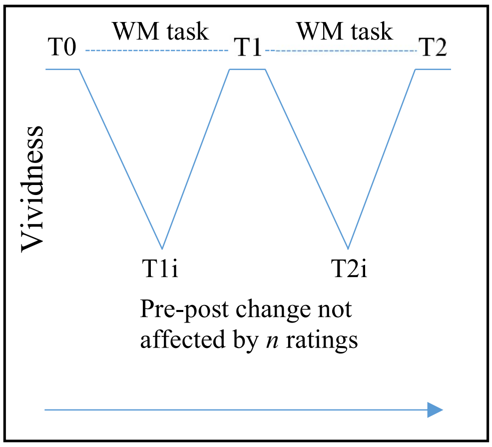
Our findings raise an important question about how performing several blocks of EMs can cause continual decreases in image vividness and emotionality. According to WM theory, the gradual decrease in vividness and emotionality across blocks of dual-tasking would suggest that the WM interference caused by the concurrent task increases over time. This seems unlikely to explain the additive effect of EMs in our experiments and previous studies, as the executive WM load of the dual-task should decrease with practice. Another explanation for the additive effects of dual-tasking on imagery is that each time the memory is recalled while dual-tasking, the memory is reconsolidated with the degrading effects of dual-task. Retrieval of the degraded image during subsequent blocks of dual-tasking would then further degrade the image followed by reconsolidation, and so on. The issue with this explanation is that reconsolidation of the effects of dual-tasking takes at least 6 hr (Nader et al., 2000), whereas the delay between each block of dual-tasking in studies finding additive effects of EMs are much briefer (around 10 – 20 s). Put differently, there would not have been sufficient time for reconsolidation of the image between blocks of dual-tasking in these studies, meaning the additive effect of EMs with each retrieval of the image cannot be explained in terms of reconsolidation. The continual decrease in emotionality of the image with each block of concurrent EMs is consistent with theories of EMDR that suggest EMs can directly reduce the emotional impact of negative memories by reducing emotional arousal (MacCulloch & Feldman, 1996), which is supported by larger reductions in markers of physiological arousal following EMs compared to no EMs (Barrowcliff et al., 2004; Barrowcliff, Gray, MacCulloch, Freeman, & MacCulloch, 2003). Van den Hout et al. (2001) offered an alternative explanation for the lasting effect of EMs on image vividness based on reductions in emotionality. Specifically, they argued that participants in their study might have used the de-arousing effect of EMs as biofeedback when judging the vividness of their memory at post-task, hence why an effect of EMs on vividness was found after the WM interference caused by EMs was removed. There are at least two issues with this explanation. The first is the evidence that the vividness of neutral mental images is also lower after concurrent EMs compared to a no-EMs (Andrade et al., 1997; Leer et al., 2017). As neutral images are unlikely to elicit emotional arousal, an effect of EMs on the vividness on these images after outside of dual-tasking cannot be explained by biofeedback. Furthermore, it seems unlikely that the effects of dual-tasking on vividness are caused by reduced emotional arousal, given that reductions image vividness appear to occur before reductions in emotionality (Smeets et al., 2012). As far as we can tell, no explanation has been put account for how a reduction in image vividness during one block of EMs can carry over to the next block of EMs when both retrievals occur within the window of consolidation.

I suggested in chapter 6 that the larger effect of EMs with repeated rating of the target image raises the possibility that when participants were asked to rate their image 'right now', they intuitively try to compare the current mental image to the image as it was when last retrieved. That is to say they anchor their score based on the previous experience of the image rather than the anchor points of the scale (e.g. image as vivid/emotional as real life versus no image/emotion). I argued that comparing the current image to its most recent variant could explain the additive effect of dual-tasking, if we assume that participants make a same-different judgements. According to this explanation, participants may use rating of the last image as an anchor, which is then used to rate the current image. If the current image is less vivid than the previous image, the current image would be given a lower rating. With each subsequent same-different judgement, the final rating would then represent the sum of changes to the image across multiple ratings, rather than representing how vivid and emotional the image at post-task is compared to the actual experience of the distressing event. Of course this explanation is based on the assumption that the image recalled after the task remains degraded by the preceding dual-task interference. As I argued above, there may be no theoretical basis for predicting a lasting effect of dual-tasking in the moments after the dual-task is removed.

Perhaps combining van den Hout's explanation - that participants rate the image during rather than after the dual-task - with our explanation - repeatedly rating the target image encourages comparisons between images – would better explain the additive effect of EMs found in our experiments and previous studies. Let us assume that the sustained effects of WM interference outside of dual-tasking are due to participants rating the image during the dual-task rather than after. Whereas our earlier explanation for the larger effect of EMs with multiple image ratings assumed that participants make comparisons between the image before and after a block of dual-tasking, it may instead be the case that they are making a same-difference judgement between the image retrieved *during* the most recent block of dual-tasking and the image retrieved *during* the previous block of dual-tasking. This is a subtle difference from van den Hout et al's., explanation, which assumes that participants are rating the vividness of the image during the dual-task compared to actual perceptual experience.

It may be that repeatedly rating the target image shows additive effects of dual-tasking because participants rate the image during the most recent block of dual-tasking compared to the image as it appeared during the previous block of dual-tasking. Figure 24 below summarises the expected effects of dual-tasking across multiple ratings of the image according to van den Hout's explanation (a) and our own (b). As van den Hout suggest, when a participant reports that the image retrieved after a block of dual-tasking (T1) is less vivid than baseline (T0), they may instead be reporting that the image during the dual-task (T1i) was degraded. According to WM theory, the image after the task (T1) would have recovered to baseline, but van den Hout et al's explanation is that it appears that the effect of dual-tasking is still present because the rating of the image after the task reflects the impact of resource competition on the image during the dual-task. With each subsequent retrieval of the image (T2), the participant should again rate their experience during the dual-task (T2i). As the effects from the first block are not yet reconsolidated, the image should recover each time the participant is asked to bring the image to mind between sets of dual-tasking. We similarly would argue that vividness should recover to baseline level when the WM interference of the dual-task is removed. However, we would also argue that if participants report a decrease in image vividness because they are rating their experience during the dual-task (T1i), they may then use this rating as an anchor when rating the image as it appeared during the next block of dual-tasking (T2i). If participants use the previous rating (T1i) as an anchor when reporting a decrease in vividness on the next block of dual-tasking (T2i), this would result in a sum of the ratings (T2i – T1i) and hence a linear decrease over time. The difference between our explanation and that of van den Hout et al. is that we assume participants are making comparisons between images, or at least ratings when asked to make repeated judgements of vividness and emotionality.

a)



b)

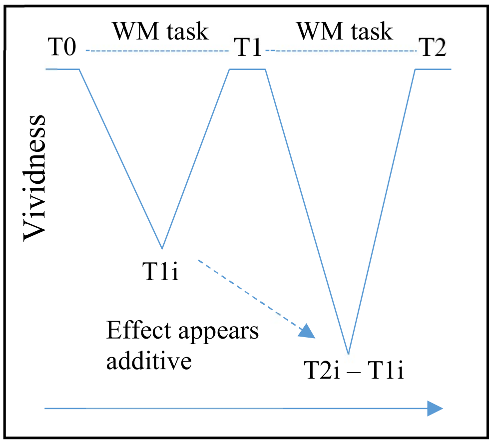


Figure 24: an illustration of how dual-tasks may have appear to have lasting effects on imagery after removal of WM interference. Participants may rate the image during the task compared to actual experience (a), or in relation to the previous retrieval of the image (b).

Note that the above explanation for the additive effects of dual-tasking is consistent with our finding that ratings increased rapidly between the rating taken immediately after the last block of dual-tasking and the image taken after subsequent imaginal exposure (experiment 3 and 4). The difference between the image during the final block of dual-tasking and the image held in mind during subsequent imaginal exposure would have been more noticeable in the EM task condition than the fixation task condition, as the image during the last block of EMs would have been degraded by the WM load of the task. We do not assume that previous studies which use only pre-post ratings are affected by participants making same-difference judgements; only studies that use multiple ratings of imagery may encourage this type of rating, hence the pattern of results observed are different to those found in pre-post studies. The results of studies that use pre-post only ratings may reflect van den Hout et al's. argument that ratings taken at post-task, following imaginal exposure, represent the extent to which the image was degraded during the final block of dual-tasking i.e. how much the dual-task reduced the similarity of the image to actual perception.

To summarise, in the absence of a clear mechanism by which the impact of concurrent WM interference can continue to effect images once the WM interference is removed, the best explanation for how EMs can have an effect on imagery at post-task can be explained by in terms of how participants interpret the instruction to rate the image held in mind. van den Hout's suggestion that participants give ratings at post-task that represent the image during the dual-task can explain the larger decrease in vividness and emotionality caused by EMs when the study procedure involves few ratings i.e. baseline and post-task. We can assume that when the study uses only pre-post measurements, it is unlikely to encourage participants to compare the image during the final block of dual-tasking to previous retrievals of the image, therefore post-task ratings in these studies are more likely to reflect the extent to which the image represents the actual perception and experience of the imagined episode. As there is no apparent mechanism by which the effects of concurrent WM interference can be sustained after removal of the concurrent task, the vividness and emotionality of the image presumably recovers at post-task, only appearing to be less vivid due to participants rating the image during dual-tasking. When the study includes repeated ratings of the target image, in addition to pre-post ratings, this is likely to encourage participants to compare each retrieval of the image to the previous retrieval. This can explain why the effects of EMs appear to be additive in such studies, as each rating is anchored to the one before, reducing the meaning of each rating to a same-different judgement rather than a description of how closely the image represents re-experiencing the imagined event. Studies that use repeated measurements of the image may be less likely to find a sustained effect of EMs when the participant engages in imaginal exposure post-task, as the participant may be more likely to report larger increases in image vividness and emotionality when the previous image was degraded by the WM load of the task, than if the previous image was relatively unaffected because the concurrent task placed few demands on WM resources. While increasing the frequency of ratings during the dual-task may increase the sensitivity of the study to the effects of taxing WM on imagery, the trade-off may be that it is harder to interpret the pre-post decrease in vividness and emotionality. Since clients in EMDR are not asked to rating the vividness and emotionality of the image after every block of dual-tasking, studies using pre-post rating designs may offer a more generalizable comparison to EMDR when the aim is to establish if EMs cause the target image to appear and feel less like reliving the trauma episode.

Our explanation for the additive effects of EMs is of course is entirely speculative and requires an experimental approach to first confirm if interference effects are moderated by the number of imagery ratings. Preliminary analysis of combined data Experiments 3 (pre-post ratings) and 4 (repeated-ratings), taking into account the experiment as a between-subjects factor, suggests pre-post changes in vividness was not significantly affected by study design, $F(2, 140) = 2.63$, $p = .076$, partial eta-squared = .036. However, a more conclusive tests will require researchers to compare pre-post changes in imagery to EM procedures with increasingly frequent image ratings. Findings could then be used to investigate underlying psychological processes if it was confirmed that tasks interference to a greater extent depending on the rating procedure that is used.

Turning to the broader question of how researchers should measure the effects of concurrent task performance on imagery, it would also be interesting in future studies to investigate the effects of EMs when the vividness/emotionality of the concurrent image is continuously measured. This could involve, for example, having participants turn a dial or adjust a sliding scale to provide 'live' recordings of their experience; although the exact method will depend on the response requirements of the interference task – tasks used in Experiments 1-5, for example, would require a hands-free method due to the button press requirement. Alternatively, participants could be asked to provide a verbal rating of image vividness and emotionality while performing the concurrent task, as this is less likely to encourage comparisons between the momentary changes in the image. Such a method would also provide a more valid test of the WM hypothesis, which predicts effects on imagery *during* the performance of a concurrent task. While some researchers have measured imagery immediately after dual-task performance (Andrade et al., 1997; Smeets et al., 2012), there is a need for research in which the effects on imagery are measured while the secondary task is being performed.

9.2.2 The potential importance of study design

Inconsistencies in the results of Experiments 1-5 suggest the choice of study design may be important when investigating the effects of dual-tasking on imagery. Specifically, an added effect of dual-tasking (EMs and auditory interference) compared to no-task (fixation) was found in Experiments 3 and 4 (within-subjects) but not in Experiments 1, 2, and 5 (between-subjects). This suggests the use of within-subjects design in the present research was necessary for demonstrating a significant effect of dual-task performance on imagery.

The current findings form part of a wider collection of laboratory studies using both within and between –subjects designs. Collectively, these studies paint a mixed picture about the need to investigate EM effects using a within-subjects design. Most of the studies cited as evidence for an effect of EMs have used a within-subjects design, while those using a between-subjects design are relatively few in comparison. Of the studies that have used a between-subjects design, results reveal that an added effect of EMs on image vividness and/or emotionality is observed (Leer, Engelhard, Altink, et al., 2013; Smeets et al., 2012; Van den Hout et al., 2013; van Veen et al., 2016) almost as often as no effect of EMs (Keller et al., 2014; Leer, Engelhard, Dibbets, et al., 2013; Leer et al., 2017). The discrepancy in the effect of EMs in between-subjects designs suggests differences in features other than study design may be the reason for mixed findings. However, as our experiments (3 versus 4, 1 versus 5) differed only in terms of the study design, they suggest that the added effects of EMs on imagery are generally less reliable when using a between-subjects design, which may explain why previous studies using a between-subjects design offer less consistent evidence for an effect of EMs than those that have used a within-subjects design. It is likely that this unreliability is due to individual difference in the subjective interpretation of image vividness and emotionality, and therefore variation in the perceived effects of WM interference on these ratings.

It is perhaps not a coincidence that the average size of the effect of EMs compared to no EMs in is larger in studies where concurrent negative recall was performed in laboratory settings compared to therapy. Inspection of the results from (Lee & Cuijpers, 2013) meta-analysis revels that the additional effect of EMs compared to a no-EM control (the definition of which varied between studies – some use dual-focus of attention, while other involve no-dual focus of attention. Research by Sack et al. (2016) suggests the effect of EMs may be smaller when compared to dual-focus rather than recall alone) was, on average, larger in laboratory studies (combined outcomes: cohen's *d* g = 0.74; subjective distress: cohen's *d* g = 0.66) than in treatment studies (combined outcomes: cohens *d* g = 0.41; subjective distress: cohen's d g = 0.53). As the authors note, the larger effect of EMs in treatment studies may reflect the additional therapeutic components that are present in EMDR but not in laboratory studies. While this is certainly a possibility, it is notable that all but one of the treatment studies reviewed used a between-subjects design, whereas most of the laboratory studies included used a within-subjects design. While a tenuous explanation, the larger effect of EMs in laboratory studies may reflect the general tendency for researchers to use within-subjects designs.

9.3 Limitations of experiments 1-6

It is a general limitation of studies on the WM mechanism of EMDR that it is not checked if participants concurrently held the image in mind and performed the competing task. In experiments 1-5, we cannot rule out the possibility that participants stopped focusing on the task or target image at points during the dual-task procedure. Ratings of image accessibility in experiments 2-5 suggested that participants were not always focused on the target image during the EM and auditory tasks, as they indicated they found it difficult to retrieve the image at least some of the time. This could be interpreted to mean participants sometimes stopped focussing on the image because they were focussed on the concurrent task, which required them to detect the presentation of a target letter among distractors. It may also have been the case that participants stopped focussing on the competing task at points in the procedure. We would not have necessarily been able to detect a lack of focus on the dual-task after the final target letter was presented, as participants may have learned the number of targets presented in each experiment block was the same; therefore, they could have stopped paying attention to the task without our knowledge after the final target in the block was presented. One way to determine if participants were focussed on the image during the dual-task would have been to ask participants at random points during the dual-task procedure to report if they were focussed the target image. To ensure compliance on the competing task, participants could have been observed by the experimenter to ensure they were tracking the letter stimuli in the EM task. Alternatively, the number of target letters presented in each block of the EM and auditory task could have been randomly selected to encourage participants to maintain their focus on the task in order to detect the anticipated target letter. Both monitoring compliance with the recall instruction and instruction to perform the dual-task would provide greater confidence that participants were attempting to divide their attention between the task and negative image. Since the WM hypothesis explains the effects of EMs on imagery in terms of competition for limited cognitive resources, studies on the WM hypothesis must ensure that participants are dividing their attention between recall and the dual-task in order the claim that any effect of the dual-task intervention was due to WM interference.

As for the instructions used to generate imagery in our experiments, we did not specify that participants should generate an image of themselves from an observer (third person) perspective rather than first person perspective. EMDR protocol encourages clients to distance themselves from the trauma memory by viewing the event in a detached manner, as opposed to re-living the experience. Furthermore, Lee and Drummond (2008) found an effect of EMs on distressing image vividness when recall was combined with distancing instructions (e.g. asking participants to picture the event as if it were projected on a movie screen), but not re-living instructions (asking participants to try and imagine the event as if it was happening in the present). The authors suggested that asking people to re-live the event might have prevented EMs from reducing vividness, while distancing instructions allowed or somehow facilitated interference. Applying this evidence to the present experiments, effects of WM interference on imagery may have been greater if instructions emphasised distancing from the recalled event. Another limitation of the image instructions used in experiments 1-5 is that they did not specify that participants should focus on a particular moment or ‘hot spot’ from the memory, only that they should recall a negative episode and form an image of the visual aspects of the memory. This instruction may have led to considerable variation in the detail of participants’ images. Participants are more likely to notice and report a reduction in the vividness of images that previously were highly detailed compared to an image that contained little specific sensory information. In contrast, instructions in experiment 6 - which were based on the image selection procedures used in EMDR and previous EM experiments (e.g. van Veen et al., 2015) – asked participants to recall a specific, highly emotional moment from the negative event. If similar instructions had been used in experiments 1-5, the effects of the EM and auditory tasks on imagery may been larger, and perhaps more consistent.

It could be argued that the criterion we used to select memories for experiments 1-5 meant that the resulting images used in the experiment were not emotional enough to detect a reliable effect of EMs. Specifically, we used an inclusion criterion where the memories selected at baseline had to be rated as 4/10 or above in terms of emotionality to be included in the experiment. It could be argued this cut-off was too low, leading to some of our participants retrieving memories that were not emotional enough for us to reliably detect the impact of taxing WM on imagery. According to recent research, the presence of emotional arousal during recall may be a prerequisite to finding an effect of EMs on the vividness of a memory. In the first of three related studies, Van den Hout et al. (2014) found that performing EMs during recall reduced memory vividness if participants recalled an emotional event, but not if they recalled a neutral event. Littel, Remijn, Tinga, Engelhard, and van den Hout (2017) later found that EMs only caused a pre-post task reduction in the vividness of neutral memories if participants performed a stressful task prior to recall, which increased emotional arousal. That EMs affected neutral memories suggested the memory itself did not have to be emotional. Rather, the presence of arousal during recall was somehow important. The authors surmised that increases in noradrenaline during emotional recall might promote the reconsolidation of the memory, and the effect of EMs, in much the same way that noradrenaline activity promotes encoding of emotional information (Cahill & McGaugh, 1998). Accordingly, if emotional arousal is reduced during recall, this should prevent the effect of EMs on memory vividness from becoming reconsolidated. Subsequent research offered preliminary support for this proposal. Littel, Kenemans, et al. (2017) showed that reductions in the vividness of negative memories were abolished if propranolol was used to inhibit the action of noradrenaline during the EM intervention. Crucially, and unlike the two previous studies, effects on memory vividness were shown after a 24-hour delay – outside of the reconsolidation window - meaning the results offer stronger evidence that the effect of arousal relies on memory reconsolidation.

Though the above research is compelling, the findings of these studies perhaps raise more questions than they answer. If lasting effects of EMs depend on reconsolidation of the memory in a weakened form, but reconsolidation depends on emotional arousal, then successful treatment requires both that the emotionality of a memory is reduced as much as possible and that it remains high enough for reconsolidation to occur. Given the delicate balance that would be required to cause changes to the target memory, it is surprising that reductions in vividness ratings are so well replicated within the EMDR literature. Furthermore, the conclusion that arousal may be important was partly based on the failure to find an effect of EMs at post-task (Littel, Remijn, et al., 2017), which is too soon after the dual-task to be explained in terms of reconsolidation. If the lack of arousal blocks pre-post reductions in vividness, this does not explain why other studies have found an effect of EMs the vividness of neutral memories immediately after dual-tasking (Andrade et al. (1997). Why some studies have found arousal to be vital for pre-post reductions in vividness while others have not is an important question that deserves further attention. It is noteworthy that the studies finding arousal to be a necessary precondition used neutral autobiographical memories as the intervention target, whereas studies that have found no requirement for arousal used memories of novel neutral stimuli that participants encoded for the first time during the study procedure. This distinction between targeting recent versus long-term neutral memories may only be incidental, but should not be overlooked.

It is possible that experiments 1-6 would have yielded more reliable and larger effects of our experimental tasks had our control task involved recall only rather than concurrent fixation. We decided to use fixation in order to limit the amount that participant may naturally make EMs if told to look at a blank screen. While it is unlikely that the requirement to focus on a stationary stimulus produced a substantial WM load, we must acknowledge previous evidence that dual-focus of attention (central fixation) during memory retrieval in EMDR leads to superior outcomes than the same procedure without engaging attention using a secondary task (Sack et al., 2016). Furthermore, using the studies analysed by Lee and Cuijpers (2013) in their meta-analysis, there is some evidence that the average size of EM effect in laboratory EM studies was larger when the control task involved no dual-focus of attention (e.g. looking at a blank screen: *M* Cohen's d = 0.943, *SD* = 0.136) than when the control task involved dual-focus of attention (e.g. looking at a stationary circle: *M* Cohen's d = 0.706, *SD* = 0.407). Based on this preliminary evidence, it is possible that had we used a control task that did not involve dual-focus of attention, we may have found a clearer effect of the general WM load imposed by the experimental tasks on measures of vividness and emotionality.

9.4 Directions for future research

While the tasks used in experiments 1-5 served to determine if the visuospatial and executive WM load demands of EMs interfere with visual imagery, our experiments were not designed to distinguish the active components of the EM task beyond its visuospatial WM load. However, a helpful direction for future research would be to include additional control tasks that vary single components of the EM task, in order to investigate which components are active. As I mentioned in chapter 8, an improvement in future experiments may be to include both a procedure that involves dual-focus of attention without EMs (i.e. fixation on a stationary cross) and no dual-focus of attention (e.g. staring unfocussed toward a blank wall/screen). This would not only allow researchers to investigate the effect of EMs compared to recall alone, which should be larger than compared to fixation, as well as testing if the active component of the EM task is involved in the movement of the eyes. Previous research indicates that making voluntary eye movements interferes with the maintenance component of spatial WM compared to keeping both eyes still (Postle, Idzikowski, Sala, Logie, & Baddeley, 2006). The larger effect of EMs compared to central fixation (experiment 3 and 4) may therefore reflect the additional attentional processes involved in planning and executing EMs, though clearly the effect these processes have on visual imagery are matched by the demands of the auditory task. There is also evidence that dynamic visual displays disrupt performance on imagery-based tasks (McConnell & Quinn, 2000; Quinn & McConnell, 1996), suggesting the striped background may have contributed to image interference; the static striped display would have created a dynamic image on the retina during saccades. In future studies, removing individual aspects of the tasks used in experiments 1-5 may prove useful in investigating the mechanisms by which the active components of the task contribute to the desensitisation and reprocessing of memories in EMDR.

In addition to investigating the mechanisms by which the components of EMDR contributes to its effectiveness, there is a need for further research to determine which components of EMDR are active. Without establishing, for example, that the mind wandering component of EMDR makes a meaningful difference to clinical outcomes, efforts to investigate the mechanisms by which EMs contribute to this process may be better directed to understanding the mechanisms of other components of EMDR that make a bigger difference to treatment effectiveness. Even if one component, such as

EMs, contributes indirectly to other components of EMDR, such as free association, its removal should be consequential in terms of treatment outcomes. That is to say component research can identify which aspects of EMDR are important, and further controlled research can analyse these components to determine how they contribute to therapy.

Most component research has focussed on establishing if the EM component of EMDR contributes to its therapeutic outcomes (e.g. Davidson & Parker, 2001; Lee & Cuijpers, 2013) and more specifically which components of this EM procedure – bilateral stimulation or dual-focus of attention – actively helps recovery from conditions such as PTSD (Sack et al., 2016). In her book, Shapiro (2001, pp. 364-369) summarises existing evidence from and ideas for future component research, such as comparing the effectiveness of EMDR with bilateral versus unilateral stimulation, and with dual-focus on autobiographical versus trauma memories. The logic of these recommendations is that EMDR including the proposed active component should be more effective than the same procedure in which this component is removed. As I explained in chapter 8, and as Shapiro highlights, there is a need for research aimed at establishing if the mind wandering component of EMDR facilitates recovery from trauma, as the AIP model suggests this component is vital in the adaptive reprocessing of trauma memories (Shapiro, 2001; Shapiro & Maxfield, 2002). Consistent with the logic of testing EMDR's effectiveness without other components of therapy, removal of the mind wandering procedure, by asking participants to focus only on the target memory, should reduce the effectiveness of EMDR in terms of treating PTSD symptoms compared to the standard procedure in which clients are instructed to let their mind wander to new information.

As our hypothesis suggests the effects of WM interference on image vividness and/or emotionality contributes to EMDR by facilitating this mind wandering component, establishing that mind wandering contributes to the effectiveness of EMDR would be a sensible first step in establishing the external validity of our hypothesis, and any support it my garner from future research. The procedure described in experiment 6 can be used to provide complimentary evidence, in a laboratory setting, by exposing participants to an analogue of the desensitisation phase of EMDR without and without mind wandering, where the outcomes measured could be the reduction in distress caused by the target memory. Our procedure could also extend existing research on the EM component of EMDR by replicating experiment 6 to investigate if EMs contribute to the effectiveness of the mind wandering process. Specific predictions about the underlying mechanism of action can then be investigated. The specific predictions made by our WM hypothesis about the role the central executive may play in the contribution of EMs to mind wandering (chapter 8) could be tested in order to elucidate the mechanisms by which EMs contribute memory reprocessing in EMDR, in addition to testing how the desensitisation of memories is affected by varying interference with the central executive. The importance of establishing how WM contributes to the effects of EMs on distressing imagery and mind wandering is that this research can be used to focus efforts on improving the efficiency and effectiveness of treatment (Holmes et al., 2018). Evidence that EMs contribute to therapeutic changes in EMDR, such as reduced subjective distress and reduced validity of unhelpful cognitions (Lee & Cuijpers, 2013) are an important foundation for further research into the processes by which these changes occur, which in turn can be used to offer evidence-based recommendations based on an understanding of the underlying mechanisms involved.

10 Conclusion

The WM hypothesis of EMDR has provided several testable predictions about the effects of EMs and other forms of dual-attention stimuli as used on their own or as part of EMDR. Reliable evidence has been garnered to support predicted reductions in the vividness of negative, positive and neutral mental imagery related to negative autobiographical memories. The WM hypothesis has also, for the most part, successfully predicted the modulation of dual-task outcomes according to concept of a limited capacity storage and rehearsal system. Where predictions have not been supported with respect to the concept of modality-specific slave systems, null-results might be explained by limitations in task design and better explained by the existence of a limited capacity attentional system. A key limitation of the WM hypothesis relates to unreliable and effects of concurrent WM load on the intensity of emotion associated with upsetting imagery. Important predictions about to the interaction of WM capacity and task load in determining image outcomes have also been challenged by empirical findings, potentially limiting the practical implications of the WM hypothesis. With these limitations considered, experiments 1-5 provide presented in this thesis offer an important step forward in addressing key questions about the role that modality-specific WM systems play in the desensitisation of distressing memories in EMDR. Furthermore, experiment 6 addresses the limitations of previous WM accounts in explaining how a WM mechanism might contribute to the adaptive reprocessing of memories during EMDR.

The results of experiments 1-5 and previous EMDR WM research suggest that EMs reduce the vividness and emotionality of negative and trauma-related imagery by taxing limited central executive resources. Furthermore, mind wandering of the type that occurs in EMDR also appears to rely on limited central executive resources (experiment 6). If the central executive plays a major role in both the desensitisation and reprocessing components of EMDR, as our WM hypothesis suggests, the recommendation for clinical practice is that the dual-attention tasks used in EMDR should be most effective when they impose sufficient demands on the central executive to reduce the vividness/emotionality – and thus the WM load of the target image, but not be so demanding that they interfere with the ability to mind wander during the dual-task procedure or deplete executive functioning to the point that mind wandering between sets of dual-tasking is impeded. While it may not be feasible to achieve this balance within the context of therapy, we would argue that the evidence from this thesis and previous WM research suggests against simply increasing the WM demands of the dual-attention task without consideration of the unintended impact this may have on the mind wandering process that presumably drives the adaptive memory reprocessing of memories in EMDR.

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12 Appendices

Appendix A

The following was considered as an alternative method of analysing the results for experiment 1b. This analysis was not used because it involved using additional tests to compare if ratings of vividness, emotionality and negative affect had decreased significantly from baseline, before then comparing the change from baseline between task conditions. The method used in our main analysis, 2x3 ANOVA, allowed us to investigate the effect of time, task and their interaction in a single model, therefore reducing the risk of type-1 error.

Vividness

Histograms and Q-Q plots showed that baseline and/or post-task vividness scores were negatively skewed in all task conditions. Transforming vividness scores using square and exponent transformations failed to correct skewness, therefore we analysed vividness scores using non-parametric tests.

To test our prediction that the EM and auditory tasks would reduce image vividness, but not the fixation task, we compared vividness at baseline and post-task in each task condition. Preliminary inspection of the data using boxplots revealed that pre-post difference scores in all task conditions were distributed asymmetrically. Therefore, we used separate Sign tests to analyse the change in vividness in each task condition. This revealed that image vividness decreased significantly from baseline in the EM, $Z(38) = -2.69$, $p = .007$, and auditory task conditions, $Z(38) = -2.79$, $p = .01$, but did not change significantly from baseline in the fixation task condition, $Z(38) = -0.95$, $p = .35$.

To test if the EM task had a larger effect on image vividness than the auditory and fixation tasks, a Kruskal-Wallis H test was used to compare the mean ranked change in vividness between task conditions. This revealed that the change in image vividness from baseline to post-task did not differ significantly between task conditions, $H\,(2) = 4.04$, $p = .13$, $\varepsilon^2 = .03$. To determine if this null result was due to our use of a non-parametric test – which tend to be less powerful than parametric tests – we also performed a one-way ANOVA to compare the change in vividness between task conditions. This analysis also showed that the mean change in image vividness from baseline to post-task did not differ significantly between task conditions, $F(2, 117) = 2.21$, $p = .12$, $\eta p^2 = 0.04$.

Emotionality

To test if image emotionality decreased from baseline, we compared the median rank of emotionality at baseline and post-task. Boxplots showed that in all task conditions, the distribution of difference scores was approximately symmetrical. We used Wilcoxon-Signed rank tests to analyse the change in emotionality in each task condition. This revealed a significant median decrease in emotionality in the EM, $Z(38) = -3.30$, $p = .001$, and auditory task conditions, $Z(38) = -3.67$, $p < .001$. In contrast, image emotionality in the fixation task condition did not change significantly from baseline, $Z(38) = -1.42$, $p = .16$.

A Kruskal-Wallis H test revealed that task conditions differed significantly in terms of the mean ranked change in emotionality from baseline, $H\,(2) = 8.91$, $p = .01$, $\varepsilon^2 = .08$. Post-hoc Dunn's tests, using Bonferroni correction, indicated that the image emotionality decreased significantly more in the auditory task condition (68.95) than in the fixation task condition (47.43), $p = .02$. None of the other pairwise comparisons were significant, all *ps* > .06.

One-way ANOVA showed that task conditions differed significantly in terms of the change in image emotionality, $F(2, 117) = 4.27$, $p = .02$, $\eta p^2 = 0.07$. Pairwise post-hoc comparisons, using Bonferroni correction revealed that the decrease in image emotionality was greater in the auditory task condition than in the central fixation condition ($p = .02$, $d = 0.64$). None of the remaining comparisons were statistically significant.

Negative Affect

Boxplots showed that in all task conditions, the difference between baseline and post-task negative affect was approximately symmetrical; therefore, separate Wilcoxon-signed rank tests were used to compare the change in negative affect between task conditions. Analysis showed there was a significant median decrease in negative affect in the EM, $Z(38) = -3.50$, $p < .001$, auditory, $Z(38) = -3.80$, $p < .001$, and fixation task conditions, $Z(38) = -3.21$, $p = .001$.

A Kruskal-Wallis H revealed that task conditions did not differ significantly in terms of the mean ranked change in negative affect, $H\,(2) = 0.68$, $p = .71$, $\varepsilon^2 = .006$.

Appendix B

Below is the script that was used to deliver the instructions for experiment 6.

EXPERIMENTER'S SCRIPT

KEY:

Read text **in bold** word-for-word on first time of reading.

Read text in CAPITALS with emphasis.

Read *italicised* text depending on the participant's response/task condition.

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INTRODUCTION AND PRACTICE TRIAL

We are interested in the MENTAL IMAGES that come to mind when people recall negative memories. Mental imagery involves using your imagination to experience something that is not present in real life. It can be imagining what something looked like, but also what something sounded, smelt, tasted, and felt like – imagining something with all of your senses.

Today I will ask you to imagine a particularly unpleasant moment from a negative memory. I will then ask you to bring this upsetting image to mind and perform a second task. I would like to give you a demonstration of the task that you will be performing.

Please sit facing the screen and move your chair so that you stomach is touching the desk. The task requires you to look at a dot on the screen. The dot will ((Eyes Stationary)) *Remain stationary in the middle of the screen* ((Eye Movement)) *Move from left to right*. Keep your head still and look at the dot until it disappears.

Do you have any questions about this?

Look at the dot on the screen. Now, continue looking at the dot until the task has finished.

Experimenter starts the task

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MEMORY RECALL

I would like you to recall a single negative memory. This memory should be of one occasion that has made you very fearful, anxious, or distressed and that STILL has some emotional impact on you when you think of it NOW. If you need a moment to come up with a memory then let me know.

Wait until the participant indicates that they have selected a memory

How UNPLEASANT would you say this memory is for you right now on a scale of zero to ten, zero being not unpleasant at all and ten being extremely unpleasant?

Experimenter records the participant's response

If the score is below 4, say "Would I be right in saying that this memory is unpleasant to a limited extent? For this study, we are interested in memories that are unpleasant to a somewhat higher degree when you think back on it. Could you select a new memory that is somewhat more unpleasant to you?

If the participant still cannot come up with a different memory, say, "if you feel it would be useful, I can give you a list of examples - the types of memories other people have selected."

Roughly, how long ago did the event take place?

Experimenter records the participant's response

I would now like you to describe the event to me. Could you give me a BROAD OUTLINE of how you remember the unpleasant event, from the point where you feel it began up to the point where you feel it ended? **Remember that everything you say is confidential.** Do you have any questions about this?

If the participant says no more than a few words, ask, "Could you tell me a little more about this memory? Where exactly does this memory begin...? What happened then…?

If it appears the description will take too long to reach a conclusion, interrupt the participant and say, "I understand that you are trying to remember and tell it all correctly, but I would like to ask you to tell me your memory in a BROAD outline. Is that okay with you?"

Allow the participant to tell describe the memory until they reach a natural conclusion

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IMAGE SELECTION AND RATING

If you visualise this memory in your mind, what is now, at this moment, the most unpleasant image for you to focus on? Play the memory in your mind as if it were a movie and freeze it when it is most unpleasant, so that it becomes a static image. Let me know when you have an image in mind by saying ""yes".

Experimenter waits for the participant to confirm they have an image in mind"

I would like you to choose a title for this image. This should be a reasonably neutral title, but one that you feel refers to this specific image of the memory. A few words is sufficient.

Experimenter records the image label

In a moment, I would like you to bring this image to mind and focus on it until I say stop. I will ask you questions about the image while you focus on it. Do you have any questions about this?

Please face the screen and keep your eyes open. Now, bring the target image to mind. Let me know when you have it in mind by saying "yes".

Experiment waits for participant to confirm they have the image in mind

How VIVID is this image of the memory in your mind right now on a scale of 0 to 10, 0 being 'no image at all' and 10 being 'image as clear and as vivid as real life'?

Experimenter records the participant's reponse

How EMOTIONAL is this image in your mind right now on a scale of 0 to 10, 0 being 'neutral' and 10 being 'as bad as if it were happening'?

Experimenter records the participant's reponse

Ok, you can stop focusing on the image.

From now on**, I will refer to the image you were just focusing on as 'THE TARGET IMAGE'.** Whenever I talk about 'the target' or 'the target image', I am referring to this image of the memory.

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INSTRUCTIONS FOR MAIN PROCEDURE

Now we are going to start the main part of the experiment in which you will perform the task you practiced earlier.

We will start by asking you to mentally focus on the target image. Then I will ask you to look at the dot on the screen. After we do this for a while, we will stop and I will ask you to take a deep breath and let your mind go blank. I will then ask you to notice and report the first image that comes into your mind. You can describe this mental image in any way you like, using as few or as many words as you wish.

It is important that throughout this process you just let whatever happens, happen. The target image may change in appearance, or it may just stay the same. Sometimes new images may come to mind that are related or unrelated to the memory; equally, you may experience only the target image. Just notice what happens, without trying to influence it or judge whether it should be happening or not.

Could you describe to me your understanding of what you have to do?

Experimenter listens and corrects the participant if there is any misunderstanding

Do you have any questions before we start?

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START OF TASK

Please face the screen.

Bring the target image to mind. Let me know when you have it in mind by saying 'YES'.

*** Experiment waits for the participant to confirm they have the image in mind***

Focus on this, and now look at the dot.

Experimenter starts the task

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END OF TASK:

Good. Keep looking toward the screen. Now, **take a deep breath and let go of whatever you are thinking about. Blank it out.**

Allow silence for around 5 seconds

What image comes to mind now?

If the participant say "no image", record response as 'no image' and then encourage them to "take as long as you need and just let me know the first image that comes to mind".

If the participant describes things in the room, instruct them to “try and turn your attention ‘inwards’ to what is in your mind, not what is in the room. What image comes to mind when you do this.”

Which of the following statements best describes this image? A, B, or C?

Experimenter hands the participant the response statements and points to the appropriate statements. Experimenter then records the statement selected

If the participant selects more than one statement, ask them to clarify which statement is most accurate.

I would now like to ask what was going through your mind while you were performing the task a moment ago. Which of the following statements best describes your experience DURING the task? You can select more than one option.

*** Experimenter points to the appropriate statements. Experimenter then records the statement selected ***

If the participant selects C, ask, “Approximately how many new images did you experience”? Record their response on the excel spreadsheet.

If they do either of the following, ask them to explain their experience. Explain the discrepancy and then ask them to read the statements again and select which statements best capture their experience during the task:

- *Participant selects both A and B (they should pick one or the other)*
- *Participant selects D as well as another letter (they should only choose D on its own)*

I would now like to return to the target image.

Please face the screen and keep your eyes open. Now, bring the image of *say image title* to mind. Let me know when you have it in mind by saying “YES”.

*** Experiment waits for the participant to confirm they have the image in mind”***

How vivid is the image of the memory in your mind right now on a scale of 0 to 10, 0 being ‘no image at all’ and 10 being ‘image as clear and as vivid as real life’?

Experimenter records the participant’s reponse

How emotional is this image in your mind right now on a scale of 0 to 10, 0 being ‘neutral’ and 10 being ‘as bad as if it were happening’?

Experimenter records the participant’s reponse

Ok, you can stop focusing on the image.

We will now repeat the procedure. Again, we will start by asking you to mentally focus on the target image. Then I will ask you to look at the dot on the screen. After a while, we will stop and I will ask you some questions. **As before, just notice what happens, without trying to influence it or judge whether it should be happening or not.**

Repeat from “**START OF TASK***

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END OF EXPERIMENT:

Once all experiment blocks have been completed, thank the participant for their time, provide the debrief form and ask for any questions or concerns about the experiment.

Example Memories

- Being involved in/seeing an accident
- Experiencing/seeing a raid/burglary
- A broken relationship
- A fight with an acquaintance/relative/friend
- Your parents' divorce
- Being rejected, for instance after a job application
- Experiencing a certain disease yourself or being faced with the illness of an acquaintance/relative/friend
- Experiencing a medical intervention
- The death of an acquaintance/relative/friend
- Experiencing a natural disaster
- Experiencing extreme weather conditions
- Having a severe mental episode

Appendix C

Below are the response statements used in experiment 6.

RESPONSE STATEMENTS

Which statement best describes the first image that comes to mind ***after*** the task:

A) I get an image of the negative incident. It is identical in every way to the target image I had in mind at the start of the task.

B) I get an image of the negative incident, but aspects of the image are different to the target image I had in mind at the start of the task (e.g. it is more/less detailed, closer/further away, quieter/louder, it relates to a different part of the incident).

C) I get an image of something other than the negative incident.

Which statements best describe your experience ***during*** the task:

A. I experienced the target image. All aspects of the image stayed the same while it was in my mind.

B. I experienced the target image, but aspects of the image changed while it was in my mind (e.g. it became more/less detailed, closer/further away, quieter/louder, it changed to a different part of the incident).

C. I experienced at least one mental image of something other than the negative incident.

   Approximately how many of these mental images did you experience?

D. I did not experience any mental images.