Media Exposure to Trauma, Psychotherapy, and False Memories: A Recipe for Disaster?

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Abstract

The proliferation of social media use in recent years has meant individuals are at increased risk of being exposed to images of real-life violence and trauma. This issue, along with growing concerns about the inaccessibility of formal psychological therapy, raises questions about the demand for computer-delivered therapeutic interventions. However, previous research has raised concerns about the potential for some therapeutic techniques to increase susceptibility to misinformation (e.g., Houben et al., 2018). In this thesis, I aimed to test the effects of a computerised trauma intervention on trauma memory vividness, emotional intensity, and susceptibility to misinformation. Experiment One describes novel procedures and materials for investigating misinformation effects in an online context. Participants ($N = 99$) completed the study online. They first watched a 10-minute video of a fictional school shooting. Between five and ten days later, they were randomly assigned to receive misinformation or no misinformation about the video before completing a recognition test. Misinformed participants were less accurate at discriminating between misinformation and true statements than control participants. This effect was most strongly supported by ROC analyses (Cohen's $d = 0.59$, $BF_{10} = 8.34$). The study showed the misinformation effect can be established in an online experiment using candid violent viral-style video stimuli. The novel materials developed in Experiment One were employed in a second experiment to test the misinformation potential of Cognitive Bias Modification – Appraisal (CBM-App) training; a computerised trauma intervention. In Experiment Two, participants viewed the school shooting video and rated the vividness and emotionality of the video. They then received a post-trauma debrief via video before being randomly assigned to either complete the CBM-App training intervention or a control task. Participants again rated their memory vividness and emotionality. Five to ten days later, all participants re-rated their memory vividness and emotionality and then received misinformation about the trauma video. Lastly, participants completed a recognition test. Results showed CBM-App training successfully instilled a positive cognitive bias; however, the intervention had no effect on trauma memory vividness or emotionality. ROC analyses also demonstrated no effect of CBM-App training on susceptibility to misinformation. The present research raises questions about the efficacy of CBM-App training for reducing trauma-related distress. Moreover, findings suggest that while there is the potential for memory distortion in many therapeutic interventions, this may not be the case for CBM-App training. This research has implications for the CBM-App, misinformation, and trauma literature. It is hoped the present research provides a foundation for further research investigating therapeutic interventions and misinformation effects in an online trauma context.

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CHAPTER 1

Introduction

On the 15th of March, 2019, 51 people were shot dead and 49 were wounded during a terrorist attack at two mosques in Christchurch, New Zealand (Christchurch City Council, 2019). The impact of this tragedy was far-reaching, with graphic live-streamed videos of the attacks appearing across multiple social media platforms, and more than 1.5 million video uploads to Facebook in the first 24 hours (Besley & Peters, 2019). New Zealand media coverage of the event through television, radio, and internet was ubiquitous and continuous. Such prolific up-to-the-minute coverage of unfolding events often includes inaccurate reports based on incomplete information, rumours, speculation, and mistaken details (Rapp & Salovich, 2018; Rich & Zaragoza, 2016). For example, during the Christchurch mosque attacks it was erroneously reported that 'a good guy with a gun' stopped the shooter. In fact, the man had rushed at the shooter with an electronic payment device (Cooke, 2019). Spread of this type of erroneous information can lead to memory errors, persistent false beliefs, and can lead people to remember an event as being more traumatic, thereby exacerbating event-related distress (Strange & Takarangi, 2012).

A related but distinct issue is that the proliferation of social media use has meant people are at increased risk of being exposed to potentially traumatic events and erroneous information about real-world events

(Peterson & Densley, 2017; Redmond et al., 2019). Exposure to misleading information can infiltrate and distort our memory of an event; this is known as the misinformation effect (Loftus, 2005). In addition to the impact on someone personally, in terms of exacerbating their levels of distress, misinformation effects are problematic because they reduce the reliability of our memory in forensic eyewitness contexts (Loftus, 2018). Moreover, once exposed to misinformation, it is extremely difficult to correct or negate the information (Chan et al., 2017; Swire et al., 2017).

Some researchers argue that memory for traumatic or distressing events are more vulnerable to misinformation effects (Knott et al., 2018; Porter et al., 2008; Strange & Takarangi, 2015). In addition, there is some evidence to suggest certain therapeutic techniques used to reduce trauma-related distress may increase susceptibility to misinformation (Houben et al., 2018). However, research in this field is divergent, with subsequent replications failing to find any misinformation effects (Calvillo & Emami, 2019; Sievwright, 2018; van Schie & Leer, 2019).

One explanation for the failed replications is that existing methods and materials for instilling misinformation effects in experimental research may not be particularly well-suited to investigating misinformation effects in the context of real life trauma and therapy. This is because traditional stimuli used to instil misinformation effects are often not designed to evoke strong emotional responses analogous to real-life traumatic events. The present research aims to address possible methodological issues through the development and testing of novel misinformation stimuli and procedures. The novel materials and procedure will then be used in a second study that aims to clarify whether a computer-administered trauma intervention can reduce the vividness and emotional intensity of traumatic memory without increasing susceptibility to misinformation.

The literature review for this research is divided into two chapters; part one discusses the issue of media exposure to trauma, the nature of memory for traumatic events, and introduces the theoretical and experimental underpinnings of the misinformation effect. Part two highlights the issue of false memories in therapy—with particular focus on recent conflicting findings. The review concludes with a discussion of the methodological issues relevant to the context of trauma and therapy, and the overarching rationale for the present research.

CHAPTER 2

Memory for Traumatic Events and the Impact of Misinformation

Social media has rapidly become part of our everyday lives. More than 3.6 billion people worldwide were using social media in 2020 (Clement, 2020). Although the majority of social media users are everyday law abiding citizens, social media platforms are also increasingly being used by a range of questionable actors such as gangs, extremist groups, and terrorists to incite, coordinate, and broadcast acts of violence (Kydd, 2021; Peterson & Densley, 2017). Furthermore, technological advancements have meant that even the average person is able to record and broadcast violent events to the world as they unfold—without the editing and censorship that would normally occur with traditional news media coverage of such events. As a result, there is an increasing risk for people to be inadvertently exposed to disturbing images as they go about their daily social media use. This chapter reviews the literature surrounding the impact of media exposure to traumatic events in terms of psychological distress and memory. The issue of the dissemination and exposure to misinformation about traumatic events is also discussed.

Media Exposure to Potentially Traumatic Events

Approximately 75% of people will experience at least one potentially traumatic event (PTE) during their lifetime (Benjet et al., 2016; Goldstein et al., 2016; Kilpatrick et al., 2013; Overstreet et al., 2017). A PTE is defined as exposure to actual or threatened death, serious injury, or sexual violence (DSM-5; American Psychiatric Association, 2013). Exposure to such events can lead to serious mental health and functional impairment, including post-traumatic stress disorder (PTSD); a psychological disorder characterised by symptoms of mental intrusions, avoidance, negative changes in mood and cognition, marked changes in arousal and reactivity, and significant distress (APA, 2013). Although many people experience PTEs, only around one third of people exposed to PTEs experience trauma-related distress (Overstreet et al., 2017). A portion of those who experience trauma-related distress will develop symptoms consistent with PTSD. A subset of this group (around 6.1%) will seek treatment and be diagnosed with PTSD, while others will have symptoms considered too mild to meet criteria for a formal PTSD diagnosis (Goldstein et al., 2016).

An individual may be exposed to a PTE either directly or indirectly. Direct exposure can involve experiencing or witnessing the event first-hand. Indirect exposure can involve learning about a close friend or family member's traumatic experience or experiencing repeated or extreme exposure to aversive details of a traumatic event (APA, 2013). According to the current DSM-5 definition (APA, 2013), exposure to events through media (such as television, movies, and pictures) does not qualify as a traumatic experience unless this exposure is work-related, which may be the case for police officers, internet child exploitation investigators, medical professionals, therapists, journalists, and media content moderators. Research with these groups has shown that media exposure to traumatic content is associated with post-traumatic stress symptoms (known as vicarious trauma or secondary traumatic stress), reduced sense of safety, burnout, sleep disturbances, increased alcohol use, depressive symptoms, and anxiety (Edelmann, 2010; Feinstein et al., 2014; Hurrell et al., 2018; Perez et al., 2010; Smith et al., 2018). A range of coping strategies have been identified as being effective in mitigating the psychological effects of exposure to distressing material among internet child exploitation investigators and law enforcement. These strategies include good nutrition, exercise, engaging in hobbies, using humour, diaphragmatic breathing, talking to social supports, receiving organisational support (including accessing counselling/therapy), using grounding skills, mindfulness techniques, and taking a break from the work environment (Burns et al., 2008; Polak et al., 2019).

However, the impact of traumatic media content is not limited to those who work directly with such content. Recent advances in technology and social media have afforded the public widespread—and often uncensored—access to media coverage of PTEs as they unfold in real time (Holman et al., 2019; Hopwood & Schutte, 2017). Moreover, PTEs, particularly large-scale collective traumas (e.g., mass murders, natural disasters, and terrorist attacks), often receive extensive and repetitive media coverage of graphic footage (Thompson et al., 2019). Although research exploring the impact of media exposure to trauma in the workplace is still in its infancy, even less is known about the impact on viewers from the general public. This is especially concerning given that the general population typically has less knowledge of effective coping strategies and poorer access to relevant support for managing distress than individuals employed to work with traumatic material.

The issue of media exposure to PTEs has become particularly salient due to online streaming of violent media. A domestic example of this global phenomenon is the Christchurch mosque attacks, where in March 2019, 50 people were shot dead and dozens were wounded by a terrorist at two mosques in Christchurch, New Zealand (Christchurch City Council, 2019). The impact of this tragedy was felt far beyond the local community, with graphic live-streamed videos of the attacks appearing internationally across multiple social media platforms. The event represented an unparalleled display of mass violence and terror through media, and has sparked ongoing debate regarding the role and responsibilities of social media corporations in such instances (Baccarella et al., 2020), culminating in the New Zealand government's international accord, *Christchurch Call*, to eliminate terrorist and violent extremist content online (*Christchurch Call*, 2019).

Extant research on media exposure to PTEs has largely focused on traditional media forms, where violent or distressing events have been depicted through television, movies, radio, or newspaper coverage (Houston et al., 2018). However, in the past two decades *social* media has played an increasingly significant role in the propagation of information globally (Monfort & Afzali, 2017). Social media refers to a multitude of websites and internet applications that facilitate social communication and networking between individuals from all over the world (Kapoor et al., 2018). Some of the most commonly recognised social media sites include Facebook, Instagram, Reddit, YouTube, LinkedIn, and Twitter. These internet platforms enable users to develop public or semi-public profiles, connect with other users, and view, share, and contribute information about an infinite array of topics (Kapoor et al., 2018; Monfort & Afzali, 2017). Consequently, violent content on social media has the potential to reach and affect vast audiences at unprecedented speed.

Accurate estimates of the prevalence and frequency of media exposure to PTEs are scarce. This is due to several factors; first, there is a lot of ambiguity surrounding the definition of trauma in an online context, with many academics debating whether media exposure can be considered a traumatic experience (Hopwood & Schutte, 2017). Second, measures of media exposure to PTEs are inconsistent. For example, some studies ask participants to quantify the number of hours spent viewing PTEs during a specified timeframe, while some may use rating scales to enquire about frequency of viewing more generally (Felix et al., 2019). Third, individuals may be reluctant to report viewing violent content online due to the stigma associated with viewing extremist material and the threat of legal ramifications associated with objectionable materials. For example, shortly following the 2019 Christchurch Mosque attacks, it was deemed a criminal offence in New Zealand to access, possess, or distribute the video of the attacks (Department of Internal Affairs, 2019).

A recent study by Redmond et al. (2019) provides some indication of the extent of online exposure to traumatic media content. In a nationally representative sample of Americans, 25% of participants reported watching all or part of a beheading video created by the terrorist organisation Islamic State of Iraq and Syria (ISIS). The most common reasons reported for watching this graphic content were to gain information and verify the video's authenticity (55%), or to satisfy curiosity (24%; Redmond et al., 2019). In line with this, research suggests seeking information following a public crisis may be a rational response for reducing anxiety and uncertainty (Lachlan et al., 2009; Thompson et al., 2019). Although some people may actively seek out graphic content, circulation of violent content through social media increases the potential for people to be inadvertently exposed to material they may otherwise avoid (Holman et al., 2019). Given the proliferation of media consumption and increased access to distressing material online, it is likely that exposure to PTEs is a much more prevalent issue than currently recognised.

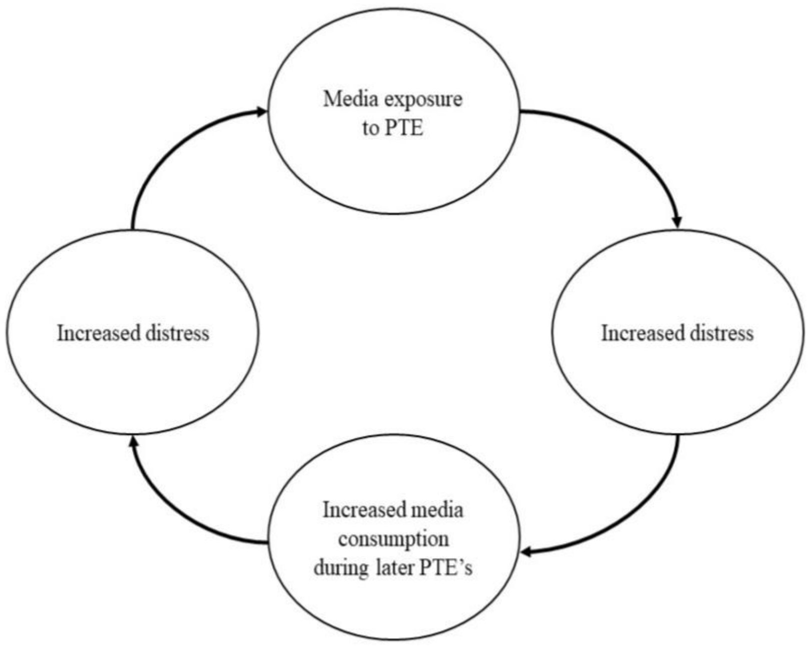
Effects of Media Exposure to PTEs

Attention was first drawn to the potential psychological impact of media exposure to PTEs after the Oklahoma City bombing in 1995 (Houston et al., 2018). Early research suggested greater exposure to media coverage of the Oklahoma City bombing was linked to negative mental health outcomes, including posttraumatic stress symptoms and functional impairment (e.g., Pfefferbaum et al., 2000, 2001). Additionally, some individuals (who reported no direct exposure to the bombing) experienced negative outcomes that were attributable to media exposure to the event (Pfefferbaum et al., 2003). These initial studies provided the first evidence that indirect exposure to PTEs through media may be just as impactful as directly experiencing them, with subsequent studies providing additional support for this position. Research using real-life traumatic images suggests viewing such material can elicit the same physical and psychological symptoms experienced by direct victims of trauma, including posttraumatic stress disorder (PTSD), depression, and anxiety (Pfefferbaum et al., 2019). A number of studies have established an association between the number of hours spent viewing media coverage of disastrous events and posttraumatic stress symptoms (Fallahi & Lesik, 2009; Holman et al., 2014, 2019; Otto et al., 2007; Silver et al., 2013). For example, Silver et al. (2013) found watching four or more hours of 9/11-related television coverage per day predicted a 51% increase in the likelihood of reporting high acute stress two-to-three years after the 9/11 terrorist attacks. Similarly, Holman et al. (2014) observed that six or more hours of daily media exposure to the Boston Marathon bombings was associated with higher acute stress symptomatology than was direct exposure to the event.

A meta-analysis by Pfefferbaum et al. (2019) attempted to establish an overall effect size for the association between media exposure to mass trauma and posttraumatic and acute stress symptoms across 43 studies. Small but significant correlations were found for both posttraumatic symptoms (r = .17) and acute stress reactions (r = .26). However, the authors noted most research cannot make any causal claims regarding the effects of media exposure due to the use of cross-sectional designs. While it is possible media exposure to PTEs produces posttraumatic stress reactions, it may also be that individuals with post-trauma symptoms seek more media coverage in attempt to cope with uncertainty and anxiety (Pfefferbaum et al., 2019).

The prospect of a bi-directional relationship between media exposure and psychological distress has been explored in a recent study by Thompson et al. (2019). They conducted a longitudinal study over three years with a national sample of US residents (N = 4,165). Data were collected at four time points, including in the days following two mass violence events: the 2013 Boston Marathon bombings and the 2016 Pulse nightclub massacre. Results showed media exposure to the Boston Marathon bombings was associated with posttraumatic stress symptoms six months later, which, in turn, was associated with subsequent worry about future events. This distress predicted subsequent amount of media exposure and distress reactions to the Pulse nightclub massacre. Thompson et al. (2019) concluded that, indeed, media exposure to PTEs may fuel a cycle of distress (as illustrated in Figure 1), whereby exposure may leave some individuals vulnerable to developing symptoms of distress, and this distress may trigger greater media consumption during later events.

Figure 1.
Cycle of distress arising from media exposure to PTEs, proposed by Thompson et al. (2019)



Holman et al. (2019) set out to further clarify the effects of media exposure on psychological distress and functional impairment by investigating whether these effects may be explained by the amount of exposure or the extent of image graphicness. Results demonstrated a small direct independent association for the quantity ($\beta = 0.16$) and a very small association for the graphicness (β = 0.06) of media coverage with distress and functional impairment following the Boston Marathon Bombings. The authors posited image graphicness may serve to increase vivid visual imagery associated with intrusion symptoms of PTSD. Although only small effects were found, this study provides some preliminary evidence that both the amount and quality of media exposure play a role in the development of event-related distress. Therefore, the highly accessible and uncensored nature of graphic material on social media may have a particularly powerful impact on people's psychological distress.

The relationship between media exposure to PTEs and psychological distress can also be explained through the lens of cultivation theory (Gerbner & Gross, 1976). Cultivation theory suggests that media, particularly television, significantly shapes our understanding of social reality (Gerbner & Gross, 1976; Morgan et al., 2015). Applied to PTEs, cultivation theory proposes that intense media coverage of PTEs, such as natural disasters or terror attacks, may influence people to perceive the world as more dangerous than it is in reality (Felix et al., 2019; Houston et al., 2018). Exaggerated perceptions of danger and threat can subsequently lead to heightened distress that is disproportionate to the actual level of risk associated with the traumatic event. This notion has received widespread support, with findings demonstrating associations between television media consumption and inflated senses of danger, crime rates, and likelihood of victimisation (for a review see Morgan et al., 2015). From this perspective, psychological distress can be considered partly the product of a disproportionate sense of threat and danger arising from sensationalised media coverage of PTEs. Moreover, when people feel threatened they seek control over their environment

– including trying to predict more threats and avoiding situations perceived as threatening.

While most studies have asserted that media exposure to PTEs can evoke post-traumatic stress symptoms, some researchers have raised questions regarding the severity of symptoms experienced following media exposure to trauma. For example, a study by Monfort and Afzali (2017) found no association between traditional or internet media consumption and psychopathological symptoms. Although social media consumption was associated with cognitive and emotional distortions and psychological distress, there was no link to any other PTSD symptom clusters. Therefore, the authors concluded that media exposure to PTEs cannot be considered a traumatic experience.

Lack of acknowledgement of media exposure as a legitimate source of trauma has significant implications in terms of access to mental health services. If people's distress is disregarded due to its source, this will likely prevent people from receiving the support they need. However, even if symptoms do not meet clinically-diagnosable levels, the distress experienced by some individuals warrants attention. This issue was highlighted in an unpublished study by Thompson (2015), which investigated reports (n= 1,609) of people's worst life experiences one year after the 9/11 terrorist attacks. Despite none of the participants directly experiencing the 9/11 attacks, 21% of the sample reported 9/11 as their worst life experience, even when they had experienced other significant PTEs. Importantly, Thompson (2015) also found that watching at least four hours of 9/11-related television coverage in the week following the attacks was associated with an 87% increase in the odds of endorsing 9/11 as the worst life event. Overall, this suggests media exposure can have significant and lasting impact on people; sometimes perhaps even more so than directly-experienced PTEs.

In summary, media exposure to real-life traumatic events can result in significant psychological distress. Most of the extant research in this area has focused on news representations of PTEs, which are often heavily censored to protect viewers from the most graphic details. However, such censoring is less likely to occur in social media depictions of PTEs. Moreover, live-streamed events may more closely resemble the experience of directly witnessing a traumatic event. Given the proliferation of social media use and recent incidents of mass trauma depicted on social media platforms, there is a clear need for empirical research on the impact of this type of exposure to trauma.

Trauma Film Paradigm

One way to investigate effects of media exposure to PTEs is using the trauma film paradigm. Since exposing participants to real-life traumatic events is unethical and limits experimental control, researchers use laboratory analogues of traumatic events to model posttraumatic stress reactions. In studies using the trauma film paradigm, participants watch a video clip depicting fictional aversive content consistent with the DSM-5 definition of a traumatic event, such as a violent car crash or physical or sexual violence (Arnaudova & Hagenaars, 2017; James et al., 2016; Weidmann et al., 2009). The purpose is to temporarily induce mild psychological and physiological symptoms of posttraumatic stress; analogous to symptoms experienced in reaction to actual trauma (James et al., 2016). Studies using the trauma film paradigm consistently produce analogue symptoms of posttraumatic stress, including intrusive memories of film content, increased heart rate, intense emotional responses, and distress (James et al., 2016; Weidmann et al., 2009). Intrusive memories are considered a key mechanism in maintaining and exacerbating a person's distress following a traumatic event (Ehlers & Clark, 2000; Iyadurai et al., 2019). Memory intrusions are sensory-perceptual experiences, often visual images, that come to mind involuntarily and often elicit negative emotions and physiological sensations (Ehlers et al., 2004; Iyadurai et al., 2019). The ability to instil intrusive memories at a sub-clinical (below diagnostic) level using a trauma film paradigm provides useful insight regarding the factors that may moderate a key symptom of PTSD.

Despite widespread use and strong evidence of the effects of trauma films, the paradigm has been criticised for a lack of ecological validity in terms of what is clinically considered 'real' trauma. However, as James et al. (2016) argue, the addition of indirect trauma exposure through electronic media in the DSM-5 indicates a shift in what is understood to cause clinically significant distress and impairment in the modern world. Moreover, the trauma film paradigm closely simulates the experience of viewing real-world traumatic material through social media—a key focus of this research.

The Nature of Memory for PTEs

The trauma film paradigm may be a valid method for exploring the nature of memory for traumatic events. Memories for emotionally charged events are often rich and powerful. Growing evidence suggests negative emotions associated with PTEs may enhance event memory (Congleton & Berntsen, 2019; Kensinger & Ford, 2020). Despite this, negative-valence memories have also been found to be more susceptible to distortion than neutral or other emotional memories (Bookbinder & Brainerd, 2017; Knott et al., 2018; Porter et al., 2008; Strange & Takarangi, 2015). Even details of highly publicised impactful events, such as 9/11, can be forgotten over time, with research showing that most 9/11-related forgetting occurred in the first year following the attacks and stabilised thereafter (Hirst et al., 2015).

Arguments about the nature of trauma memories disagree on whether trauma memories are more disorganised and fragmented than other autobiographical memories. On the one hand, research with trauma victims tends to support the memory fragmentation hypothesis. Findings show trauma victims often have difficulties recalling the temporal order of events and specific details related to the most distressing moments of the traumatic event (Ehlers et al., 2004; Foa et al., 1995). Trauma memories also tend to be recalled with repetition, speech fillers, and incomplete thoughts characteristic of fragmented memory (Ehlers et al., 2004). On the other hand, researchers arguing against the memory fragmentation hypothesis claim that traumatic events are remembered very well and are rarely forgotten. They argue the intense emotions associated with traumatic events, the impact of the event on one's sense of identity, and frequent recounting of the traumatic experience all serve to strengthen trauma memory (Berntsen et al., 2003; Berntsen & Rubin, 2006). Others posit that traumatic memories are processed using the same mechanisms as any other memory and are therefore no more vulnerable to fragmentation or incoherence than other memories (Bedard-Gilligan et al., 2017; Taylor et al., 2020, 2021).

For example, Taylor et al. (2020) found subjective feelings of coherency were reduced when participants were asked specific questions about their memory compared to simply describing the event, regardless of whether the memory was traumatic or not (Taylor et al., 2020). In line with this finding, it is possible that probing questions about traumatic memories in therapy or forensic interviews may reduce perceived memory coherence. This may, in turn, increase reliance on post-event information. In other words, if people are less trusting of their own account of an event (due to perceived incoherence), they may be more likely to rely on external sources of information, which may or may not be accurate. Therefore, even if traumatic memories are as coherent as other memories, people's subjective feelings of incoherence when questioned about their memory may leave traumatic memories vulnerable to distortion.

Although emotions associated with traumatic events can enhance trauma memories, this enhancement is not global. Emotional arousal may enhance central aspects of a trauma memory, such as the weapon involved in an assault, but these same processes may weaken memory for peripheral or semantically-unrelated details (Kaplan et al., 2016; Kensinger & Ford, 2020). Moreover, Bisby et al. (2020) suggest negative events may be encoded in a way that can strengthen negative content, but can disrupt the processing of the associative structure of an event, leading to later poorer holistic recall. Therefore, while some aspects of trauma memory are well-remembered, other aspects can be easily forgotten and may be more vulnerable to distortion.

A major issue with fragmented memory is that humans have a natural tendency to fill in the blanks to create a 'complete' memory or narrative (Foa et al., 1995; Magliano et al., 2017). Sometimes the information we use to fill in the blanks in our memory is inaccurate, leading to the development of a *false memory*. False memories typically refer to recollections of events or parts of events that never occurred or recalling details of the event incorrectly. For example, Thomas and Loftus (2002) showed that people can develop false memories of performing bizarre actions that they never actually performed, such as kissing a magnifying glass or sharpening a shoelace with a pencil sharpener. Unlike mere lying, the rememberer of a false memory genuinely believes that the non-experienced event actually occurred (Zhu et al., 2010).

One way in which false memories may develop for traumatic events is through the memory amplification effect (Oulton et al., 2016). The memory amplification effect facilitates remembering a traumatic event as being more traumatic than it actually was. For example, in Strange and Takarangi's (2012, 2015) experiments, participants reported seeing 26% of clips that had actually been removed from the video shown to participants. Similarly, Gerrie et al. (2006) showed that participants confidently, but incorrectly, remembered seeing non-crucial details that were missing from the video shown to participants. They reasoned details that are more easily imagined are more likely to be confused with real details from the event, leading to acceptance of imagined details as part of the original recollection.

Another common way false memory may develop for traumatic events is through the misinformation effect. Often wide-scale traumatic events, such as terrorist attacks, mass murders, and natural disasters are highly publicised through news and social media. Up-to-the-minute coverage of unfolding events often includes inaccurate reports based on incomplete information, rumours, speculation, and mistaken details (Rapp & Salovich, 2018; Rich & Zaragoza, 2016). For example, misinformation spread rapidly in 2011 after the U.S. National Public Radio mistakenly reported that Arizona Congresswoman, Gabrielle Giffords, had died in a shooting incident (Shephard, 2011). Exposure to misinformation about an event can infiltrate and distort memory for the original event; this is known as the misinformation effect.

The Misinformation Effect

Misinformation effects occur when misleading post-event information becomes accepted as part of an individual's original recollection of an event (Loftus, 1975). The impact of misinformation is particularly salient in legal settings, where eyewitness accounts of distressing events are frequently relied upon to determine the guilt or innocence of a suspect. Thus, misinformation effects can have very real, harmful consequences.

The harmful effects of post-event misinformation on memory have been demonstrated across decades of research. Early laboratory experiments by Loftus (1977) showed that participants exposed to misleading post-event information more often reported seeing a categorically incorrect colour vehicle after viewing a video clip of a car accident than participants not exposed to such misinformation. Others have shown participants exposed to misinformation reported false memories of seeing a thief using a hammer instead of correctly identifying the tool as a screwdriver (McCloskey & Zaragoza, 1985), and others reported seeing the Eiffel Tower in a wall hanging instead of the Leaning Tower of Pisa they actually saw (Takarangi et al., 2006). Misinformation effects have also been demonstrated with real-world traumatic events. For example, 73% of participants incorrectly reported seeing video footage on the day of the 9/11 attacks of the first plane crashing into the first tower on September 11, 2001, when this footage was not broadcast until a later date (Pezdek, 2003). Similarly, Ost et al. (2002) found 44% of participants claimed to have seen non-existent footage of the car crash in which Diana, Princess of Wales, was killed.

Misinformation Paradigm Within laboratory research, the misinformation effect is studied using the misinformation paradigm, developed by Loftus, Miller, Burns, and Hoffman (Loftus, 1977; Loftus et al., 1978; Loftus & Hoffman, 1989). The misinformation paradigm involves three key stages, as shown in Figure 2; first, participants witness a target event, often through a video or slides depicting an event. Second, after a delay (typically ranging from five minutes to one week) participants are exposed to a written or audio fictional eyewitness account of the target event which contains misinformation about details from the event. Finally, participants are tested on their memory for the original event details, often immediately after exposure to misinformation or following a short delay (typically ranging from five to twenty minutes). A misinformation effect occurs when participants endorse misinformation presented in the eyewitness account as part of their original recollection of the target event. Participants' endorsements of misleading details at test is analogous to misinformation effects that may occur in clinical (Patihis & Pendergrast, 2019) and legal settings (Loftus, 2018).

Figure 2.
The three-stage procedure for the misinformation paradigm.

Since its initial development, this three-stage procedure has been used in many studies to instil misinformation effects. For example, Zhu et al. (2012) employed a misinformation paradigm to investigate the long-term effects of brief exposure to misinformation. In their study, participants viewed two separate events, each presented through 50 digital colour slides. The first event depicted a man breaking into a car and stealing items, and the second event depicted a girl's wallet being stolen. Twelve of the slides for each event were "critical slides" that would be described inaccurately in the misinformation narrative phase. Thirty minutes after viewing the slides, participants read two narrations describing the two events they previously witnessed. For each event, the narration contained 12 inaccurate descriptions (misinformation). Ten minutes later, participants completed a recognition test of their memory for the slides. Each question had three possible response options, including the original item from the slides, a misinformation item from the narrative, and a new foil item. Participants then completed a follow-up test 1.5 years later. Participants were shown the slides again, but with the 12 critical slides missing from each event. Participants then described what they remembered from the missing slides. Results showed that misinformation effects were produced using this paradigm, and persisted for a 1.5 years after exposure to misinformation.

Over time, many different versions of the misinformation paradigm have been developed. For instance, Takarangi et al. (2006) created a misinformation paradigm using digital technology to resolve counterbalancing issues that existed in previous video formats. In their study, participants were randomly assigned to watch one of two video clips of a tradesman rifling through personal belongings and stealing items while working on an electrical job. The two video clips were identical except for 11 critical items that were digitally manipulated (e.g., a baseball cap the tradesman wore was *blue* in one clip and *black* in the other clip). Twelve minutes after viewing the video clip, participants read one of four narratives describing the events depicted in the video clip. The four narratives were identical except for how the eight critical items were described. For each narrative, participants read misleading information about four critical items (misled items) and neutral information about the remaining four items (control items). Three minutes later, participants completed a recognition test about items or events from the video clip with two response options for each item. Using this procedure, Takarangi et al. (2006) demonstrated a large misinformation effect (Cohen’s $d = 0.89$), whereby participants were less accurate about misled items than control items. This effect was also replicated in a second experiment (Cohen’s $d = 1.23$).

In summary, the misinformation paradigm is a valuable tool for investigating the factors that influence misinformation effects in the laboratory. Findings from misinformation studies have been applied to real-world forensic and legal settings to inform police questioning practices and highlight the fallibility of eyewitness testimony (e.g., Loftus, 2018). The present study employs novel misinformation materials and procedures to explore misinformation effects in the context of traumatic events witnessed through the media—an issue which has received little attention to date within the false memory literature.

Encoding, Storage, and Retrieval of Autobiographical Memories

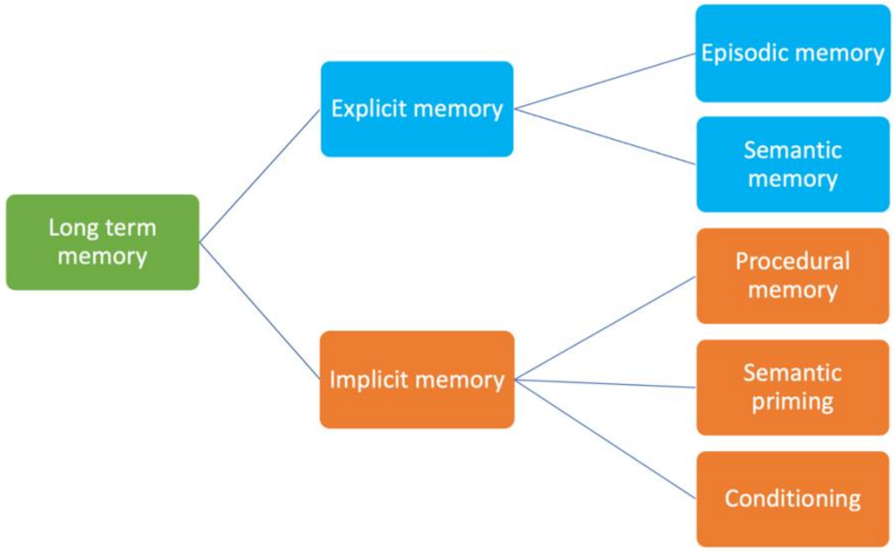
To understand how misinformation effects occur we must first understand the general processes involved in memory. Although many theories of memory exist, it is generally agreed there are three key stages: encoding, storage, and retrieval (Schoenberg & Scott, 2011). Encoding describes the processes involved from the initial perception of information through to storage in long-term memory. When an event occurs, sensory information (such as what we can see, hear, smell, feel, and taste) is processed during the first milliseconds of perception. Depending on a range of processes (including attention and conscious selection), sensory information will be transferred to short-term (working) memory, otherwise it will rapidly decay and fail to be stored in memory (Schoenberg & Scott, 2011). Short-term memory has a limited capacity and information is typically quickly forgotten unless it is consolidated into long-term memory through a process such as organisation or rehearsal. Information consolidated into long-term memory is relatively enduring and can usually be retrieved voluntarily unless a form of forgetting occurs (Baddeley et al., 2015; Schoenberg & Scott, 2011).

Long-term memory comprises an assemblage of interconnected systems; each responsible for organising, storing, and retrieving distinct types of information (Friedman & Johnson, 2000). Long-term memory can be divided into two main categories: implicit memory and explicit memory (Schacter et al., 2000). The subtypes of long-term memory are depicted in Figure 3. Implicit memory typically refers to a range of memory processes involving nonconscious and unintentional retrieval (Dew & Cabeza, 2011). Implicit memory is further divided into procedural memory, semantic priming, and conditioning (Bauer & Dugan, 2020). Procedural memory refers to memory for learned skills and procedures, such as riding a bike. Semantic priming is a memory process where prior exposure to a stimulus influences subsequent response to a related stimulus; for example, exposure to positive self-statements may positively influence later responses to personal questions. Lastly, conditioning refers to a process where two typically unrelated stimuli become associated with each other through repeated pairing; for example a child may learn to associate a nursery rhyme with sleep (Bauer & Dugan, 2020).

Explicit memories, on the other hand, are primarily retrieved through conscious recollection and can be further divided into two sub-types: episodic memory and semantic memory (Schoenberg & Scott, 2011). Episodic memory typically refers to memory for personally experienced or autobiographical events (Bauer & Dugan, 2020). Semantic memory represents general knowledge or facts about the world, such as knowing the capital city of New Zealand is Wellington. Since PTEs are autobiographical events, the present research is primarily concerned with episodic memory.

Figure 3.

Subtypes of long-term memory



Recollections of traumatic events, including details of the scene and sequence of events, generally rely on autobiographical memory systems. Unlike semantic memory (memory for fact-based general knowledge), autobiographical memory positions the rememberer within the context of the event as an observer or actor (Mayes & Roberts, 2001; Tulving, 2002). Tulving (2002) likens autobiographical memory to mental time travel; whereby the rememberer is able to travel back in time inside their mind to an event from their past. Importantly, autobiographical memory differs from a video camera in that only some aspects of an event are encoded and only a portion of events will be stored in long-term memory (Schacter, 1999; Simons & Chabris, 2011). Which details are encoded into memory largely depends on how attention is deployed at the time of the event (Mayes & Roberts, 2001).

Retrieval of memory traces can occur through recall or recognition, both of which involve different processes. Recall is a model of memory retrieval that requires specific information to be produced (Schacter et al., 2000). There are two types of recall: free recall and cued recall. Free recall requires retrieval of an entire memory trace without explicit cues, whereas cued recall requires retrieval of more specific information that is directed by additional cues (Schacter et al., 2000). Recognition differs to recall in that it involves decision-making processes that are generally based on a sense of memory strength, rather than specific recollection. In a recognition test, a person must discriminate between previously encountered stimuli (signals) and new stimuli (noise). The present research focuses on recognition memory because we are concerned with people's ability to discriminate between original event information (signals) and post-event misinformation (noise). Misinformation effects occur when noise (post-event misinformation) is incorrectly judged at test as being a signal. The strength of misinformation effects can be estimated by quantifying how well these signal processes stand out from noise processes. This principle is discussed further in the following section on signal detection theory.

Signal Detection Theory of Recognition Memory. Understandings of recognition memory processes fall under two main schools of thought: single-process and dual-process theories. Single-process theory of recognition memory posits that recognition decisions are based solely on memory strength—a sense of 'knowing' an item was previously encountered (Baddeley et al., 2015; Wixted, 2007). Alternatively, dual-process theory holds that recognition decisions involve memory strength as well as a more complex process of recollection. Unlike decisions based on memory strength, recollection requires retrieval of specific details of the original memory trace (Wixted, 2007).

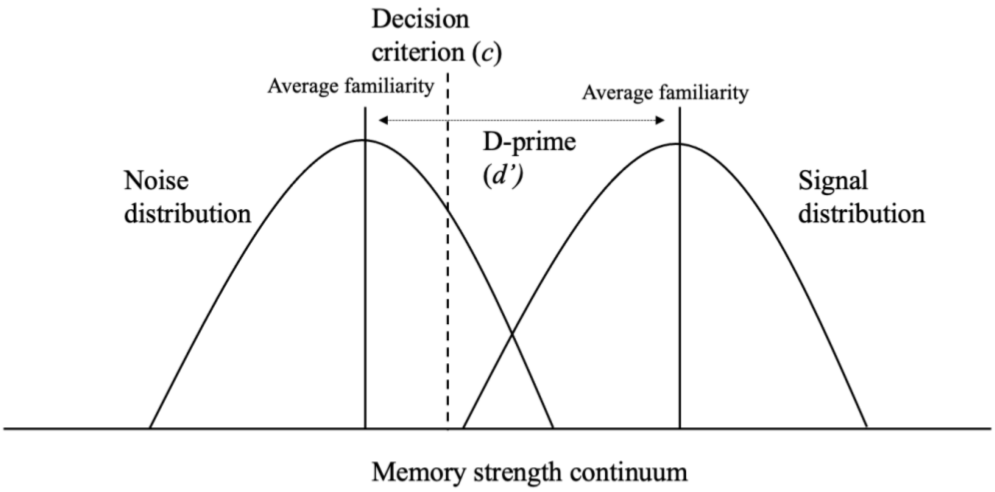
A popular theory that can account for both single-process and dual-process views of recognition memory is signal detection theory (SDT; Green & Swets, 1966). SDT has been applied across a wide variety of psychological domains as a theoretical framework for understanding how people make decisions when presented with competing options (Stanislaw & Todorov, 1999). With respect to memory, SDT posits that recognition decisions are based on the strength of previously encountered items (signals) and new items (noise) presented at test.

According to SDT, the strength of signals and noise are each normally distributed, with varying distances between noise and signal distributions representing varying levels of average memory strength (Stanislaw & Todorov, 1999). Ability to discriminate previously encountered items from new items is denoted by the distance between the average strength of noise and signals and is expressed as d-prime (*d'*), as shown in Figure 4. Higher values of *d'* represent a greater ability to discriminate between old and new items (Stanislaw & Todorov, 1999).

Central to SDT is the idea that the strength of signal and noise test items are compared against some decision criterion (Wixted, 2007). Items above the decision criterion are judged to be previously encountered (i.e., old) and items below the criterion are judged to be new. This produces four possible outcomes, as illustrated in Figure 5: hit (correctly judging an old item as old), miss (incorrectly judging an old item as new), false alarm (incorrectly judging a new item as old, i.e., a false memory), and correct rejection (correctly judging a new item as new).

Figure 4.

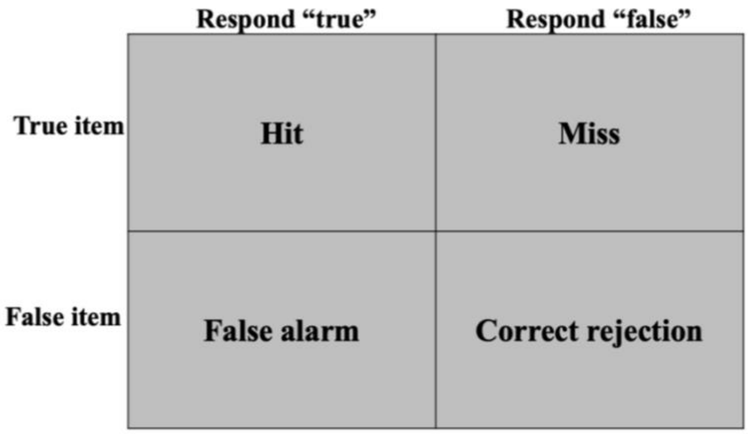
Calculation of d-prime (d') and bias (c) using signal detection theory.



The level of memory strength at which a person sets their decision criterion provides an indication of their response bias, sometimes signified as *c*. A decision criterion exactly in the middle of the noise and signal distributions means an item is equally likely to be judged as old or new (Baddeley et al., 2015; Stanislaw & Todorov, 1999). Decision criterion shifted further to the left of the memory strength continuum (Figure 6a) produces a more liberal response; there is a bias towards judging items as old. Adopting a more liberal criterion leads to a relative increase in hits, with the drawback of also increasing false alarms (Baddeley et al., 2015; Oliver et al., 2008). On the other hand, decision criterion shifted further to the right (Figure 6b) produces a more conservative response, where there is a bias towards judging items as new. Conservative criterion results in a relative decrease in both hits and false alarms (Baddeley et al., 2015; Oliver et al., 2008).

Figure 5.

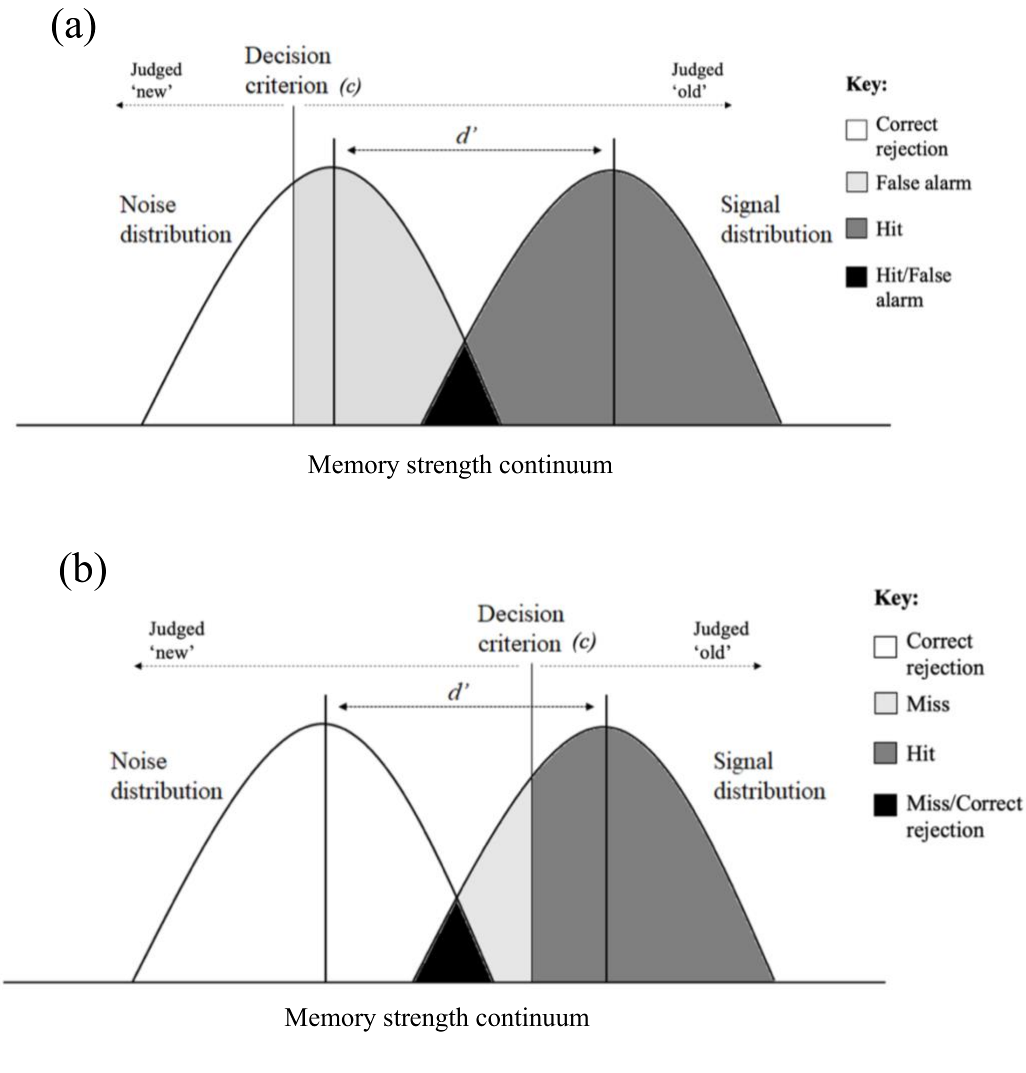
Outcomes of recognition responses for true and false items.



The process described so far represents the single-process view of recognition memory. Dual-process theories build on this explanation by incorporating the process of recollection in their account of recognition decisions. Recollection involves direct retrieval of qualitative information about a specific event (Yonelinas et al., 2010). Dual-process signal detection theory suggests that memory strength is a secondary, back-up process in recognition decisions and that recollection is the primary process (Wixted, 2007; Yonelinas, 1994). Unlike memory strength, which is a continuous variable, recollection is considered an all-or-none high-threshold process where a person will base their recognition decision on recollection only when they are able to retrieve enough information to meet the high threshold (Yonelinas, 1994). If this threshold is not met, memory strength will typically be employed. As a result, decisions based on recollection are often made with high confidence relative to recognition decisions based on memory strength.

Figure 6.

Illustration of hits, misses, false alarms, and correct rejections for: (a) liberal decision criterion, and (b) conservative decision criterion



However, it is important to note that SDT was originally developed as a model of discriminability in psychophysics research (see Wixted, 2020 for a review). Some psychophysics researchers argue SDT does not provide a good model of recognition memory because memory strength does not have the same measurable properties as the phenomena studied in psychophysics, such as discriminability of heaviness, pitch, brightness, and volume. In fact, the concept of 'memory strength' is relatively ill-defined in the literature, creating ambiguity regarding how memory strength is perceived and measured (Brady et al., 2022; Kellen et al., 2021). Despite this potential limitation, SDT has been used to study memory for decades and continues to provide a valuable framework for understanding memory performance.

Signal detection theory can be applied to our understanding of misinformation effects. From this perspective, exposure to post-event misinformation may increase the strength of familiarity associated with misleading details (i.e., noise). This sense of memory strength is interpreted as evidence that noise is actually a signal (i.e., that misleading details were actually part of the original recollection), leading to a false alarm. Combined with a bias toward responding that items were "old", this leads to greater acceptance of "noise" that was not present or that was mistakenly suggested after the event. This perspective of recognition memory is reflected in many theoretical accounts of misinformation effects, which draw upon both single and dual-process views of recognition memory to describe the mechanisms involved in misinformation effects. The next section describes some of the key theories that underpin the effect of misinformation on false memory reports.

Theoretical Underpinnings of Misinformation Effects Several theories attempt to explain the misinformation effect. The most prominent include the source monitoring framework (Johnson et al., 1993), fuzzy-trace theory (Brainerd & Reyna, 2002), and the discrepancy detection principle (Tousignant et al., 1986).

Linear ballistic accumulator models (Brown & Heathcote, 2008) can also be applied to our understanding of recognition decisions in the context of misinformation effects. These theories draw upon the signal detection concepts of memory strength and recollection to describe how misinformation can become integrated into our accounts and influence how we remember events. This section discusses each of these theories and the different proposed mechanisms involved in the misinformation effect.

Source Monitoring Framework. Johnson et al. (1993) proposed that misinformation effects occur due to a failure to accurately judge the source of information that formed the memory; known as a source monitoring error. An example of a source monitoring error would be mistakenly recalling an event you read about in the newspaper as an event that you personally experienced. The source of a memory comprises information that helps to determine the conditions under which the memory was formed, such as spatial, temporal, affective, semantic, and contextual details (Johnson et al., 1993). Source monitoring, therefore, is the process of determining the origins of a particular memory. Determining the origins of a memory helps us make judgements about whether our memory is based on personal experience of a true event, or based on something we imagined, read, or heard about.

There are three main distinctions between source types that can help us to determine the source of a memory. First, we need to be able to discriminate between internally and externally generated information (Johnson et al., 1993). Internally generated information refers to details that are produced through internal thoughts or imagination. On the other hand, externally generated information refers to memories for perceived or experienced events. Second, distinctions need to be made between different external sources. For example, to accurately determine the source of a memory we need to distinguish whether the information came from one person or another, or whether the information came from a personally experienced event or something we read about. The final distinction is between different internal sources, such as distinguishing between memories of what was internally thought and what was said aloud. Although false memories may arise from errors in any of these areas, misinformation effects occur when there is a failure in distinguishing between different external sources (i.e., errors in distinguishing whether information came from the true experienced event or from post-event misinformation).

Brainerd and Reyna (2005) propose that errors in source monitoring are more likely to occur when post-event misinformation and memory for the original event share similar features. Such similarities mean that making distinctions between the original memory and misinformation become particularly difficult, increasing the likelihood that misinformation will be integrated into the original recollection of the event. Brainerd and Reyna (2005) also suggest misinformation effects are more likely to occur when a person recalls insufficient information to be able to accurately identify the source of the memory. This may occur for a number of reasons, including poor encoding of the event to begin with, complications during memory retrieval, or the decay of event memory over time. From a signal detection perspective, in this instance the high threshold for recollection-based decisions would not be met, driving reliance on memory strength processes to determine the memory’s source.

Source monitoring has been widely used to explain the mechanisms involved in the misinformation effect. For example, Frost et al. (2002) tested whether source monitoring errors contribute to misinformation effects using the misinformation paradigm. In two experiments, participants viewed a series of slides, read a misinformation narrative, and either 10 minutes or one week later, completed a recognition test about details from the slides and the source of the details. They found participants were more likely to mistakenly accept misinformation as part of their original recollection after a longer retention interval. Participants also tended to commit more source errors (where misinformation details were more frequently misattributed to the slides) after the one week retention interval than after 10 minutes (Frost et al., 2002). They concluded that the source monitoring framework accounts for their findings well since the associations between event details and source memory tend to fade over time; leading to increased acceptance of misinformation as part of the original event recollection.

Horry et al. (2014) conducted two similar studies using the misinformation paradigm to investigate the effect of source cue availability on confidence-accuracy resolution for misinformation items. Source cue availability was manipulated by asking half of the participants to complete a recognition test immediately (high source cue availability) or after a one-week delay (low source cue availability). In the first study, participants rated how confident they were in each of their recognition answers. They found that confidence-accuracy resolution was higher in the immediate recognition condition than in the delayed condition and was also higher for control items than for misinformation items.

In the second study, the same materials and procedures were used, but participants also completed a source memory test where they judged the source of each test item. Results showed that source memory was significantly worse in the delayed recognition condition than in the immediate condition (Horry et al., 2014). The authors concluded that the confidence-accuracy resolution following exposure to misinformation is affected by the availability of source cues, and source cue availability declines over a longer retention interval. Therefore, the ability to accurately discriminate between misinformation items and original event items declines over time.

Fuzzy-Trace Theory. Fuzzy-trace theory is one of the most recent theoretical accounts of false memories. Fuzzy-trace theory is based on the dual-process account of memory, which assumes that recognition memory comprises two processes; familiarity and recollection (Wixted, 2007). Familiarity is represented by gist memory traces, which store information about the general concept or meaning of an event, such as that you last saw your car keys in the *kitchen*. In contrast, recollection is represented by verbatim memory traces, which store information about item-specific characteristics of an experience (Brainerd & Reyna, 2005, 2002), such as that you last saw your keys on the *kitchen bench* next to the *fruit bowl* at *lunchtime*.

Gist and verbatim memory traces are stored parallel to one another, meaning that specific event details (e.g., kitchen bench, fruit bowl, and lunchtime) are stored and accessed separately to more global and conceptual details (e.g., kitchen). In this way, fuzzy-trace theory closely aligns with a signal detection account of recognition decisions. Memory retrieval may rely on either gist or verbatim traces, depending on the relative strength of one trace over the other (Wixted, 2007). This means that a person can be correct about the general gist of an experience (e.g., that you last saw your keys in the kitchen), but incorrect about specific details of the experience (e.g., exactly where and what time you last saw your keys). Critically, verbatim traces are thought to deteriorate and become inaccessible much faster than gist traces, supporting greater reliance on gist memory over time (Reyna et al., 2016). Much like a target event, post-event misinformation leaves a memory trace; meaning that post-event misinformation can also be retrieved through either gist or verbatim processes. According to fuzzy-trace theory, recognition outcomes are based on various interactions between gist and verbatim traces of the original event and misinformation (Reyna et al., 2016). Correct recognition (or hits) can be supported by either remembering the general gist of details or verbatim recollection of specific details. However, misinformation effects (or false alarms) are supported by reliance on gist traces, and can also be supported by verbatim recollection of post-event misinformation or mitigated by verbatim recollection of the original event (Brainerd & Reyna, 2002; Reyna et al., 2016). Exposure to post-event misinformation may increase the sense of familiarity (or memory strength) associated with incorrect details. Combined with weakened verbatim memory for an event, one may be forced to rely on gist memory and accept misinformation as part of the original event.

Frost (2000) tested whether fuzzy-trace theory could explain misinformation effects by asking participants to indicate whether their memory decisions at test were based on recollection or familiarity. Participants viewed a series of slides depicting a crime, read a narrative containing misinformation about the slides, and completed a recall test of their memory for the slides. For each test item, participants were asked to make a judgement about whether they "remember" or "know" they saw the item in the original slides. "Remember" judgements represented vivid recollection of the items, and "know" judgements represented a sense of familiarity, but no specific memory of the items. Results showed participants were more likely to make "know" judgements for misinformation items and "remember" judgements for original event items (Frost, 2000). This is consistent with the fuzzy-trace notion that misinformation effects are supported by reliance on familiarity/gist memory. They also found participants responded "remember" to misinformation items more frequently after a long delay (1 week) than after a short delay (10 minutes) between misinformation presentation and test. The authors concluded that weakened verbatim *slides* over time led to greater reliance on verbatim *misinformation* memory.

Marche et al. (2010) argue that fuzzy-trace theory may explain differences in the qualitative characteristics of true and false memories. In their study, participants listened to a narrative describing a robbery and then answered some misleading questions about the robbery. Participants rated their remembered details using the Memory Characteristics Questionnaire, which asks about different qualitative details of a memory, such as degree of confidence in the memory and feelings/reactions at the time of encoding (Johnson et al., 1988). They found that qualitative details were richer, and confidence was higher, for true details from the narrative than for gist-consistent suggested details. They also found that qualitative details were richer for suggested details that were more gist-consistent, indicating that gist and verbatim memory traces contribute to phenomenological differences in how details are remembered.

Taken together, fuzzy trace theory suggests misinformation effects arise due to a combination of weakened memory for specific event details and stronger verbatim memory for post-event misinformation. Similar to signal detection theory, fuzzy trace theory posits that memory is more prone to the distorting effects of misinformation over time. This is due to the temporal decay of verbatim memory that naturally occurs, resulting in greater reliance on gist traces during recall. However, unlike most other false memory theories, fuzzy trace theory holds that these memory errors arise due to interactions between recollection and familiarity processes, rather than supporting one process over the other.

Discrepancy Detection Principle. Unlike the theories discussed so far, the discrepancy detection principle was developed to specifically explain misinformation effects, rather than false memories more generally (Tousignant et al., 1986). In the context of misinformation effects, discrepancy detection describes the process of identifying differences between a witnessed event and post-event misinformation (Butler & Loftus, 2018). Tousignant et al. (1986) proposed that discrepancy detection may account for individual differences in susceptibility to misinformation (i.e., discrepancy detection may explain why only some people who are exposed to misinformation later endorse that misinformation during recall). The discrepancy detection principle holds that memory is more likely to change or become distorted when an individual is unable to detect subtle differences between post-event misinformation and original event memory (Tousignant et al., 1986).

Much like source monitoring and verbatim memory, our ability to detect discrepancies between misinformation and original event details diminishes over time (Butler & Loftus, 2018; Tousignant et al., 1986). This is because event memory fades over time, meaning there is less information in long-term memory to draw upon when later attempting to process and detect discrepancies in post-event information. Therefore, the longer the interval between witnessing an event and exposure to misinformation, the more likely a person is to have poor discrepancy detection and the more likely it is that they will accept the misinformation.

Other factors that influence discrepancy detection include memory strength for the original event, feasibility of post-event misinformation, and warning about misinformation (Leding & Antonio, 2019). Tousignant et al. (1986) also suggest that rapid reading of misinformation may lead to a decreased likelihood that people will detect a discrepancy between what they read and what is stored in their memory about an event. However, other studies have failed to find such an effect (e.g., Butler & Loftus, 2018), or have even demonstrated the opposite effect – showing that slower reading times are associated with increased misinformation endorsement due to greater attention deployed to misinformation details (Gordon et al., 2015). These mixed findings suggest that reading times may not be a reliable measure of an individual's attention.

The discrepancy detection principle has been supported by many studies investigating the mechanism underlying misinformation effects. For example, Butler and Loftus (2018) proposed that discrepancy detection may account for the effect of retrieval practice on misinformation acceptance. In three experiments, participants witnessed an event via slideshows and afterwards completed either an initial cued recall test (retrieval practice condition) or a filler-task (control condition). After a short delay, participants were presented with a misinformation narrative and completed a three-alternative forced-choice recognition test and source memory task measuring participants' ability to detect discrepancies between slide details and narrative details. The authors found that misinformation effects were attenuated for participants who retrospectively detected discrepancies between the slides and misinformation. Discrepancy detection also attenuated the effect of retrieval practice on greater misinformation endorsement, although participants with retrieval practice tended to endorse more misinformation than control participants, irrespective of discrepancy detection (Butler & Loftus, 2018). Taken together, these results suggest discrepancy detection plays an important role in how people process and respond to post-event misinformation.

Leding and Antonio (2019) investigated the role of discrepancy detection in the relationship between high need for cognition (i.e., enjoyment of effortful cognitive tasks) and resistance to misinformation. Participants viewed a 10-minute video clip of a burglary and, after a short delay, listened to an auditory misinformation narrative about the video before completing a recognition test and the need for cognition scale. Results showed that high need for cognition participants were less susceptible to misinformation than low need for cognition participants. The authors reasoned that people with high need for cognition are more likely to detect discrepancies between event details and misinformation because they are more likely to closely monitor information than low need for cognition individuals (Leding & Antonio, 2019). This study is unique in that it shows the effect of manipulating discrepancy detection ability (through high versus low need for cognition) on misinformation acceptance.

Umanath et al. (2019) shed further light on discrepancy detection processes by examining the effect of explicit error detection instructions during the misinformation phase. Participants read a story and answered a series of questions containing details that were supplementary (additive misinformation) or contradictory (contradictory misinformation) to the original story before completing a recognition test. During the misinformation phase, participants were randomly assigned to either a full attention or divided attention condition and were either given explicit instructions to detect errors or no instructions. Results showed a reduction in endorsement of contradictory misinformation at final test for participants who were instructed to detect errors during the misinformation phase, though this effect was non-significant.

The authors proposed the influence of detection instructions on later misinformation acceptance may be contingent on whether misinformation was successfully detected during the misinformation phase (Umanath et al., 2019). Indeed, further analyses confirmed this, with successful discrepancy detection of contradictory details attenuating misinformation effects during the final test. This suggests initial detection of errors during misinformation exposure lessens the likelihood that contradictory misinformation will be reported. Therefore, misinformation effects may rely upon poor discriminability not only at test (as signal detection theory holds) but also during misinformation encoding.

Ballistic Accumulator Models. Ballistic accumulator (BA) models are used to understand the relationship between decision-making and response time (Brown & Heathcote, 2008). Although BA models are not directly related to misinformation effects, they can account for misinformation effects that arise through decision-making processes, such as multiple-choice recognition tests. BA models are grounded in the idea that recognition decisions are made through the accumulation of 'evidence' (e.g., perceptual details) for different competing response options (Brown & Heathcote, 2008). Each response option begins the decision trial with an initial amount of evidence, and this increases over time as evidence accumulates for each option. Similar to SDT, recognition decisions are made based on the first response option to accumulate enough evidence to reach a particular threshold. For example, a witness to a theft may be asked whether the thief's t-shirt was blue or green. Evidence is gathered for each of the two response options (blue or green) based on event memory, and a decision is made when enough evidence has accumulated for one of the options to reach the response threshold.

The rate at which evidence is accumulated for a particular response option is called the 'drift rate' (Brown & Heathcote, 2008). Each possible response option for a decision has its own drift rate; for example, in a four-alternative multiple choice test each of the four multiple choice responses will have its own drift rate. At its core, the drift rate is a measure of memory strength; an increase in drift rate typically results in greater likelihood of the corresponding response option being selected and decreased response time for a particular decision trial (Osth et al., 2017). Therefore, which response option is chosen during a recognition trial is a function of the initial amount of evidence and the drift rate for each response option. For example, a response option with high initial evidence but a low drift rate may still reach the response threshold faster than a response option with low initial evidence and a high drift rate (Osth et al., 2017).

While BA models do not make specific predictions about misinformation effects, we can apply the concepts of initial evidence and drift rates to explain how misinformation effects may occur. We know that misinformation effects arise when a person is exposed to misleading information about a previously witnessed event. For instance, a person may witness a thief wearing a blue t-shirt but later are told the thief's t-shirt was green (misinformation). When tested on their memory for the theft, the person may be asked to decide whether the thief's t-shirt was blue or green. Since memory for perceptual details decays and is subject to interference over time (Berman et al., 2009; Portrat et al., 2008), initial evidence for the blue t-shirt (correct response) may be lower than initial evidence for the green t-shirt (misinformation response). Moreover, due to recency effects the drift rate for the green t-shirt is likely to be greater than for the blue t-shirt. Therefore, the blue t-shirt response requires less evidence to be gathered to reach the decision threshold and evidence is accumulated faster than for the green t-shirt. This means the blue t-shirt is more likely to be selected as the correct answer, thereby increasing the likelihood that misinformation will be reported.

Metamemory Judgements. A separate but related issue is the role of metamemory judgements in the misinformation processes described throughout this section. Metamemory refers to judgements about the likelihood of remembering a studied item during future recall (Yang et al., 2018). There is mixed evidence about the accuracy of metamemory judgements in relation to actual memory performance. Some research suggests that positive predictions about one's own memory performance may be associated with better actual memory performance and negative predictions about one's own memory performance may be associated with poorer actual memory performance (e.g., Kelemen, 2000; Koriat, 1997). However, other findings suggest people often fail to take into account the many factors that influence memory, leading to inaccurate metamemory judgements (e.g., Mieth et al., 2021; Schaper et al., 2019; Schaper & Bayen, 2021).

Predictions about memory performance can influence how attention is deployed during encoding. Information that is assumed to be remembered well typically receives less attention (i.e., less time spent studying), while information that is predicted to be poorly remembered may receive greater attention (i.e., more time spent studying) during encoding in attempt to improve memory (Schaper & Bayen, 2021). However, basing decisions on how to deploy attention during encoding based on metamemory judgements can be problematic. This is because predictions about later memory performance can be influenced by the expectancy illusion; the commonly-held (but often incorrect) assumption that memory will be better for information that is *expected* (e.g., *banana* in the *fruit bowl*), than for information that is *unexpected* (e.g., *banana* in the *dishwasher*)

(Mieth et al., 2021; Schaper & Bayen, 2021). In contrast to this assumption, research tends to support that, to an extent, information that is unexpected is remembered better than expected information (e.g., Besken & Mulligan, 2013; Yue et al., 2013). This may be because unexpected material is often more difficult to process and requires more elaboration, which may result in improved memory.

Metamemory judgements may relate to the processes involved in misinformation effects. People may incorrectly assume they will remember information about a particular experienced event due to the expected nature of information in the context of the event. This may lead people to deploy less attention to expected aspects of the event, thereby leading to poorer elaboration of this information during encoding. In turn, this may lead to poorer source memory (SMF), poorer verbatim memory (FTT), or more lenient decision criterion (SDT; e.g., Brewer et al., 2022). Therefore, predictions about how well details of an event will be remembered may increase susceptibility to misinformation effects through this mechanism.

Summary of Misinformation Theories. Although each of these theories implicate different mechanisms, they share one important point in common: Misinformation effects are more likely to occur over time due to memory for the original event becoming weaker and vaguer. As memory weakens over time, people become increasingly susceptible to misinformation through source monitoring errors, reliance on gist memory, poor discrepancy detection, or familiarity strength of misinformation. Applied to traumatic events, this means people are more likely to report misinformation as time passes between initially witnessing the traumatic event and being exposed to misinformation about the event. Memory for traumatic events is especially susceptible to the effects of post-event misinformation due to the often-fragmented nature of such memories. Therefore, it is critical to understand the external factors that may influence susceptibility to misinformation in the context of traumatic memories.

Moderating Factors of the Misinformation Effect

A number of studies have explored the conditions under which misinformation effects may be moderated. For example, some studies have demonstrated the *protective effects* of certain factors against misinformation effects, including self-affirmations (Szpitalak & Polczyk, 2019), state anxiety (Kuczek et al., 2021), and mental warm-up (Szpitalak & Polczyk, 2014). Additionally, Crozier and Strange (2018) found misinformation effects can be *reversed* by providing corrections to post-event misinformation. However, other researchers have found misinformation effects can persist despite providing corrections (see Walter & Tukachinsky, 2020, for a meta-analysis). Mixed findings may be due to variations across studies in the timing of corrections in relation to exposure to misinformation, perceived credibility of misinformation sources, or the amount of exposure to misinformation prior to the corrective message (Walter & Tukachinsky, 2020).

Conversely, other studies have shown that certain factors may *increase* susceptibility to misinformation, such as mental fatigue (Szpitalak & Polczyk, 2014), alcohol placebo (Assefi & Garry, 2003), sleep between misinformation exposure and memory recall (Calvillo et al., 2016), and repetition of misinformation (Foster et al., 2012). These factors were all found to increase participants’ reliance on post-event misinformation at test and led to greater endorsement of misinformation at test.

In summary, there are a plethora of factors that can influence an individual’s susceptibility to misinformation. Understanding the factors that moderate misinformation effects is important because it enables people to recognise situations where the accuracy of people’s memory should be questioned. Recently, several studies have explored the effects of various psychotherapeutic techniques on susceptibility to misinformation (e.g., Calvillo & Emami, 2019; Houben et al., 2018; Wilson et al., 2015). This research is discussed in detail in the following chapter, and provides context for this thesis.

Chapter Summary

Advancements in internet technology and social media now offer us immediate access to coverage of real-world traumatic events. While we do not yet understand the extent of psychological impact of live-streamed footage of traumatic events, research on news media coverage of collective trauma (such as 9/11 and the Boston Marathon bombings) suggests that viewing such material can elicit physical and psychological symptoms of PTSD, depression, and anxiety (Pfefferbaum et al., 2019). Such highly emotional and impactful events tend to produce vivid autobiographical memories. Despite this, research shows that aspects of trauma memory tend to be fragmented and details can be forgotten or misremembered over time (e.g., Foa et al., 1995; Hirst et al., 2015; Strange & Takarangi, 2012).

The nature of human memory, and especially trauma memory, means that it is vulnerable to the distorting effects of misinformation. We also know from several theories of memory and misinformation effects (e.g., signal detection theory, source monitoring framework, fuzzy-trace theory, and discrepancy detection) that memory becomes more susceptible to misinformation over time. Despite the links between viewing traumatic material, psychological distress, and misinformation effects, few studies to date have investigated these links in the context of media exposure to trauma. Therefore, the present research investigates the effects of media exposure to trauma on several aspects of memory, including susceptibility to misinformation. The chapter also introduces the issue of misinformation effects in psychological therapy; a key issue that will be explored in the present research.

CHAPTER 3

Misinformation in Therapy

Chapter 2 focused on the nature of memory for traumatic events, particularly for events witnessed through the media. To recap, the fragmented nature of traumatic memories leaves them vulnerable to the distorting effects of post-event misinformation. As a result, people are at risk of misremembering details or report details that did not actually take place during the original event. This is problematic for two reasons; first, details may be misremembered as more distressing or dangerous, thereby exacerbating a person's distress. Second, it means caution should be applied when memory is the only evidence employed in a forensic eyewitness context.

Another factor that may leave traumatic memories susceptible to misinformation effects are post-trauma interventions (e.g., psychological therapies). Decades of research has shown that some techniques used in therapy can facilitate the production of false memories for traumatic events (for a review see Loftus & Davis, 2006). For example, Wilson et al. (2015) established a link between mindfulness, a popular psychotherapeutic technique, and false memory susceptibility. They reasoned that mindfulness increases susceptibility to false memories by reducing reality-monitoring accuracy; that is, the ability to discriminate between external memory sources (e.g., an event) and internal memory sources (e.g., an imagined event). A number of other therapeutic techniques, particularly projective psychodynamic approaches, have also been linked to increased false memory reports, including guided imagery, dream interpretation, hypnosis, reinterpretation of past events, and other suggestive techniques (Garry et al., 1996; Loftus & Davis, 2006; Madill & Holch, 2004; Sharman et al., 2004, 2005).

Therapists employ a wide variety of therapeutic interventions to reduce their clients' post-trauma symptoms. A vast majority of post-trauma interventions invoke memory mechanisms aimed at reducing distress associated with traumatic memories. Although these therapeutic techniques have often been well-studied in terms of their therapeutic benefits, few studies have investigated their risk for inducing misinformation effects. This chapter highlights the issue of false memories in therapy and introduces the focus for the present research.

False Memories in Therapy

Distressing experiences and traumatic autobiographical memories are often of central importance in therapy. In these instances, the aim of therapy is to decrease emotional distress associated with the traumatic memory (Iyadurai et al., 2019). Vivid memory intrusions are one of the major mechanisms through which emotional distress is maintained or exacerbated (Ehlers & Clark, 2000). Therefore, many therapies attempt to reduce the vividness and emotional intensity of these intrusive memories. According to the tenets of several theories (including source monitoring, fuzzy-trace theory, discrepancy detection, and ballistic accumulator models), false memories are more likely to occur when original event memory becomes weaker (Brainerd & Reyna, 2002; Brown & Heathcote, 2008; Johnson et al., 1993; Tousignant et al., 1986). Consequently, the very mechanisms that make therapeutic techniques effective in reducing distress may actually facilitate the development of false memories by weakening the memory strength of the event.

Additionally, some researchers argue that processes involved in exposure-based therapies may present opportunities for misinformation to be inadvertently introduced (Drivdahl et al., 2009). This is because exposure therapies require people to repeatedly recall and reconstruct their memory of a traumatic event. As part of this process, therapists typically guide clients by asking follow-up questions about aspects of their memory (Lindsay & Read, 1994). It is this process in exposure-based therapies that may create potential for therapists to unknowingly introduce misinformation. Furthermore, memory may be particularly susceptible to misinformation during exposure-based therapy due to the reconstructive nature of memory during recall (Drivdahl et al., 2009; Zaragoza et al., 2011).

The issue of false memories in psychotherapy is well established, with a longstanding debate between practitioners and researchers, known as the ‘memory wars’ (Patihis et al., 2014). Since the 1990s there have been numerous reports of cases where people who underwent memory recovery in therapy for past childhood abuse have later realised their ‘recovered memories’ were false or were disproven (Loftus & Davis, 2006; Patihis & Pendergrast, 2019).

At the core of the ‘memory wars’, some practitioners argue that some forms of current psychological distress may result from repressed childhood trauma and recovering these memories is necessary for current symptoms to improve (for a review see Loftus & Davis, 2006). Alternatively, cognitive researchers posit that entire traumatic memories are unlikely to be repressed because memory for such events is typically stronger than memory for everyday experiences (Loftus & Davis, 2006). Cognitive researchers also argue that memory is malleable and often unreliable (Loftus & Davis, 2006; Schacter, 1999).

Empirical evidence tends to support this view of memory; a 'mega-analysis' of false memory implantation studies showed that 30.5% participants developed false memories of suggested childhood events that had not occurred when a memory implantation technique was employed (Scoboria et al., 2017). Despite the evidence against the veracity of repressed memories, a recent study has shown that belief in this phenomenon has persisted among some therapists (see Patihis et al., 2021). Although the possibility of repressed memory cannot be completely discounted, it is often difficult to prove the veracity of such memories. Moreover, given the high rates of false memories reported by Scoboria et al. (2017), it is likely that many instances of 'recovered memory' can be explained by false memory phenomena.

The 'memory wars' largely focused on memory recovery therapies that facilitated the development of entirely fabricated memories for events that people never experienced. Although many practitioners are now aware of the dangers of memory recovery procedures, there is reason to believe that other techniques currently used to help people process traumatic memories may risk introducing some false memories in a much more subtle way; through distorting, blurring, or weakening memory for specific details of an event. This highlights a particular challenge for practitioners in facilitating the processing of traumatic memories while simultaneously avoiding significant distortion of these memories.

Eye Movement Desensitisation and Reprocessing Therapy One such therapy that has recently been linked to false memory phenomena is Eye Movement Desensitisation and Reprocessing therapy (EMDR; Shapiro, 1989). EMDR is an empirically validated frontline psychotherapy, most commonly used to treat symptoms of PTSD. A unique component of EMDR therapy involves bilateral stimulation, usually through the client making horizontal eye movements while simultaneously thinking about the traumatic memory that is causing symptoms of distress (Shapiro & Laliotis, 2011). The primary aim of EMDR therapy is to alleviate distress associated with the traumatic memory.

EMDR therapy comprises eight treatment phases with specific focus on adverse experiences underlying a client's presenting symptoms, current perpetuating circumstances, and identifying the skills necessary for alleviating symptoms (Shapiro & Solomon, 2017). In a typical session, eye movements are completed in several sets lasting approximately 30 seconds each. At the end of each set, there is an interview stage where the therapist asks the client what aspects of the memory they were thinking about, and this information is used to guide the focus for the next set of eye movements (Shapiro & Solomon, 2017).

Effectiveness of EMDR Therapy. Multiple meta-analyses of randomised clinical trials have evidenced the effectiveness of EMDR therapy for alleviating symptoms of PTSD (Chen et al., 2014; Cusack et al., 2016; Khan et al., 2018). Some studies have even demonstrated comparable effectiveness with other treatments, including pharmacological treatment with fluoxetine (Cusack et al., 2016), and trauma-focused cognitive-behavioural therapy (Ana et al., 2017). According to Lee and Cuijpers (2013), the incremental effect of the eye movement component of EMDR has also been evidenced across several studies. However, a meta-analysis by Davidson and Parker (2001) revealed no difference in treatment effectiveness between EMDR and other exposure therapies, and no additive effect of the eye movement component. Such contrasting findings in the literature may be, in part, attributed to a general lack of randomised control trials, typically small sample sizes, variations in the outcome measures employed, and differences in how EMDR techniques are applied.

Other studies have attempted to isolate the eye movement component of EMDR therapy in laboratory experiments. These experiments have investigated the effects of eye movements on the vividness and emotional intensity of traumatic memories. It is theorised that eye movements may enhance the processing of traumatic memories by decreasing the vividness and emotionality of traumatic memory images, thereby also reducing related distress and avoidance (Shapiro & Maxfield, 2002). Eye movements are proposed to reduce memory vividness and emotionality through taxing working memory – this is explained further later in this section. Ultimately, this allows people to engage more effectively in desensitisation procedures and attend to more adaptive information regarding current life experiences.

Beneficial effects of eye movements on the emotionality and vividness of traumatic memories have been highlighted in a meta-analysis of 10 analogue laboratory studies (Lee & Cuijpers, 2013). In these studies, participants rated the emotionality and vividness of a traumatic autobiographical memory before and after performing either eye movements or an eyes-stationary control task during memory recall. The meta-analysis revealed a significant mean effect of eye movements on distressing memories, with eye movements resulting in larger reductions in both memory emotionality (Cohen’s $d = 0.66$) and vividness ($d = 0.91$) compared to eyes-stationary control tasks (Lee & Cuijpers, 2013). The authors concluded that eye movements in EMDR therapy alter the processing of emotive memories by reducing the intensity of image vividness and emotionality.

Since its early development there has been widespread debate about the mechanism involved in EMDR therapy. However, it is now generally accepted that eye movements most plausibly reduce the vividness and emotionality of traumatic memories by taxing working memory through a variety of potential mechanisms such as dividing attentional resources and increased load on the visuo-spatial sketchpad (Andrade et al., 1997; Gunter & Bodner, 2008; Maxfield et al., 2008). More specifically, performing eye movements and recalling a traumatic memory are thought to simultaneously draw upon the central executive component of working memory and expend the limited processing resources available to this system. Since working memory has a limited capacity (Baddeley, 2012), less attentional resources are available for memory recall, leading to reduction in the vividness and emotional intensity of the memory. The working memory theory of EMDR has received support in a number of laboratory studies demonstrating similar effects on memory using a variety of alternative taxing tasks, including counting (van den Hout et al., 2010), complex spatial tapping (Andrade et al., 1997), copying a complex figure (Gunter & Bodner, 2008), playing the computer game Tetris (Engelhard et al., 2010), and performing a mindful breathing exercise (van den Hout et al., 2011).

The majority of laboratory studies have focused on the effects of eye movements on participants' autobiographical memories for directly experienced events. However, one study has investigated the effect of eye movements on distressing memories arising from traumatic media via the trauma film paradigm. In the study, participants used two 10-point scales to rate the vividness (0 = not vivid at all, 10 = extremely vivid) and emotionality (0 = extremely negative, 10 = extremely positive) of a video depicting a fictional serious car crash before and after performing either eye movements or no eye movements (Houben et al., 2018). Ratings of memory vividness decreased and emotionality was rated more positively from pre to post test in both conditions, though the effect was more pronounced in the eye movement condition (d_{vivid}= 0.66, d_{emo}= -0.30) compared to no eye movements (d_{vivid}= 0.35, d_{emo}= -0.05) (Houben et al., 2018).

Two subsequent replications of this study have found mixed results for the effect of eye movements on memory ratings. Van Schie and Leer (2019) found no effect of eye movements on pre-post memory vividness but some evidence of a decrease in negative emotionality. In contrast, Calvillo and Emami (2019) found that pre-post decreases in memory vividness were significant in the eye movement group (Cohen's d = 0.66), but not in the control group (d = 0.10), and no effects were found for memory emotionality. Taken together, these findings suggest eye movements may have an effect on the vividness and emotional intensity of distressing memories; however, the extent of this effect remains unclear.

Inconsistencies in findings may be due to the nature of the trauma film employed in these studies. All three of the studies induced traumatic memories using a staged video of a car crash which clearly depicted a fictional event and induced only moderate vividness and emotionality ratings. The obviously staged nature of this film may prevent people from developing vivid or intense emotional imagery analogous to real-life traumatic events that might become the focus of therapy. In a comparison study of different films in the trauma film paradigm, Arnaudova and Hagenaars (2017) found fictional films depicting sexual or physical violence produced higher ratings of distress and disgust than a fictional film depicting a car accident. High base levels of memory vividness and emotionality are required to effectively target factors that influence posttraumatic reactions, such as the effects of therapy. Therefore, the effects of eye movements on memory vividness and emotionality may be clarified by using a more realistic and emotionally intense trauma film.

Misinformation Effects in EMDR Therapy. More recently, research has sparked debate regarding the false memory potential of eye movements used in EMDR therapy. Often after a set of eye movements, therapists will ask detailed follow-up questions to clarify certain aspects of memory that emerged during the session (Shapiro, 1989). Houben et al. (2018) suggest that during these follow-up questions, therapists might inadvertently introduce misinformation that can influence the client’s memory. Moreover, eye movements may leave clients especially susceptible to the distorting effects of misinformation due to their memory for the original event becoming less vivid. As false memory theories suggest, misinformation effects are more likely to occur when memory for the original event has become vague. Therefore, reductions in memory vividness resulting from eye movements may also plausibly have the unintended consequence of increasing reports of misinformation.

Houben et al. (2018) investigated this claim by testing whether horizontal eye movements increase susceptibility to misinformation. In their experiment, 82 participants viewed a video of a fictional car crash and rated how vivid and emotional they found the video. Participants recalled images from the video while simultaneously performing sets of eye movements or an eyes-stationary control task and were later presented with an eyewitness narrative containing misinformation about the car crash event. Finally, participants completed a recognition test for their memory of the original video. The experiment revealed that participants who engaged in eye movements incorrectly reported misinformation more frequently at test than control participants (Cohen's d = 0.77). A Bayes factor of 35.26 indicated strong support for the effect of eye movements on susceptibility to misinformation.

Despite the large effects reported by Houben et al. (2018), results from three subsequent replications evidenced no such effect of eye movements on susceptibility to misinformation (Calvillo & Emami, 2019; Sievwright, 2018; van Schie & Leer, 2019). In fact, these attempts to replicate Houben et al.'s (2018) findings failed to instil the misinformation effect altogether. Floor effects were observed for endorsement of misinformation in all three recent replications; on average, participants endorsed fewer than a single misinformation item out of a possible score of five (Calvillo & Emami, 2019; Sievwright, 2018; van Schie & Leer, 2019). Failures to successfully establish the misinformation effect may be attributed to methodological issues with the misinformation procedures and stimuli adopted in these studies. Methodological issues include the ecological validity of the target video, length of retention intervals, and measurement of misinformation endorsement and memory accuracy. These issues are discussed in a later section of this chapter and will be addressed in Experiment One of the present research.

Cognitive Bias Modification A downside of many psychotherapies, including EMDR, is that they require access to professional psychological services, which are often costly, time-consuming, and difficult to access (Spence et al., 2011). Moreover, recent COVID-19 public health injunctions have created additional temporary barriers to accessing face-to-face psychotherapy (Bennett et al., 2020). As a result, psychologists are increasingly turning to alternative modalities for offering psychological services, including telepsychology and internet-based interventions (Pierce et al., 2021). Internet-based interventions not only increase convenience in accessing mental health treatment but may also be useful in providing effective evidence-based interventions to large populations to alleviate early symptoms of distress in situations of mass trauma (e.g., following a terrorist attack or natural disaster).

Cognitive Bias Modification-Appraisal training is a computerised self-administered therapeutic tool that can be readily adapted for online use. This intervention is based on components of Cognitive Behavioural Therapy (CBT). CBT is considered a leading psychotherapy for many types of mental distress, including PTSD (Watkins et al., 2018). The foundation of CBT largely rests on the assumption that many types of mental distress are caused and maintained by dysfunctional thinking patterns and maladaptive behaviours (Watkins et al., 2018). As its name suggests, CBT involves both behavioural techniques, such as exposure, and cognitive techniques such as cognitive restructuring.

A recent meta-analysis established comparable efficacy of EMDR and CBT for reducing symptoms of PTSD at three months follow-up (Khan et al., 2018). However, there is a paucity of evidence evaluating the effectiveness of *online* EMDR therapy for PTSD (see Lenferink et al., 2020). Alternatively, multiple meta-analyses have evidenced promising effects of internet-based CBT in treating PTSD compared to waitlist controls (Lewis et al., 2019; Olthuis et al., 2016). Therefore, Lenferink et al. (2020) suggest CBT-based interventions are preferable when it comes to internet-based treatment for trauma.

Research on internet-based CBT for PTSD has largely focused on interventions involving multiple sessions and minimal therapist contact (e.g., Lewis et al., 2019; Olthuis et al., 2016). These interventions are typically designed to treat people who meet diagnostic criteria for PTSD. Cognitive Bias Modification-Appraisal (CBM-App) training is unique in that it is designed for single-session use, it does not require any therapist contact, and is designed as an early intervention to reduce post-trauma reactions at a sub-clinical level (Woud et al., 2012; Woud, Zlomuzica, et al., 2018). For these reasons, CBM-App training may be a useful tool for reducing the vividness and emotionality of memories that arise immediately following media exposure to traumatic images.

At its core, CBM-App training focuses on reframing maladaptive cognitive appraisals regarding a traumatic event and one’s ability to cope in the aftermath of the event. A multitude of research suggests the way an individual makes meaning of a traumatic event predicts subsequent development of post-traumatic stress symptoms (e.g., Barlow et al., 2017; Cromer & Smyth, 2010; DePrince et al., 2011). This meaning-making process results in the development of post-traumatic cognitive appraisals. Cognitive appraisals generally relate to the way people view themselves, the world, and other people (Cromer & Smyth, 2010; Ehlers & Clark, 2000). Maladaptive cognitive appraisals occur when an individual interprets a traumatic event in a way that does not accurately reflect reality (Cromer & Smyth, 2010). For example, a person may interpret their experience of a violent assault as evidence that the world is a dangerous place or that they are vulnerable, helpless, or responsible.

According to the cognitive model of PTSD, maladaptive appraisals of a traumatic event can fuel a current sense of threat, which in turn leads to the development of symptoms such as intrusions, avoidance, emotional distress, and anxiety (Lang et al., 2009; Woud, Zlomuzica, et al., 2018). A number of empirical studies have demonstrated the significant role of cognitive appraisals in the development and maintenance of post-traumatic stress symptoms. In these studies, researchers have found that negative appraisals are a greater predictor of post-traumatic stress symptom severity than any characteristic of the traumatic event itself (Cromer & Smyth, 2010; DePrince et al., 2011; Marks et al., 2018; Martin et al., 2013). Relatedly, emotional processing theory posits that recovery from post-traumatic stress symptoms occurs when dysfunctional negative appraisals are disproven; giving way to more adaptive and rational appraisals (McLean et al., 2015). Fortunately, cognitive appraisals are malleable, and changes in maladaptive appraisals can reduce related symptoms of distress (Barlow et al., 2017). To that end, maladaptive cognitive appraisals make a promising target for psychological intervention.

CBM-App training was initially developed by Woud et al. (2012) based on methods originating from the Cognitive Bias Modification literature (e.g., MacLeod & Mathews, 2012; Mathews & MacLeod, 2002). These methods were developed for use in experimental psychopathology research to investigate the effects of cognitive biases on symptom alleviation for a range of disorders (de Kleine et al., 2019). Traditional Cognitive Bias Modification involves the repeated practice of responding in a systematically positive manner over a series of computerised trials. Woud et al. (2012) extended this paradigm to specifically address dysfunctional cognitive habits, such as selectively attending to threat or catastrophising, that may arise following exposure to a traumatic event or stressor.

In Woud et al.'s (2012; 2018) CBM-App procedure, participants are shown a series of incomplete sentences related to appraisals of a traumatic event. The sentences were adapted from the 'Self' subscale of the Posttraumatic Cognitions Inventory (Foa et al., 1999). Each sentence contains a partially presented word and participants are required to complete the sentence by identifying the missing letters. Only one possible solution is available for each sentence, and the solution produces a sentence that is consistent with a positive interpretation of the event. For example, participants may be asked to complete the following sentence: "My expectations for the future are *ho-ef-l*", which is resolved as "*hopeful*". The purpose is to shift people's negative interpretations of the traumatic event's implications and their responses to the event.

Effectiveness of CBM-App Training. Several recent studies have examined the effectiveness of a computerised cognitive reappraisal training activity for reducing symptoms of post-event distress. Woud et al. (2013) investigated whether completing reappraisal training *before* a stressful event prevents later development of posttraumatic symptoms. Participants completed the CBM-App procedure; half of the participants were trained to appraise positively, and half were trained to appraise negatively (comparison condition). All participants then viewed a traumatic film and completed several measures assessing mood, cognitive bias, intrusions, and posttraumatic cognitions. Posttraumatic cognitions were measured at pre-test, immediately post viewing the trauma film, and at a seven-day follow-up.

Results showed CBM-App training induced cognitive appraisal styles consistent with each training condition. Participants trained in positive appraisals reported less distress related to intrusive memories of the trauma film compared to those trained negatively, despite both groups experiencing the same frequency of intrusions (Woud et al., 2013). Although participants in the positive CBM-App group demonstrated improvements in posttraumatic cognitions (PTCI scores) over one week, the change from baseline PTCI scores was non-significant. The authors concluded that CBM-App training may be used as a preventative measure to protect people against posttraumatic distress before even experiencing a stressor.

Another study by Woud et al. (2018) examined the effect of CBM-App training on reactions to distressing autobiographical events. In the study, participants imagined a distressing event they had experienced before receiving either positive or negative CBM-App training. Participants completed measures of posttraumatic stress symptoms, trauma intrusions, and intrusion-related distress. Results showed CBM-App training was successful in inducing training-congruent appraisals. They found participants who received positive appraisal training reported more functional trauma-related appraisals immediately post training and experienced less intrusion-related distress compared to participants who received negative appraisal training (Woud, Zlomuzica, et al., 2018). Participants trained positively also reported less posttraumatic stress symptoms than those trained negatively. Overall, this study demonstrated that the effects of CBM-App training can be replicated using participants' own distressing life events. Findings further strengthen evidence that CBM-App training may be useful for reducing distress arising from real-world traumatic events.

Computerised CBM-App training has also been employed to examine whether positive appraisals reduce centrality (perceived importance of an event to one's life) of traumatic events. Vermeulen et al. (2019) hypothesised that greater event centrality is associated with PTSD symptoms; therefore, reductions in centrality should lead to decreased symptoms of PTSD. In their study, participants recalled their most distressing memory before completing several measures of PTSD symptoms. Half of the participants completed CBM-App training related to the centrality of their traumatic memory while the remaining participants completed CBM-App training that was unrelated to event centrality and referred to neutral everyday events. All participants completed post-intervention measures of PTSD symptoms immediately after training and three days later. Results showed that centrality CBM-App training led to reduced event centrality after three days, but not immediately post-training (Vermeulen et al., 2019). The authors suggested that delayed effects of the training may be due to the need for participants to consolidate their new appraisals within the context of their everyday life. Importantly, this emphasises the need to include a delayed follow-up session to better understand the impact of CBM-App training.

More recently, de Klein et al. (2019) investigated the effect of CBM-App training for people with PTSD seeking treatment. Half of the participants received positive CBM-App training and the other half received neutral CBM-App training (comparison condition). de Klein et al. (2019) found participants in both groups experienced a small-to-moderate decline in negative appraisal bias and PTSD symptoms, but noted no significant difference in outcomes between the two groups. This suggests positive appraisal training may not be superior to neutral appraisal training for people with PTSD. The authors speculated that a possible explanation for their null findings may be that neutral appraisal training was actually a mild version of the positive training and may have unintentionally induced a positive bias (de Kleine et al., 2019). It may also be that CBM-App training is more appropriate for sub-clinical levels of posttraumatic stress, rather than for those with a PTSD diagnosis.

Similar cognitive bias modification tasks have demonstrated promising results in treating symptoms of obsessive-compulsive disorder (Grisham et al., 2014), social anxiety disorder (Liu et al., 2017), and intrusion-related distress in depression (Newby et al., 2014). Moreover, a randomised controlled trial by Woud and Blackwell et al. (2018) is underway to test whether CBM-App training is an effective adjunct to inpatient PTSD treatment in improving treatment outcomes. Taken together, these studies provide strong evidence of the causal role of trauma-related appraisals in post-trauma symptomatology. They also show that maladaptive appraisals can be modified, and this can impact levels of distress in a positive way. Changing the narrative and appraisal of traumatic events is also the foundation of well-known therapies, including Trauma-Focused Cognitive Behavioural Therapy and Narrative Therapy (Ehlers et al., 2021; White, 2004).

One issue with the extant research on CBM-App training is that most studies have compared positive reappraisal training to either negative or neutral reappraisal training. Since positive reappraisal training is thought to be the mechanism by which trauma-related distress is reduced, it is important to examine the effectiveness of this intervention in comparison to an inactive control condition, or to other psychological interventions. For this reason, the present research will compare the effects of positive CBM-App training and a post-trauma debrief (active control condition) on memory for a traumatic video.

Cognitive Reappraisal Training and Misinformation Effects. Although little research has investigated the link between cognitive reappraisal training and false memory, there are theoretical reasons to believe an association exists. Since cognitive reappraisal training aims to change how individuals appraise their recollection of a traumatic event, it is possible that this intervention may also increase susceptibility to misinformation. However, currently there appears to be limited research exploring this specific claim.

Patihis et al. (2019) present evidence of a possible mechanism through which there could be a link between cognitive reappraisals and false memory. They investigated whether cognitive reappraisals of past events distort memory of important and complex emotions, such as love. The authors posit that memory is reconstructed from a combination of memory traces and current cognitions (Patihis et al., 2019). Therefore, changes to current cognitions, such as through CBM-App training, may lead to changes in the way we remember autobiographical events.

In their study, Patihis et al., (2019) manipulated participants' appraisals of their mothers by randomly assigning participants to one of four conditions. In the first condition, participants wrote about their mothers' negative attributes. (appraisal-down condition). Participants in the second condition wrote about their mothers' positive attributes (appraisal-up condition). The remaining two conditions were control comparison groups, where participants either wrote about negative attributes of a teacher or had no writing task. All participants completed the Memory of Love Towards Parents Questionnaire and the Positive and Negative Affect Scale immediately after completing the writing task and at two follow-up session: two weeks and four weeks after the initial session.

Results showed the appraisal-up and appraisal-down conditions were successful in manipulating participants' current appraisals of their mothers (Patihis et al., 2019). As hypothesised, participants who wrote about their mothers' positive attributes had significantly higher memory-of-love scores than participants who wrote about their mothers' negative attributes. The cognitive appraisal condition also affected participants' current feelings of love towards their mothers; participants in the appraisal-down condition reported lower current feelings of love towards their mother than those in the appraisal-up and the two control conditions. These effects appeared to remain for at least four weeks; however, after eight weeks, the effects had substantially reduced (Patihis et al., 2019). Importantly, this study shows that shifts in appraisals can cause shifts in attitudes and emotions associated with an autobiographical event, at least temporarily. Therefore, it is possible that CBM-App training may cause shifts in the way an event is remembered and may leave people susceptible to false memories.

Alternatively, Hayes et al. (2010) argue cognitive reappraisal enhances memory accuracy because it encourages more elaborative encoding of event details. They found participants who engaged in cognitive reappraisal experienced a greater reduction in negative emotions and showed superior memory performance for negative images than participants who either suppressed the images or viewed them passively. Although these findings suggest elaborative encoding through cognitive reappraisal may increase memory accuracy, such elaborative processing may also facilitate the formation of false memories or integration of misinformation into recollections.

Indeed, research has shown elaborative processing of events can increase susceptibility to false memories. Zaragoza et al. (2011) used a misinformation paradigm to test whether reflecting on the meaning and implications of suggested information about a witnessed event increases false memory reports. They found participants who elaborated on the meaning of suggested event details accepted misinformation more frequently than those who elaborated on perceptual details or did not elaborate at all. However, they also found that elaboration improved overall memory for witnessed details, consistent with Hayes et al.'s (2010) findings.

Since CBM-App training involves reflecting on the meaning of an event, and shifting one's interpretation of this meaning, there is a potential for misinformation effects to be established during this process. Taken together, these studies suggest reappraisals can improve memory accuracy through elaboration, but this same process may also lead to elaboration and subsequent integration of misinformation. One of the aims of the present research is to investigate whether positive CBM-App training increases acceptance of misinformation relative to a post-trauma debrief control condition. The purpose is to understand the potential drawbacks of this therapeutic intervention and how it influences memory, particularly in the context of trauma.

CHAPTER 4

Rationale for the Present Research

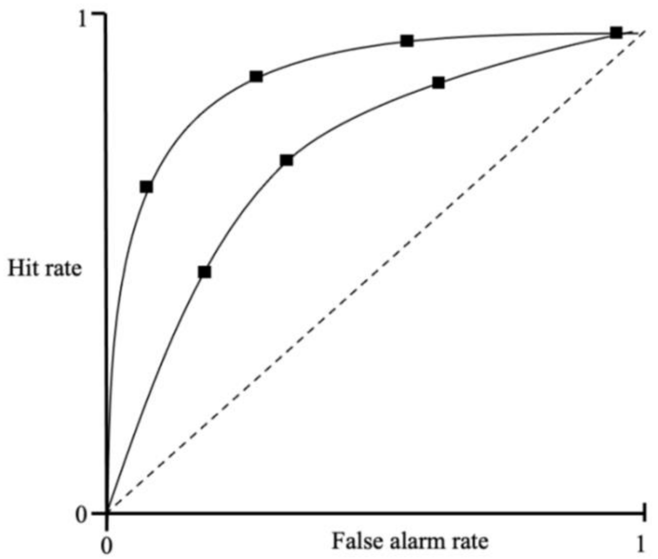
Despite extensive research demonstrating the magnitude of misinformation effects, inconsistencies remain in the detection of the effect under certain experimental conditions. Several recent studies have failed to replicate the effect that performing eye movements increases susceptibility to post-event misinformation (Houben et al., 2018). In fact, some replications of Houben et al.'s (2018) study have failed to instil misinformation effects altogether. Specifically, floor effects were observed for endorsement of misinformation in all three recent replications; on average, participants endorsed fewer than a single misinformation item out of a possible score of five (Calvillo & Emami, 2019; Sievwright, 2018; van Schie & Leer, 2019).

Failures to successfully establish a misinformation effect can likely be attributed to methodological issues with the misinformation procedures and materials adopted in these studies. In particular, floor effects of endorsement of misinformation suggest the memory test may be too easy for participants. A short retention interval between encoding of the initial event and exposure to post-event misinformation may be one factor affecting test difficulty. Despite research suggesting that longer retention intervals (i.e., at least 24 hours) result in larger misinformation effects (Frost et al., 2002; Paz-Alonso & Goodman, 2008), the majority of misinformation studies use retention intervals of no longer than 30 minutes between participants witnessing the target event and presentation of misinformation. Theoretically, increasing the retention interval should increase susceptibility to misinformation because event memory, particularly for peripheral details, decays over time (Conway, 2009; Sekeres et al., 2016). As previously discussed, memory is more vulnerable to distortion from post-event misinformation when the original memory trace has been weakened.

Test difficulty, and the ability to detect misinformation effects, are also determined by the type of memory test used. To quantify memory accuracy and acceptance of misinformation, many studies adopt a two-alternative forced-choice (2AFC) recognition test, where participants are asked about some aspect of the target event and are given two possible response options to choose from (e.g., Crozier & Strange, 2018; Moore & Lampinen, 2016; Takarangi et al., 2006; Wyler & Oswald, 2016). However, 2AFC tests do not allow researchers to detect small discrepancies in participants' abilities to distinguish between true event details and misinformation. This is because 2AFC tests offer a limited range of responses, making it difficult to distinguish between a guess and genuine recognition of an item. 2AFC tests also do not account for the misinformation effect relative to the accuracy of memory more generally.

One method to overcome this issue is to ask participants to make a judgement regarding how confident they are that a particular item was present in the target event. These ratings can be used to determine discriminability (*d'*) and the points on a receiver operating characteristic (ROC) curve. The points on the ROC curve provide a non-parametric estimate of the relationship between the hit rate and the false alarm rate at varying confidence levels (Stanislaw & Todorov, 1999), as shown in Figure 7. ROC curves that bow further towards the left of the graph represent greater discrimination accuracy between true event details and misinformation details (Stanislaw & Todorov, 1999).

Figure 7.
Example of two receiver operating characteristic curves.



Note: Each square data point along the curves represents a different confidence level. The dotted line represents chance performance on a recognition test. This is where the hit rate is equal to the false alarm rate at each confidence level.

Confidence judgements are not new to the forensic context. For example, confidence ratings have been used in a series of studies by Sauer, Brewer and colleagues (Sauer et al., 2008, 2012; Weber & Brewer, 2004) to discriminate guilty from innocent suspects in eyewitness line-ups. Confidence judgements have also been used in research by Horry et al. (2012) to investigate memory conformity; a phenomenon where one person's memory report influences another person's memory report. There is both a theoretical and empirical rationale for using confidence judgements in eyewitness research. Firstly, confidence judgements allow a more direct measurement of the match between a presented item at test and the witness' memory for the original event (Sauer et al., 2012).

This is sometimes referred to as *ecphoric similarity* (see Tulving, 1981). Secondly, confidence judgements offer a more sensitive estimate of memory and richer information about recognition decisions (Sauer et al., 2012). This advantage of confidence judgements is further described below in the context of misinformation research.

As previously mentioned, confidence judgements can be used to plot points on a ROC curve. ROC analyses are increasingly used to investigate processes underlying recognition memory (Gronlund & Benjamin, 2018). The advantage of this signal detection paradigm is that it may detect subtle changes in memory strength and bias when memory change does not meet the threshold for reporting misinformation as true (Stanislaw & Todorov, 1999). For example, ROC curves enable us to pick up on reduced certainty that misinformation is false even when accuracy rates are not explicitly affected. Another key advantage of using ROC curves is that they are unaffected by response bias (Stanislaw & Todorov, 1999). ROC curves are also not constrained by theoretical assumptions regarding the nature of recognition memory (e.g., normal distribution of signal and noise) in the same way as other signal detection analyses (Wixted & Mickes, 2018).

In addition, existing misinformation paradigms are not always well-suited to investigating misinformation effects in the context of media exposure to traumatic events. Typical misinformation studies use staged videos of either a burglary or car crash, which participants may suspect depict fictional events. Such videos are often not designed to evoke strong emotional responses analogous to real-life traumatic events that might become the focus of therapeutic intervention. In fact, they are often designed to pass low risk ethics standards and be minimally intrusive for participants (Stirling et al., 2021). Relatedly, misinformation studies are generally conducted in the laboratory and are vulnerable to experimenter demand effects. An online misinformation experiment minimises experimenter effects, increases ecological validity, and allows us to explore the impact of computerised CBM-App training on misinformation reports in a context where people are more likely to come into contact with traumatic material—through their own computers.

Overview of the Present Research

There is some evidence that certain therapeutic techniques may distort people's recollections of autobiographical events (e.g., Houben et al., 2018). From a theoretical perspective, the dampening effects of such therapeutic techniques on the vividness and emotional intensity of traumatic memories may leave people susceptible to post-event misinformation (Brainerd & Reyna, 2002; Johnson et al., 1993; Tousignant et al., 1986). Moreover, techniques such as CBM-App training aim to shift people's interpretation of traumatic events, which may directly impact how well details are remembered and recollected in the future. Currently, there is limited research investigating the effects of CBM-App training on susceptibility to misinformation. This is an important contribution to the extant literature because it may highlight a potential drawback of cognitive reappraisals in therapy. It is critical to understand whether the vividness and emotional intensity of traumatic memories can be reduced without unnecessarily increasing people's susceptibility to misinformation.

The present research aims to examine the effects of CBM-App training on memory vividness and emotionality, as well as on susceptibility to misinformation in the context of media exposure to traumatic images. The research will address the methodological concerns outlined earlier in this chapter by developing and employing novel experimental materials and procedures for instilling misinformation effects. This novel procedure will implement a longer retention interval of approximately five days between participants witnessing the target event and exposure to misinformation. It will also involve an ostensibly realistic viral-style video of the target event to evoke mental images analogous to a real-life traumatic event. Lastly, a signal detection paradigm will be used to maximise sensitivity to small changes in memory strength following exposure to misinformation. The aims and hypotheses of this research are discussed in further detail in Chapter 5 (Experiment One) and Chapter 6 (Experiment Two).

CHAPTER 5

Experiment One

This chapter outlines the rationale, hypotheses, method, and results for Experiment One. Experiment One was published in *PeerJ* (Sievwright et al., 2021) and therefore this chapter is based on the published article. Some aspects of the previous chapters have been repeated here as part of the original published article.

Misinformation Effects in an Online Sample: Results of an Experimental Study with a Five Day Retention Interval

For decades, researchers have demonstrated the harmful effects of misleading post-event information on memory. Numerous laboratory studies have shown that participants exposed to post-event misinformation frequently endorse misinformation as part of their original memory of an event; this is known as the misinformation effect (Loftus, 2005). The application of these findings in legal cases has shed light on the detrimental effects of misinformation in the real-world and has contributed to the overturn of wrongful convictions. Misinformation effects may also lead people to remember events as being more traumatic than initially reported (Porter et al., 2008). This study builds on extant misinformation research by testing a novel method for investigating misinformation effects in an online context.

Misinformation effects are traditionally studied in face-to-face laboratory settings using a three-stage procedure (Loftus et al., 1978). First, participants witness a target event, often through a video depicting a crime. Second, after a delay, participants are exposed to misleading information about the target event – this is often presented through a fictional eyewitness account, fictional news report, memory test, or through discussion with a co-witness. Finally, when tested on their memory for the target event (ranging 5 minutes to 2 weeks later), participants frequently report misinformation as part of their original recollection. Misinformation effects have led participants to report seeing a thief using a hammer instead of screwdriver (McCloskey & Zaragoza, 1985), or even recall non-existent footage of the car crash in which Diana, Princess of Wales was killed (Ost et al., 2002). This procedure has taught us a great deal about how false memories develop in laboratory conditions (for a review see Loftus, 2005).

One area of emerging concern that has been relatively under-researched is the impact of misinformation following exposure to traumatic events that are viewed online. Advancements in Internet technology now offer us immediate access to coverage of real-world traumatic events (Peterson & Densley, 2017). Consequently, people are inadvertently exposing themselves to footage of such events during their regular Internet use. For example, within 24 hours of the 2019 Christchurch, New Zealand mosque attacks, the live-stream of the attack had been uploaded to Facebook over 1.5 million times (Besley & Peters, 2019). This is particularly concerning given accumulating evidence that traumatic content online can have harmful psychological effects for viewers, including post-traumatic stress symptoms, depression, and anxiety (e.g., Pfefferbaum et al., 2019; Redmond et al., 2019; Thompson et al., 2019).

Up-to-the-minute media coverage of unfolding events often includes inaccurate reports based on incomplete information and mistaken details (Rapp & Salovich, 2018; Rich & Zaragoza, 2016). For instance, during the Christchurch mosque attacks it was erroneously reported that 'a good guy with a gun' stopped the shooter. In fact the man rushed at the shooter with an electronic payment terminal (Cooke, 2019). Research suggests memory for highly negative events is susceptible to distortion (Nahleen et al., 2020; Strange & Takarangi, 2012). Exposure to inaccurate information facilitates misinformation effects for real-world traumatic events witnessed through the media. People who view traumatic videos may be vectors of distressing and inaccurate information to the general public. Thus, memory distortions from misinformation effects may have clinical implications even in the absence of legal ones. For instance, even if people who witness violent online videos are unlikely to be called as factual event witnesses in a trial due to the ability to verify facts from the recorded footage, memory distortions from misinformation effects may have implications for psychological distress and, potentially, the development of clinical disorders such as PTSD. Therefore, it is imperative we understand exactly how misinformation effects work in the context of traumatic online media exposure.

Existing misinformation paradigms are not well-suited to investigating misinformation effects in the context of media exposure to traumatic events. Typical misinformation studies use staged videos of a burglary or car crash that clearly depict fictional events. These videos are often not designed to evoke strong emotional responses. Additionally, the artificial laboratory environment may lead to less intense emotional reactions than would typically occur when witnessing a real-life traumatic event (Chae, 2010). Examining misinformation effects in an online context minimises potential experimenter effects and increases ecological validity by exposing people to traumatic media in the context people are most likely to encounter such material—on their own computers.

Two important methodological decisions need to be made when measuring misinformation effects: retention intervals and type of memory test used. Retention intervals are the period between viewing the target event (encoding) and being exposed to misinformation about the event, and the period between exposure to misinformation and memory retrieval. Although longer retention intervals (i.e., at least 24 hours) produce larger misinformation effects (Frost et al., 2002; Paz-Alonso & Goodman, 2008), most studies use retention intervals of 30 minutes or less. Longer retention intervals between encoding and misinformation exposure have theoretical and pragmatic advantages: theoretically, increasing the retention interval should increase susceptibility to misinformation because event memory decays over time (Conway, 2009; Sekeres et al., 2016). Longer retention intervals also mimic real-world time intervals between witnessing an event and memory retrieval. An online approach should help to minimise practical issues around longer retention intervals because multiple participant visits to the laboratory are not required.

The type of memory test used is important to detecting misinformation effects. Misinformation studies employ different types of memory tests to quantify memory accuracy and misinformation endorsement, including forced-choice recognition, cued recall, free recall, remember/know memory judgements, and source monitoring tests. One of the most common memory tests used in misinformation research is a two-alternative forced-choice recognition test, a test where participants' answers are categorically coded as correct or incorrect. In these tests, participants are asked about some aspect of the target event and are given two possible response options to choose from. For example, participants might be asked to decide whether the thief's t-shirt was blue or green. Although a comprehensive analysis of the advantages and disadvantages of the various memory tests is beyond the scope of the present manuscript, there is debate about what type of test is optimal, particularly in the context of eyewitness evidence, and the primary disadvantage of such categorical measures of memory is that they are relatively insensitive to small shifts in discriminability that fail to reach participants' criteria to change their categorical response (e.g., Wixted & Mickes, 2018).

In this study, we employ a different technique which may be able to detect small discrepancies in people's ability to distinguish between memory for the original event and memory for post-event misinformation. Rather than asking participants to decide between two competing items, we ask participants to make a categorical judgement about how confident they are that a particular item was present in the target video. For example, participants might be asked how certain they are that the thief's t-shirt was blue on a scale from 1 (certain this is false) to 6 (certain this is true). These ratings allow us to plot a receiver operating characteristic (ROC) curve, which provides a clearer indication of precisely how memory accuracy differs at varying confidence levels (Stanislaw & Todorov, 1999; Wixted & Mickes, 2018). The advantage of using this type of recognition test is that it may detect subtle changes in memory strength and bias when memory change does not meet the threshold for reporting misinformation as true (Stanislaw & Todorov, 1999). ROC curves enable us to detect reduced certainty that misinformation is false even when accuracy rates are not explicitly affected. For example, misinforming participants that the thief's t-shirt was blue may cause participants to be less certain that the thief's t-shirt was actually green. Although they may correctly report 'false' for the blue t-shirt, the effect of misinformation can be seen in the reduced certainty ratings.

Present Study

This study tests the effectiveness of a novel experimental paradigm for investigating misinformation effects in the context of media exposure to trauma using an ostensibly realistic, online, violent viral-style video as the target event, and a one-week retention interval. We also used more sensitive estimates of misinformation effects by measuring participants' confidence in their memory reports to determine discriminability (*d'*) and points on a ROC curve. To our knowledge, this is the first study to measure misinformation effects after one week using such a procedure.

Memory vividness and emotionality ratings have been examined in recent misinformation and trauma therapy studies (e.g., Calvillo & Emami, 2019; Houben et al., 2018; van Schie & Leer, 2019). To contribute to future research, a secondary aim of this study was to establish the minimum therapeutically meaningful change in ratings of memory emotionality and vividness across sessions. Although not elaborated on here, the findings from these analyses will help to inform a baseline of memory vividness and emotionality for Experiment Two.

Hypotheses

The following hypotheses were pre-registered and can be accessed at https://aspredicted.org/ma4st.pdf.

Hypothesis 1: Recognition Discrimination Accuracy

The first experiment of the present research explored the effect of exposure to post-event misinformation about a witnessed traumatic video on later endorsement of misinformation using novel stimuli and procedures. Misinformation effects were investigated using a between-subjects design, with two experimental conditions: misinformation and a no misinformation control. Two key measures of misinformation effects were used: d-prime (*d'*) and area under the receiver operating characteristic (ROC) curve. Consistent with decades of research demonstrating the effect of post-event misinformation on memory (e.g., Loftus, 2005; Loftus & Hoffman, 1989), I hypothesised that:

(a) Participants who receive misinformation about a traumatic video will have a lower d-prime value (indicating poorer ability to discriminate between misinformation and original event details at test) than participants who do not receive any misinformation.

(b) Participants who receive misinformation about a traumatic video will have a lower area under the ROC curve (indicating poorer accuracy in discriminating between misinformation and original event details) compared to participants who do not receive any misinformation.

As part of the signal detection analyses, response bias (*c*) was also measured to see whether there were group differences in bias towards responding true or false on the recognition test. Since participants in the misinformation condition were hypothesised to respond “true” more often to misinformation items than control participants, I predicted that:

(c) Participants who receive misinformation about a traumatic video will have a stronger bias towards responding “true”, indicated by a larger positive value of *c*, compared to participants who receive no misinformation about the traumatic video.

Exploratory Aims: Changes in Memory Emotionality and Vividness Ratings

Experiment One also explored changes in participants’ ratings of the emotional intensity and vividness of memory for a traumatic video across two time points: immediately after viewing the traumatic video (T1) and between five and ten days later in session two (T2). This was compared against participants’ ratings of perceived change in memory vividness and emotionality from T1 to T2 to explore the smallest change in vividness and emotionality noticeable for participants. A global transition method was used to establish the smallest effect size of interest for the change in both memory vividness and emotionality from T1 to T2 (Anvari & Lakens, 2019). This will indicate the smallest clinically meaningful effect size for changes in memory emotionality and vividness, which will be used as a baseline estimate in Experiment Two. Since these analyses are exploratory, no hypotheses will be made about the strength or direction of changes in memory emotionality and vividness for Experiment One.

Method

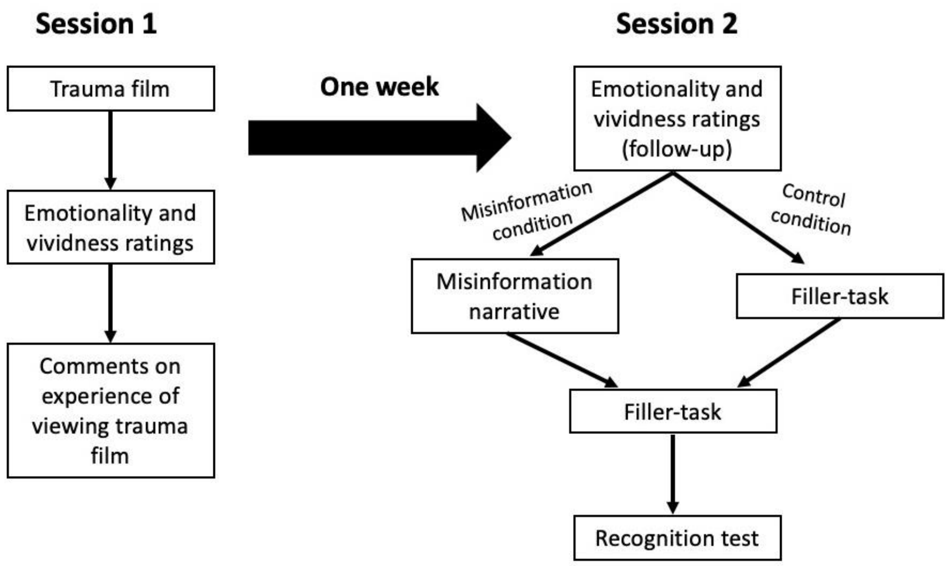
All procedures reported here involving human participants were in accordance with the ethical standards of the Massey University Human Ethics Committee and was deemed *low risk* (Notification number: 4000021787). Participants were informed of the purpose, procedures, and requirements of the study prior to participating. Informed consent was implied through participation in the study and all participants were debriefed as to the purpose of the study at the conclusion of each study. Preregistration information for this study can be accessed at: https://aspredicted.org/ma4st.pdf. The materials and data for this study are openly available on the Open Science Framework (OSF): https://osf.io/r48bm/?view_only=8137681d7e044d06a6bef57d4604b32f

Design

This study consisted of two sessions spaced approximately one week apart (see Figure 8). Session 1 was identical for everyone. In Session 2, half of participants were randomly assigned to the misinformation condition and half were assigned to the no-misinformation control condition. Memory vividness and emotionality ratings were compared across a 2(Condition: misinformation or control) × 2(Time: session 1 and session 2) mixed factorial design. We carried out an informal pilot test of the study material with a small group of students prior to collecting data. Feedback from the pilot study indicated the study was clear and plausible.

Figure 8

Schematic Representation of the Experimental Procedure for Session 1 and Session 2



Participants

Participation was restricted to those who were at least 18 years old. Potential participants were asked not to participate if they had a current or previous diagnosis of post-traumatic stress disorder, depression, or anxiety, or if they had been exposed to a traumatic or violent life event, particularly gun-related violence. We aimed to recruit 100 participants to complete both experimental sessions. This sample size was a compromise between collecting a larger sample size than in previous similar misinformation studies (e.g., Houben et al., 2018) and resource constraints.

One hundred and twenty-nine participants took part in Session 1 of the experiment. Data from 12 participants were excluded for not viewing the entire 10-minute video and data from a further 12 participants were excluded for failing both attention checks. Session 2 was made available to the remaining 105 participants 5-10 days after they completed Session 1. Ninety-nine participants completed Session 2. No participants expressed suspicion that incorrect details presented in the misinformation narrative were intentional or part of the study.

The final sample ($N = 99$) included 53 people (33 male, 20 female) in the misinformation condition and 46 (33 male, 11 female, 2 non-binary) in the control condition. The mean age of participants did not differ between the misinformation condition ($M = 28.3$, SD = 9.32) and the control condition ($M = 26.6$, SD = 8.31), $t(97) = 0.93$, $p = 0.36$. The mean retention interval between Session 1 and Session 2 also did not differ between the misinformation condition ($M = 5.49$ days, SD = 1.15) and control condition ($M = 5.35$ days, SD = 1.06), $t(97) = 0.64$, $p = 0.53$.

Measures/Materials

Trauma Film. In Session 1, participants viewed a 10-minute video comprised of excerpts from the film ‘Zero Day’ (Coccio, 2002) to temporarily induce mild feelings of distress similar to those experienced in reaction to witnessing a traumatic event. The film depicts a school shooting and the moments leading up to the shooting using ostensible home-video footage and security camera recordings. The video clip begins with two perpetrators introducing themselves to the camera and planning their attack on the school. The perpetrators are then shown entering the school and killing and tormenting multiple students via security camera recordings. The video ends with the perpetrators taking their own lives as an emergency dispatch officer pleads with them over the phone. An image of playing cards was inserted at the end of the video for attention check purposes, described later.

Vividness Rating Scale. Memory vividness was measured using a Visual Analogue Scale (VAS) during Session 1 and one week later in Session 2. In Session 1, participants rated how vivid (clear) their memory of the trauma film was on a scale from 0 (not vivid at all) to 10 (extremely vivid). At Session 2, participants rated memory vividness using the same scale. They also rated their perceived change in vividness from Session 1 using a 5-point scale with the following response options: (1) a lot more vivid, (2) a little more vivid, (3) the same, (4) a little less vivid, (5) a lot less vivid. We used this to determine the smallest effect size of interest regarding the change in memory vividness over time (Anvari & Lakens, 2019).

Emotionality Rating Scale. Memory emotionality was also measured using a VAS in Session 1 and 2. Participants rated how emotional their memory for the trauma film was on a scale from 0 (extremely negative) to 10 (extremely positive). At Session 2, participants rated their perceived change in memory emotionality from Session 1 to Session 2 on a 5-point scale, the same as described above for memory vividness.

Attention Checks. Two attention checks were included in Session 1 after participants rated the emotionality and vividness of the video. The first attention check was a directed query; participants were presented with a sliding scale and instructed to leave the question blank and not to click on the scale. The second attention check was a multiple-choice question asking participants what image appeared at the end of the trauma film. Response options included: a cartoon gun, playing cards, schoolbag, or penguin. The correct answer was *playing cards*. Participants who failed these attention checks were excluded from analyses.

Misinformation Manipulation. Misinformation was introduced to participants assigned to the misinformation condition in Session 2 through a 447-word fictional eyewitness narrative describing the events depicted in the traumatic video. The narrative contained 12 true statements about the video (e.g., ‘The emergency dispatch lady called out for *Andre* to pick up the phone’) and eight misinformation statements (e.g., ‘Andre was in the driver’s seat of the car and he was wearing a *blue t-shirt*’ instead of a *camouflage shirt*). Participants in the control condition did not receive any misinformation, but instead completed a filler-task for five minutes. In the filler-task, participants searched for thirteen words associated with ice cream flavours that were hidden among a 14 × 14 grid of letters. Participants dragged the computer mouse over the letters that formed a word to solve the word search puzzle. After five minutes, participants were automatically taken to the next task.

Recognition Test. Memory accuracy and susceptibility to misinformation was measured in Session 2 using a true/false recognition test with an associated confidence rating for each test item. The test comprised 24 statements about the traumatic video (e.g., “The blonde perpetrator’s name was Cal”, “During the shooting Andre tipped over a desk”), with eight statements directly referring to misinformation details presented in the misinformation narrative (e.g., “The dark-haired perpetrator’s name was *Chris*” instead of *Andre*, “The first gun was retrieved from the *backseat* of the car” instead of *boot*). The test also contained 4 false items (foil items) unrelated to misinformation presented in the narrative and 12 true statements about the video clip. Each statement was presented individually, and participants were not able to skip items or go back to previous items. Participants indicated how certain they were that each statement was either true or false on a 6-point scale ranging from (1) certain this is false, to (6) certain this is true. Responses of 1-3 for false items were counted as correct and responses of 4-6 for true items were counted as correct (hit). Incorrect answers to misinformation items indicated endorsement of misinformation (false alarm), with a higher number of these questions being incorrect indicating greater susceptibility to misinformation. We measured participants' ability to discriminate between misinformation items and true items using signal detection and receiver operating characteristic curve analyses, described below.

D-prime (d'). We used *d'* to measure participants' ability to accurately discriminate between true statements and misinformation statements at test. *d'* is derived from the signal detection theory (SDT) of recognition memory, which assumes recognition decisions are based on evidence strength of previously encountered items and new (misinformation or foil) items at test (Stanislaw & Todorov, 1999). Higher values of *d'* indicate a greater ability to accurately discriminate between true statements and misinformation statements. If misinformation effects are present in our study, and misinformation participants are poorer at discriminating between true statements and misinformation statements as hypothesised, *d'* should be smaller for the misinformation group than for the control group.

Response Bias (c). We used the SDT measure *c* to compare group differences in response bias. Response bias tells us whether there is a general tendency for participants to respond either "true" or "false" on test items (Stanislaw & Todorov, 1999). In our study, negative values of *c* represent a bias toward responding "true" and positive values of *c* represent a bias toward responding "false". More negative or positive values of *c* indicate a stronger bias toward responding "true" or "false", respectively. We expected there would be no difference in overall response bias between the misinformation group and the control group. However, if misinformation effects are present in our study, we would expect the misinformation group to show a greater bias toward responding "true" on misinformation items compared to the control group.

Receiver Operating Characteristic (ROC) Curves. ROC curves provide a non-parametric and atheoretical estimate of participants' ability to accurately discriminate between true statements and misinformation statements (Stanislaw & Todorov, 1999). ROC curves are produced by plotting hit rates against false alarm rates for all possible certainty ratings. Chance-level performance, where the hit rate is equal to the false alarm rate, is represented as a diagonal line. This occurs when participants are unable to discriminate between true items and misinformation items and rely on guessing. Good discrimination accuracy is represented by a curve that bows toward the left. The more the ROC curve bows toward the left, the greater the discrimination accuracy. Based on our hypothesis for misinformation effects, we expect the ROC curve for the control group to bow more toward the left than the ROC curve for the misinformation group. We also expected the curves would show similar hit rates for the misinformation and control groups, but the misinformation group would have higher false alarm rates than the control group.

Area Under the Curve (AUC). To quantify discrimination accuracy based on the ROC curves, we calculated AUC for the misinformation and control group ROC curves. AUC can be interpreted intuitively as the proportion of times in which participants correctly discriminate true statements from misinformation statements (Stanislaw & Todorov, 1999). AUC values typically range from 0.5 (chance-level recognition performance) to 1 (perfect recognition performance). Larger AUC indicates greater discrimination accuracy. Consistent with our misinformation effect predictions, we expect the control group will have a larger AUC than the misinformation group, representing poorer discrimination accuracy for the misinformation group.

Procedure

Participants were recruited via the online international participant pool Prolific (www.prolific.co), and the experiment was administered in Qualtrics (www.qualtrics.com). The experiment was advertised as a study of memory for traumatic events involving viewing a 10-minute video of a fictional school shooting and answering some questions about the video. The study was made available for participants to complete on a desktop computer or laptop, and for those whom English is their first language.

Session 1. Participants began by viewing the trauma video. If they did not watch the full video, they were asked to report the timepoint in which they stopped the video. After the video ended, participants rated the emotionality and vividness of the traumatic video and completed the two attention checks. For exploratory purposes, participants were then invited to comment on their experience of viewing the traumatic video. Finally, participants were thanked for their time, provided contact details for various mental health support services, and reminded they may be asked to participate in another session in 5 days' time. Session 1 took approximately 20 minutes to complete.

Session 2. Five days later, participants who completed Session 1 and passed the attention checks were invited to participate in Session 2. Participants were given five days to complete Session 2. They again rated the emotionality and vividness of their memory for the traumatic video and indicated their perceived change in emotionality and vividness from Session 1 to Session 2. Those assigned to the misinformation condition read the misinformation narrative, while those in the control condition completed a filler-task for 5 minutes. Following this, all participants completed a filler-task for a further 5 minutes before completing the recognition test. At the end of the session, participants were thanked for their time, debriefed as to the purpose of the study, and provided with contact details of mental health support services.

Results

We used the signal detection measure d-prime (*d'*) and receiver operating characteristic (ROC) curves to assess participants' ability to correctly distinguish between true statements and misinformation statements one week after viewing the traumatic video. We excluded non-misinformation false items from the analysis so we could compare true items directly to misinformation items. A flattening constant was applied to account for floor and ceiling effects for hit rates and false alarm rates. Standard calculations were used for *d'*, response bias (*c*), and ROC (Stanislaw & Todorov, 1999). We report 95% confidence intervals for Cohen's *d* effect size for each comparison. Post-hoc Bayes' factors for independent samples t-tests with a default Cauchy prior (0, .707) were used to determine the relative evidence in favour of the null or alternative hypothesis for each analysis. Where the assumption of equal variances was violated for between-group comparisons, a Mann-Whitney U test (*U*) was used.

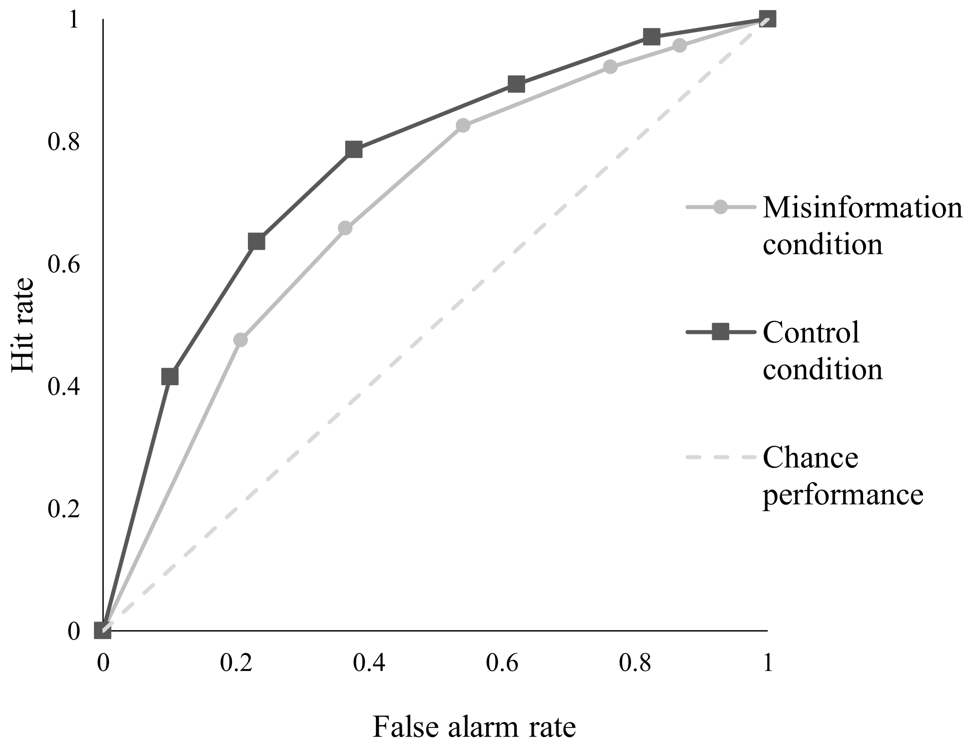
Results comparing discriminability (*d'*) for the misinformation and control groups showed a significant difference in correct discrimination between the misinformation (*M* = 0.21, SD = 0.56) and control group (*M* = 0.49, SD = 0.75), *t*(97) = -2.10, *p* = .04, Cohen's *d* = 0.42, 95% CI [0.02, 0.82]. These results suggest exposure to misinformation one week after viewing a traumatic video interfered with participants' ability to correctly distinguish between true and false statements at test. However, we found contradictory results for the post-hoc BF_{10} = 1.46, indicating only anecdotal evidence of this effect. We calculated *c* and AUC to see whether we could further clarify the effect of misinformation on discrimination accuracy.

Group comparisons for *c* showed participants in the misinformation group (*M* = 0.79, SD = 0.28) were slightly more biased towards responding "true" compared to control participants (*M* = 0.55, SD = 0.35), *U* = 699, $p < .001$, *d* = 0.76, 95% CI [0.35, 1.17]. The moderate-to-large effect size, and post-hoc BF_{10} = 91.4 indicates very strong evidence of the effect of misinformation on a bias towards responding "true". There are two possible explanations for this: first, the bias towards responding "true" may reflect misinformation effects; that is, misinformation group participants responded "true" to a greater proportion of misinformation items thereby inflating overall response bias. Second, exposure to misinformation after one week may have weakened participants' memory for the original event, leading them to agree with statements and respond "true" more often than disagreeing and responding "false" across all test items. We further examined response bias using ROC analyses.

In addition to signal detection measures, we used area under the ROC curve as a more sensitive measure of discriminability. We plotted hit rates against false alarm rates for both groups at each level of certainty (from "certain this is false" through to "certain this is true"). Figure 9 displays the ROC curves for the misinformation and control groups. The ROC curves further support the signal detection analyses above, with the control group demonstrating a greater bow to the left, moving further away from chance-level performance, and indicating superior discriminability compared to the misinformation group. The ROC curves also show similar hit rates at each certainty level for the misinformation and control groups. However, the misinformation group show higher false alarm rates than the control group. This suggests the bias toward responding "true" for the misinformation group can more likely be explained by a greater tendency for misinformation participants to respond "true" to misinformation statements than control participants. If it were the case that the misinformation group had an overall tendency to respond "true" across all test items, we would expect to see a greater effect on hit rates.

Figure 9.

A Comparison of Receiver Operating Characteristic (ROC) Curves Plotting Hit Rates Against False Alarm Rates Between Misinformation and Control Conditions



Note. The grey dotted line denotes chance performance, where the hit rate is equivalent to the false alarm rate. Each data point along the curve represents a different certainty level for recognition responses, ranging from “certain this is false” to “certain this is true”.

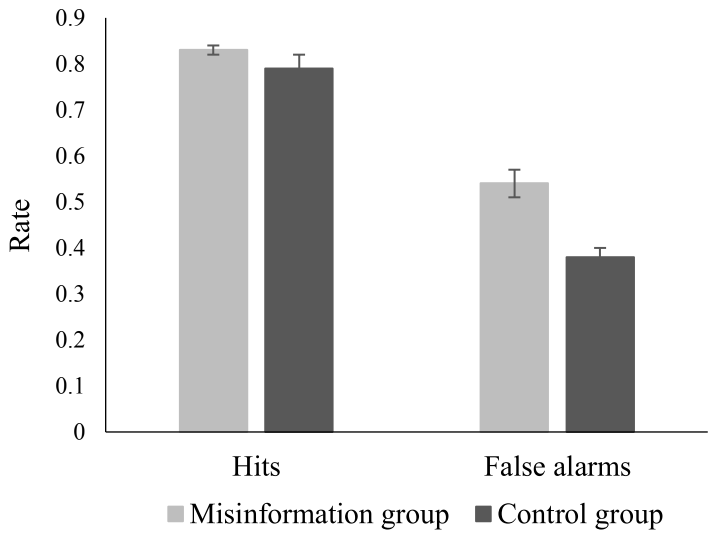
We calculated AUC for the misinformation and control group ROC curves using the inverse of the trapezoidal rule for AUC. This involved dividing the area above the curves into a series of trapezoids and summing the area of each trapezoid. We subtracted this sum from 1 to give the AUC. An independent samples *t*-test comparing AUC revealed a significant difference between groups in discrimination accuracy. Participants in the control group correctly determined whether a statement was true or false 77% (SD = 0.12) of the time when true statements and misinformation statements were presented during the recognition test. However, participants in the misinformation group correctly identified true and false items 70% (SD = 0.11) of the time, $t(97) = -2.91$, $p = .004$, $d = 0.59$, 95% CI [0.18, 0.99]. We found a post-hoc $BF_{10} = 8.34$, indicating the data is 8.34 times more likely under the alternative hypothesis (i.e., that participants in the misinformation condition are worse at discriminating between true and false statements) than the null. Although *d'* results showed some ambiguity, our AUC comparisons show a clear effect of misinformation on participants' discrimination accuracy after one week.

Exploratory Analyses

The analyses in this section were not pre-registered and are therefore exploratory. To explore whether differences in discriminability were due to endorsement of misinformation, we compared hit rates and false alarm rates for both groups. If differences can be attributed to the effects of misinformation, we would expect the misinformation group to have a higher false alarm rate than the control group, but no difference in hit rates between groups. Figure 10 displays the group comparisons of hit and false alarm rates. We found no significant difference in hit rates between the misinformation and control groups, $U = 1146$, $p = .60$. However, the false alarm rate was significantly higher for the misinformation group than the control group, $U = 659$, $p < .001$, $d = 0.91$. Post-hoc $BF_{10} = 1052$ suggests very strong evidence that between-group differences in discriminability between true and false statements can be attributed to inflated false alarm rates for participants exposed to misinformation.

Figure 10.

Recognition Test Hit and False Alarm Rates for the Misinformation Group and Control Group.



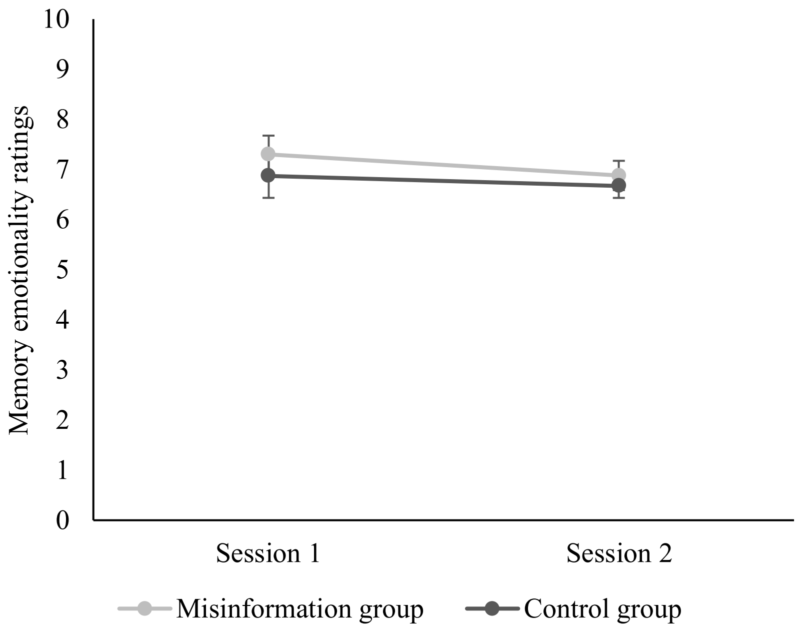
Note. Error bars demonstrate standard error of the mean (SEM) for hit and false alarm rates.

Emotionality and Vividness Ratings. We used a global transition method (Anvari & Lakens, 2019) to determine the smallest effect size of interest for changes in memory emotionality and vividness for the trauma video from Session 1 to Session 2. We conducted these analyses to determine a cut-off point for the smallest subjectively detectable change in memory vividness and emotionality that is therapeutically meaningful. Emotionality ratings were missing from 3 participants (2 misinformation, 1 control) and vividness ratings were missing from 1 participant in the misinformation group. Data from 96 participants were analysed for emotionality and 98 participants for vividness. Session 1 and Session 2 emotionality ratings were reverse coded so that higher ratings represented more negative emotionality.

Figure 11 shows the change in emotionality ratings from Session 1 to Session 2 for the two groups. A mixed-model ANOVA established no significant main effect of time, $F(1,94) = 0.84$, $p = .36$, $\eta^2 = 0.003$, or condition, $F(1,94) = 0.67$, $p = .42$, $\eta^2 = 0.005$. There was also no significant interaction between time and condition for participants' ratings of emotionality, $F(1,94) = 0.19$, $p = .67$, $\eta^2 = 0.001$. This suggests participants' memory for the school shooting video was equally negative one week after viewing the video as it was immediately after viewing the video.

Figure 11.

Change in Emotionality Ratings for the Traumatic Video from Session 1 to Session 2 for the Misinformation and Control Groups.



Note. Error bars demonstrate standard error of the mean (SEM) for ratings of memory emotionality.

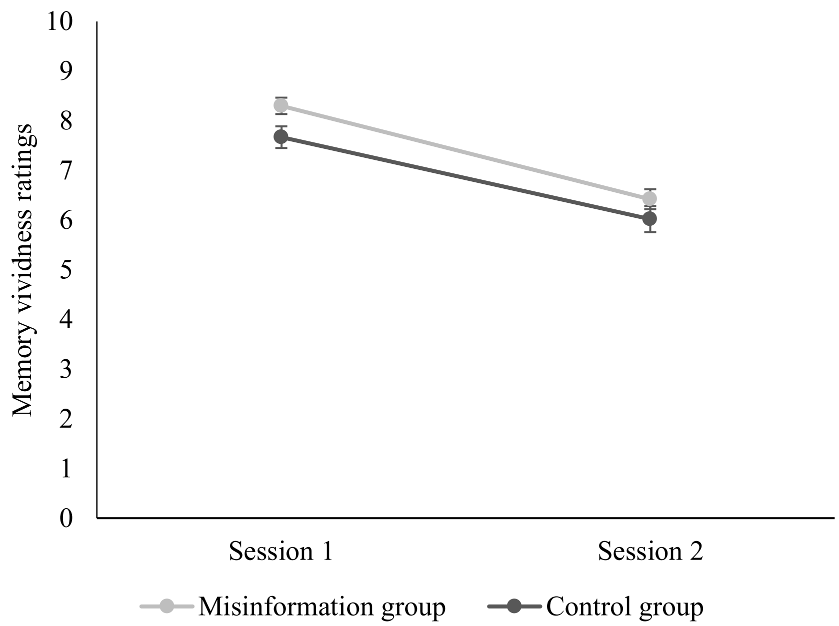
To determine the minimum therapeutically meaningful change in memory emotionality from Session 1 to Session 2, we grouped participants' responses into three categories: those reporting "no change" ($n = 71$), "a little change" ($n = 23$), or “a lot of change” (n = 3). We focused on participants who reported “a little more positive” or “a little more negative” emotionality at session 2. These ratings were combined to form the “little change” category. We conducted a Chi-Square test of independence to examine differences between conditions in the number of participants subjectively reporting “no change”, “a little change”, or “a lot of change” in memory emotionality. No significant differences were found between groups, suggesting no effect of misinformation conditions on perceived change in memory emotionality, $\chi^2(2, N = 97) = 0.81$, $p = .67$.

A paired-samples t-test found that, for participants who reported “a little change” in emotionality from Session 1 (M = 6.57, SD = 3.04) to Session 2 (M = 6.24, SD = 1.51), there was a non-significant mean decrease in emotionality of 0.33, $t(20) = 0.50$, $p = .63$, $d = 0.12$, 95% CI [-0.32, 0.54]. This suggests even for those reporting a subjective sense of change, memory emotionality remained stable over time. As a result, we were unable to calculate the smallest therapeutically meaningful effect for emotionality; however, we were able to establish a clear baseline for memory emotionality for the traumatic video over a one-week period.

Changes in ratings of memory vividness from Session 1 to Session 2 for the misinformation and control groups are displayed in Figure 12. We found a main effect of time, indicating an overall decrease in vividness for memory of the school shooting video from Session 1 to Session 2, irrespective of experimental condition, $F(1,96) = 126.53$, $p < .001$, $\eta^2 = 0.27$. There was also a main effect of condition; participants in the misinformation group tended to rate their memory for the school shooting video as more vivid than participants in the control group $F(1,96) = 4.79$, $p = .03$, $\eta^2 = 0.03$. However, the time × condition interaction was non-significant, $F(1,96) = 0.82$, $p = .37$, $\eta^2 = 0.002$.

Figure 12.

Change in Vividness Ratings for the Traumatic Video from Session 1 to Session 2 for the Misinformation and Control Groups.



Note. Error bars demonstrate standard error of the mean (SEM) for ratings of memory vividness.

Again, we grouped participants' responses into three categories: those reporting "no change" ($n = 26$), "a little change" ($n = 58$), or "a lot of change" ($n= 15$) in memory vividness. A Chi-Square test of independence examined differences between conditions in the number of participants reporting "no change", "little change", or "a lot of change" in memory vividness from Session 1 to Session 2. No significant differences were found between the misinformation and control conditions, $\chi^2(2, N = 99) = 1.77$, $p = .41$. We focused on participants who reported their memory was "a little more vivid" or "a little less vivid" at

Session 2. A paired-samples t-test found that, for participants who reported "little change" in memory vividness from Session 1 (M = 8.10, SD = 1.28) to Session 2 (M = 6.26, SD = 1.15), there was a mean decrease in vividness of 1.84 over time, $t(57) = 9.87$, $p < .001$, $d = 1.30$. Memory vividness decreased significantly more for the "little change" group than for the "no change" group, $t(82) = -2.19$, $p = .03$, $d = 0.52$. This suggests effect sizes of at least $d = 1.30$ represent a therapeutically meaningful change in memory vividness over time, with smaller effect sizes representing changes that are too small to be subjectively perceived.

Discussion

To our knowledge, this is the first study to test misinformation effects for a violent viral-style video following a one-week retention interval using an online platform. We found only anecdotal evidence that misinformation impaired discrimination accuracy when using *d'*. However, using area under the ROC curve, we established moderate evidence that participants who received misinformation were less accurate when discriminating between true statements and misinformation statements than those who received no misinformation. Our results are consistent with other studies finding an effect of post-event misinformation on memory accuracy and misinformation endorsement (e.g., Loftus et al., 1989; Takarangi et al., 2006).

Our findings highlight the added value of ROC analyses for detecting subtle changes in memory strength, even when misinformation may be below the acceptance threshold. Moreover, the medium effect sizes we found are particularly notable given that our study was conducted online, where the capacity for monitoring and control is often much lower than in traditional laboratory experiments.

This study extends our understanding of misinformation effects by using novel stimuli, procedures, and context. We elicited misinformation effects outside of a laboratory using a wholly online experiment and realistic target event to simulate the violent viral-style videos that people may come across online. Many participants commented on the realism of our video, likening it to the 2019 Christchurch mosque shootings and the 1999 Columbine High School massacre. Using an online format, misinformation effects were elicited from a diverse sample that may be more representative of the general population. Moreover, we showed the robustness of misinformation effects under relatively extreme experimental conditions—involving reduced experimental control, a high degree of participant autonomy and trust, and a one-week retention interval.

We also used more sensitive estimates of misinformation effects by measuring participants' confidence in their memory reports and using this to determine discriminability (*d'*) and points on a ROC curve. Signal detection has been used in many previous misinformation studies (e.g., Chan et al., 2017; Paz-Alonso et al., 2013; van Bergen et al., 2010); however, ROC curves are rarely employed in such research. Some argue that a key advantage is that ROC analyses are unaffected by the assumptions about recognition memory that underlie *d'* (Wixted & Mickes, 2018). Although the direction of effects in our study was consistent across analyses, ROC analyses provided stronger evidence of misinformation effects. This is because ROC curves enabled us to detect instances where misinformation reduced participants' certainty in a recognition judgement, even when they did not overtly accept the misinformation. Future research should consider employing ROC analyses as an additional measure to allow for detection of subtle changes in memory strength in misinformation effect research.

We also explored participants' ratings of vividness and emotionality for their memory of the traumatic video, both immediately after viewing and one week later. Irrespective of exposure to misinformation, participants rated their memory for the video as highly vivid and negative. Our results are consistent with other trauma film paradigm studies which have found that experimental analogues to traumatic events can produce intrusive and distressing memories (James et al., 2016). Our findings also contribute to research on the psychological effects of media exposure to traumatic events; although participants in our study were aware the traumatic video was fictional, they appear to have rated their emotions and the vividness of their memories as relatively intense. This provides some indication of the impact of media exposure to *real-life* traumatic events, such as the live-streamed footage of the Christchurch attacks, which is likely to be even greater.

Interestingly, participants rated their memory as being equally negative one week after initially viewing the video but experienced a small decrease in memory vividness over the one-week period. Our results are partially consistent with findings from similar studies, which have found reductions in memory vividness over time (Houben et al., 2018; van Schie & Leer, 2019). However, these studies also found memory emotionality became more positive over time, which we were unable to replicate. A possible explanation for this is that our video was more distressing than the staged car crash video used in previous studies, thereby we were able to produce more enduring emotional reactions.

Memory vividness tended to be higher for misinformed participants than control participants—across both sessions. According to prominent false memory and recognition decision theories (e.g., fuzzy-trace theory, source monitoring framework, and ballistic accumulator models), poorer memory vividness increases susceptibility to misinformation (Brainerd & Reyna, 2002; Brown & Heathcote, 2008; Johnson et al., 1993). Based on this assumption and our vividness results, we would expect the misinformation group to have better memory performance than the control group. However, we found the opposite; despite perceiving their memory as more vivid, the misinformation group had poorer discrimination accuracy than the control group. Our findings suggest memory vividness did not interfere with misinformation effects and demonstrates that misinformation effects can still occur for memories that are experienced as being relatively vivid.

One limitation of this online experiment is that we cannot control for extraneous variables as well in the online setting compared to a laboratory environment. Distraction during the encoding or testing phases may have affected some participants' performance on the recognition test. We attempted to minimise such effects by including multiple attention checks throughout the experiment. A second limitation of this study may be the objectively short interval of five minutes between exposure to misinformation and memory retrieval; however, the implications of this are largely unknown. Further studies should investigate whether the effect of misinformation on retrieval is affected by this interval. Additionally, although participants may have incorrectly responded to recognition test items based on their memory for the narrative, rather than the video, we believe this is unlikely for two reasons. First, we provided participants with clear instructions to answer the questions based on their memory for the video. Second, were participants responding based upon the narrative, we would have expected to see a much larger increase in acceptance of the misinformation items than the moderate effects we observed. Lastly, it is also possible that some participants searched YouTube for the target video between sessions or took notes during initial viewing since we told participants they would be asked questions about the video. However, this seems unlikely in our study, given that no participants achieved recognition accuracy greater than 91.7% at test. Furthermore, the fact we still found significant effects in our study highlights the robustness of misinformation effects.

Conclusions

This study tested the viability of a novel method for investigating misinformation effects in an online context. To our knowledge, this is the first study to investigate misinformation effects after 1 week using a fully online sample with ostensibly realistic, viral-style, traumatic stimuli. Using signal detection and area under the receiver operating characteristic curve, results showed participants exposed to misinformation one week after viewing a traumatic video were significantly worse at discriminating between true information and misinformation compared to participants not exposed to misinformation. Our results suggest misinformation effects can be established in an online experiment using candid violent viral-style video stimuli. This study also provides evidence for the validity of ROC analyses in misinformation research. We hope our novel materials can further contribute to understandings of memory, misinformation effects, and media exposure to trauma at a faster pace and with more diverse samples than previously achieved.

CHAPTER 6

Experiment Two: Aims and Hypotheses

Recent research has raised concerns about the effects of different therapeutic techniques, such as eye movements in EMDR therapy, on false memory reports (Houben et al., 2018). The premise of these concerns focuses on the notion that many trauma therapies aim to reduce the vividness and emotional intensity of trauma memories—and this reduction may leave memory vulnerable to misinformation effects. The present study aimed to investigate the misinformation effect potential of a computerised trauma intervention, known as Cognitive Bias Modification-Appraisal (CBM-App) training (Woud, Blackwell, et al., 2018; Woud et al., 2012). CBM-App training has the advantage of being easily distributed to a wide audience via computer and it does not require the costly input of a trained therapist (Woud, Blackwell, et al., 2018; Woud et al., 2012). Similar to eye movements, CBM-App training aims to change trauma memories by reducing negative appraisals or interpretations of the event. Hence, this study will investigate whether such changes in memory increase people's susceptibility to post-event misinformation.

Existing misinformation paradigms that have been used in previous studies are not well-suited to investigating memory for the types of traumatic events that may be of clinical attention. The present research employed novel experimental materials and procedures (developed in Experiment One) for inducing misinformation effects, including a new trauma film and longer retention interval. In addition, signal detection and ROC analyses were used to enable detection of subtle changes in memory strength after exposure to misinformation, where these changes may otherwise go unnoticed. Employing these materials and procedures means misinformation effects can be investigated in the intended context; that is, the impact of a computerised therapy on memory for traumatic material viewed online.

Experiment Two primarily addresses the question "can computerised positive CBM-App training reduce the vividness and emotional intensity of memories for a traumatic video without increasing susceptibility to misinformation?" This will be answered by employing the novel misinformation materials and procedures developed in Experiment One across two experimental conditions: positive CBM-App training and a post-trauma debrief-only control condition. Like the first experiment, Experiment Two will include two online experimental sessions spaced one-week apart. Broken down, Experiment Two is concerned with (1) the effects of positive CBM-App training on the vividness and emotionality of memory for a traumatic video, and (2) the effects of positive CBM-App training on acceptance of misinformation about a traumatic video.

The hypotheses for Experiment Two are divided into four main areas of interest; posttraumatic cognitive bias, recognition discrimination accuracy, memory emotionality ratings, and memory vividness ratings. Within each area, several main hypotheses are outlined to predict the effects of positive CBM-App training on the dependent variables described. The following hypotheses were pre-registered and can be accessed at: https://osf.io/acfm3

Hypothesis 1: Posttraumatic Cognitive Bias – Encoding Recognition Task

To test whether the positive CBM-App training successfully instilled a positive cognitive bias, all participants completed an encoding recognition task (manipulation check) during session 1 of the experiment. A positive cognitive bias will be considered present if participants recall neutral statements from the encoding recognition task as being more positive in meaning than neutral or negative in meaning. Given the effects of CBM-App training on posttraumatic cognitive bias demonstrated in previous research (Woud et al., 2012, 2013; Woud, Zlomuzica, et al., 2018), I hypothesised that participants in the CBM-App training condition will have more positive scores on the encoding recognition task than participants in the debrief-only control group.

Hypothesis 2: Recognition Discrimination Accuracy

The primary aim of Experiment Two was to investigate whether positive CBM-App training increases susceptibility to misinformation about a traumatic video compared to a debrief-only control condition. Similar to Experiment One, susceptibility to misinformation was measured using multiple signal detection indices; including d-prime, area under the ROC curve, hit rates, and false alarm rates. CBM-App training aims to change how traumatic events are interpreted and remembered; the tenets of false memory theories that suggest increased susceptibility to misinformation when original memory has been weakened (Brainerd & Reyna, 2002; Brown & Heathcote, 2008; Johnson et al., 1993; Tousignant et al., 1986). Consequently, I hypothesised the following:

(a) Participants who receive positive CBM-App training will have lower average d-prime scores, indicating poorer ability to discriminate between misinformation and original event details at test, compared to participants who receive the debrief-only control intervention.

(b) Participants who receive the positive CBM-App training will have a smaller area under the ROC curve, indicating poorer discrimination accuracy, compared to participants who receive the debrief-only control intervention.

(c) There will be no difference in hit rates for participants who receive the debrief-only control intervention and for participants who receive positive CBM-App training.

(d) The false alarm rate will be higher for participants who receive positive CBM-App training than for participants who receive the debrief-only control intervention.

As part of the signal detection analyses, response bias (*c*) was also measured to see whether there are group differences in bias towards responding true or false on the recognition test. Since participants who received positive CBM-App training were hypothesised to respond "true" more often to misinformation items than participants who receive the debrief-only control intervention, I predicted that:

(e) Participants who receive the positive CBM-App training intervention will have a stronger bias towards responding "true" across all items on the recognition test (indicated by a larger value of *c*) compared to participants who receive the debrief-only control intervention[1].

Hypothesis 3: Memory Emotionality Ratings

This experiment was also concerned with the effects of CBM-App training as a computerised therapy for reducing the emotional intensity of memories for a traumatic video. Ratings of memory emotionality were measured at three time points: immediately after participants viewed the traumatic video (T1), immediately after participants completed either the CBM-App training intervention or post-trauma debrief-only intervention (T2), and five to ten days later during session two (T3). Changes in memory emotionality ratings from T1 to T2 (immediate change) and T2 to T3 (follow-up change) were compared between groups. In line with evidence showing that positive CBM-App training reduces trauma-related distress (Woud et al., 2012, 2013; Woud, Zlomuzica, et al., 2018), I hypothesised that:

(a) There will be greater decreases from T1 to T2 in ratings of memory emotionality for participants who receive positive CBM-App training compared to participants who receive the debrief-only control intervention.

(b) There will be greater decreases from T2 to T3 in ratings of memory emotionality for participants who receive positive CBM-App training

[1] This prediction deviates from the pre-registered hypothesis. In the pre-registration, separate predictions were made about bias for true items and for misinformation items. However, it is not possible to calculate bias for these items separately. Therefore, in Hypothesis 2e I made a prediction that fits with the general pattern for bias that would be expected if CBM-App training increases susceptibility to misinformation.

compared to participants who receive the debrief-only control intervention.

Hypothesis 4: Memory Vividness Ratings

Lastly, Experiment Two investigated the effects of positive CBM-App training on the vividness of memories for a traumatic video. Ratings of memory vividness were measured at three time points: immediately after participants viewed the traumatic video (T1), immediately after participants completed either the CBM-App training intervention or post-trauma debrief-only intervention (T2), and five to ten days later during session two (T3). Changes in memory vividness ratings from T1 to T2 (immediate change) and T2 to T3 (follow-up change) were compared between groups. In line with findings demonstrating reductions in the frequency and/or intensity of trauma memory intrusions following positive CBM-App training (Woud et al., 2012, 2013; Woud, Zlomuzica, et al., 2018), I hypothesised that:

(a) There will be greater decreases from T1 to T2 in ratings of memory vividness for participants who receive positive CBM-App training than for participants who receive the debrief-only control intervention.
(b) There will be greater decreases from T2 to T3 in ratings of memory vividness for participants who receive positive CBM-App training than for participants who receive the debrief-only control intervention.

CHAPTER 7

Experiment Two: Method

Experiment Two aimed to investigate whether positive CBM-App training increases susceptibility to misinformation about a traumatic video. The method for testing the research hypotheses (outlined in Chapter 6) was adapted from previous research on misinformation effects in therapy (Calvillo & Emami, 2019; Houben et al., 2018; van Schie & Leer, 2019) and CBM-App training (Woud et al., 2012; Woud, Zlomuzica, et al., 2018). Methodological decisions were also made based upon current theoretical understandings of recognition memory (Stanislaw & Todorov, 1999; Wixted, 2007). This chapter describes the methods used to address the research aims for the second experiment of the present research, including details of the pre-registration, research design, and participant recruitment. The measures/materials section specifies the stimuli used to manipulate the independent variable and measure the dependent variables in this experiment. Lastly, the processes involved in the experiment are outlined in the procedure section.

Pre-registration

An online pre-registration was created prior to commencement of data collection to outline the methodology for the present experiment using the Open Science Framework (OSF; https://osf.io). The pre-registration included an a priori power analysis using G*Power version 3.1.9.4 (Faul et al., 2007) with a Cohen's *d* effect size of 0.42 and a power of .90 (two-tailed $\alpha = .05$), which indicated a sample size of 240 participants (120 per experimental condition) were required.

The effect size for this power analysis was informed by effect sizes established in Experiment One of the present research (see Chapter 5). Due to time restrictions, a cut-off point for data collection was decided upon for 5:00pm on 30th October 2020. The pre-registration also describes the research questions, hypotheses, data collection methods, and data analysis decisions for the present experiment. The pre-registration document was made public on 5th October 2020 and can be accessed at https://osf.io/acfm3.

Human Ethics

All procedures reported in this chapter were approved prior to data collection by the Massey University Human Ethics Committee: Human Ethics Northern Committee at their meeting held on Tuesday 21 July, 2020, Application NOR 20/24.

Design

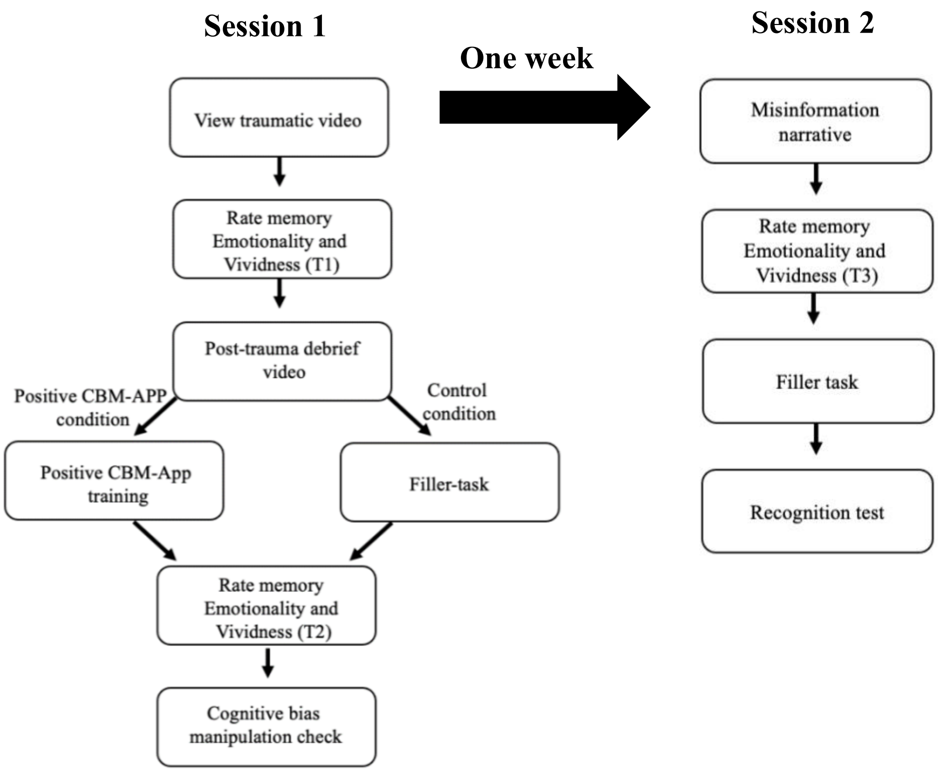
An experimental mixed design was used with two experimental sessions spaced between five and ten days apart. Participants were randomly assigned through the randomisation function in Qualtrics (Qualtrics, Provo, UT) to one of two conditions: positive CBM-App training or a post-trauma debrief-only control intervention. Dependent variables included ratings of memory vividness and emotionality, recognition discrimination accuracy between misinformation details and original event details (measured by d-prime, area under the ROC curve, false alarm rates, and hit rates), response bias on a recognition test (measured by *c*), and positive cognitive bias. Participants' attention was measured with two attention checks: the first attention check asked participants to identify what image was displayed at the end of the traumatic video. The second attention check asked participants not to click on the sliding scale presented on screen. Recognition discrimination accuracy, response bias, and positive cognitive bias were all post-test only design, and ratings of memory vividness and emotionality were both pre-test/post-test design across three time points (see Figure 13).

Participants

A convenience sample of 257 participants (140 male; 117 female; 0 non-binary) with a mean age of 32.55 years (SD = 12.30) were recruited for session 1 of the experiment through an invitation on Prolific (www.prolific.co). The experiment was available to Prolific users located in New Zealand, Australia, Canada, the United Kingdom, and the United States of America. Screening tools available through Prolific were used to ensure participants were at least 18 years old and did not have a diagnosed mental health condition that is uncontrolled (by medication or intervention) and which has a significant impact on their daily life/activities. Prior to being invited to take part in session 1, all eligible participants completed the Primary Care PTSD Screen for DSM-5 (PC-PTSD-5; Prins et al., 2004). Participants who responded “yes” to any of the questions on this measure were not eligible to participate in the present experiment. Details of the trauma film may be triggering for these individuals and could lead to elevated levels of distress beyond the scope of this study (Weidmann et al., 2009). Participants who took part in Experiment One (Chapter 5) were also excluded from participating in this experiment.

Figure 25.

Schematic Illustration of the Experiment Flow Showing Two Experimental Sessions Spaced One Week Apart.



Participants who were eligible and opted to take part in the study were randomly assigned to one of two conditions: positive CBM-App training or a post-trauma debrief-only control intervention. Thirty-six participants (CBM-App training n = 16; Control n = 20) were excluded from participating in session 2 due to failing attention checks or failing to view the full 10-minute trauma film. A further 11 participants (CBM-App training n = 2; Control n = 9) chose not to complete session 2 and were therefore excluded from analyses. Table 1 displays the participant demographics and the number of days between experimental sessions for each condition. There was no significant difference between groups in participants' age (see Table 1), $t(208) = 0.97$, $p = .34$. There was also no significant difference between groups in the number of days between session 1 and session 2 (see Table 1), $U = 5054$, $p = .19$.

Table 1

Summary of Gender, Age, and Retention Interval (in days) for Participants in the Positive CBM-App training Group and the Debrief-Only Control Group

		Condition	
		Positive CBM-App training	Debrief-only control
Demographics			
Gender	Female *n*	40	46
	Male *n*	69	55
	Non-binary *n*	0	0
	Total *n*	109	101
Age		34.00 (12.90)	32.40 (10.90)
Retention interval		5.65 (1.50)	5.33 (0.69)

Note. Gender rows report frequencies for each category. Age and retention interval rows report the mean and standard deviation in brackets.

Participants gave informed consent prior to commencing session 1. Participation in both sessions was voluntary, and participants received £5.00 for participating in session 1 and £2.50 for participating in session 2 (as per the recommended Prolific participant payment schedule). Recruitment ceased when 240 participants had agreed to participate in session 1 and 2.

Measures and Materials

The experiment was programmed using Qualtrics software, Version 9.10.20 and distributed online via Prolific (www.Prolific.co). Materials for the positive CBM-App training programme were adapted from Woud et al.'s (2012; 2018) studies with the authors' permission. All stimuli, questionnaires, and details of experimental manipulations can be accessed via this study's OSF page (https://osf.io/dp4wz/?view_only=ed7df207f34043228d0392405885699b).

Primary Care PTSD Screen

Prior to being invited to take part in session 1, all participants completed the Primary Care PTSD screen for *DSM-5* (PC-PTSD-5; Prins et al., 2004). The PC-PTSD-5 is a 5-item screening tool used to identify people with symptoms of PTSD. The measure begins by asking participants if they have had any exposure to traumatic events. Participants who indicated "yes" were then directed to five follow-up questions asking whether they have experienced various symptoms in the past month, including having nightmares about the traumatic event, avoiding reminders of the event, being easily startled, feeling numb, or feeling guilty. Participants selected either "yes" or "no" as a response for each item. For the purposes of the present research, participants who responded "yes" to any of these five items were ineligible to participate and were not invited to take part in the experiment.

Trauma Film

The trauma film used in this study was the same as in Experiment One of the present research. In session 1, participants viewed a 10-minute video comprised of excerpts from the film 'Zero Day' (Coccio, 2002) to temporarily induce mild feelings of distress similar to those experienced in reaction to witnessing a traumatic event. A detailed description of the film is provided in the method section for Experiment One (Chapter 5).

Vividness Rating Scale

Memory vividness was measured using a Visual Analogue Scale (VAS) at three time points: immediately after viewing the trauma film (T1), immediately after completing the experimental interventions (CBM-App training or debrief-only control) (T2), and five to ten days later during session 2 (T3). At each of these time points, participants rated how vivid (clear) their memory of the trauma film was on a scale from 0 (not vivid at all) to 10 (extremely vivid).

Emotionality Rating Scale

Memory emotionality was also measured using a VAS across three time points: immediately after viewing the trauma film (T1), immediately after completing the experimental interventions (CBM-App training or debrief-only control) (T2), and five to ten days later during session 2 (T3). At each of these time points participants rated how emotional their memory for the trauma film was on a scale from 0 (extremely negative) to 10 (extremely positive).

Attention Checks

Two attention checks were included in session 1 after participants initially rated the emotionality and vividness of the trauma film. The first attention check was a directed query; participants were presented with a sliding scale and instructed to leave the question blank and not to click on the scale. The second attention check was a multiple-choice question asking participants what image appeared at the end of the trauma film. Response options included: a cartoon gun, playing cards, schoolbag, or penguin. The correct answer was *playing cards*. Participants who failed one or both attention checks were excluded from analyses.

Intervention Tasks

Post-Trauma Debrief Video. All participants received a post-trauma debrief via a 5.5-minute video during session 1. The debrief video was designed specifically for the present research and was based on recommendations made by Whitworth (2016) regarding helpful information for people affected by a traumatic event. The video consisted of a voice over and accompanying visual animations providing information about common reactions to traumatic events, helpful coping strategies, and contact details for local mental health hotlines in Australia, Canada, New Zealand, United Kingdom, and United States of America. Coping strategies described in the debrief video included talking with friends and family, spending time doing enjoyable activities, having a regular routine, treating yourself with kindness, and a brief grounding exercise. The post-trauma debrief video can be accessed at this study's OSF page (linked above).

Positive CBM-App Training. The positive CBM-App training task that participants completed during session 1 was adapted from Woud et al. (2012; 2018). The task was comprised of a series of reappraisal-related sentences that were presented to participants as a sentence completion task. The sentences were not specifically related to the trauma film, but rather focused on participants' general appraisals of their ability to cope following a traumatic event. Each sentence ended in a partially presented word (fragment) which participants were asked to complete. There was only one possible grammatically-correct solution for each word fragment, and the solutions produced a sentence consistent with a positive reappraisal. Sentences were adapted from the 'Self' subscale of the Posttraumatic Cognitions Inventory (PTCI; Foa et al., 1999). Example items include "In a crisis, I predict my response will be h-lpf-l" (resolved as *helpful*) and "My reaction since the event shows that I am coping w-ll" (resolved as *well*).

For each trial, participants were shown an incomplete sentence and were instructed to type the first missing letter for the incomplete word using their computer keyboard and then click "next" to move to the next trial. The sentences were presented in eight blocks with three sentences individually presented within each block. An extra practice block was included at the beginning of the task to familiarise participants with the task. At the end of each block, participants were asked a comprehension question about one of the three sentences. For example, "Do you believe you will be able to respond in a useful way when there is a crisis?" Participants were instructed to respond yes or no based on their understanding of the sentence presented earlier. Comprehension questions were randomised so that the comprehension question corresponding to each sentence within a block was presented an equal number of times across all participants. A shortened version of the original training was used for time purposes, whereby only items with corresponding comprehension questions were used. In addition, eight neutral-valenced training sentences and nine positive sentences that did not fit with the trauma film were removed from the original CBM-App training.

Filler Task

Participants in the debrief-only control group completed a filler-task while participants in the experimental group received CBM-App training in session 1. This was to ensure similar session durations for both experimental conditions. All participants completed the filler-task in session 2 after reading the misinformation narrative to ensure a 10-minute time delay between exposure to misinformation and recognition testing. During the task, participants were presented with a list of 20 words and were instructed to identify all the words that belong in a specific category. For example, participants were given a list of 20 random words with eight months of the year scattered throughout the list. Participants were asked to identify the months of the year within the list by using their computer mouse to drag and drop the relevant words to a box presented on the screen. There were 12 trials of the filler-task during session 1 and seven trials during session 2. Each trial had a different list of words and different category for sorting through the word lists.

Cognitive Bias Manipulation Check

To test whether the positive CBM-App training intervention successfully induced a positive cognitive bias, participants completed a brief encoding recognition task. Participants were presented with ten ambiguous statements that had a neutral meaning (e.g., "Now that I have judged how I behaved during the traumatic event, I have a changed view of my own level of competence"). Each sentence was presented with a relevant title (e.g., for the example above "My view of my behaviour"). After each sentence was presented, participants were asked to imagine themselves in the situation and rate how vividly they imagined the situation on a scale from 0 (not vivid at all) to 10 (extremely vivid).

In the second phase of the task, participants were shown each of the ten sentence titles along with four new sentences for each title. Two of the new sentences were closely related to the original sentence for the title, but one sentence included a negative interpretation (e.g., "Having judged my own behaviour during the traumatic event, I now view myself as rather incompetent") and one sentence included a positive interpretation (e.g., "Having judged my own behaviour during the traumatic event, I now view myself as quite competent"). The remaining two sentences had a general positive/negative meaning but were only vaguely related to the original sentence. Participants rated how close in meaning each new sentence was to the original sentence on a 4-point scale (1 = very different in meaning, 4 = very similar in meaning). Positive cognitive bias was indicated by the degree to which the original ambiguous sentences were appraised as having a positive meaning.

Misinformation Manipulation

Misinformation was introduced to participants in session 2 (five to ten days after they viewed the trauma film). Misinformation was presented through the same 447-word fictional eyewitness narrative used in Experiment One, which described the events depicted in the traumatic video. The narrative contained 12 true statements about the video (e.g., “The boys were talking about their plan to attack their school, which they referred to as *Zero Day*”), and eight misinformation statements (e.g., “Another male victim managed to crawl out from under a desk, quickly grabbing his *cellphone* before escaping the room” instead of grabbing his *schoolbag*).

Recognition Test

Memory accuracy and susceptibility to misinformation was measured in session 2 using the same true/false recognition test used in Experiment One. The test comprised 24 statements about the traumatic video (e.g., “The blonde perpetrator’s name was Cal”, “During the shooting Andre tipped over a desk”), with eight statements directly referring to misinformation details presented in the misinformation narrative (e.g., “Andre was in the driver’s seat and he was wearing a *blue* t-shirt” instead of a *camouflage* t-shirt). The test also contained four false items (foil items) unrelated to misinformation presented in the narrative and 12 true statements about the video clip.

Each statement was presented individually, and participants were not able to skip items or go back to previous items. Participants rated how certain they were that each statement was either true or false on a 6-point rating scale ranging from (1) certain this is false to (2) certain this is true. Responses of 1-3 for false items were counted as correct and responses of 4-6 for true items were counted as correct (hit). Incorrect answers to misinformation items indicated endorsement of misinformation (false alarm), with a higher number of these question being incorrect indicating greater susceptibility to misinformation. Participants' ability to discriminate between misinformation items and true items was measured using signal detection and receiver operating characteristic curve analyses, described later in this chapter.

Procedure

The present study involved two experimental sessions spaced five to ten days apart. Both sessions were completed online on the participants' own computers using Qualtrics software. The following sections describe the processes involved during each session of the study.

Pre-screening

Prior to inviting participants to take part in session 1, all people who were interested in participating completed the PC-PTSD-5 screening measure online using Qualtrics software. The screening measure was distributed via Prolific. Participants were informed the screening measure was part of a larger study, and they were provided with an information sheet describing the main experiment. The information sheet invited participants to take part in a study on memory for a negative event viewed online, and included details about the task requirements, participant rights, confidentiality and privacy information, contact details of the researcher, and ethical integrity of the research. If participants were willing to proceed with the study, they were asked to indicate their consent by checking a box presented on the screen, which showed they understood the information provided and agreed to participate.

Participants were then automatically directed to the PC-PTSD-5 questionnaire. Participants who responded “yes” to any items on the PC-PTSD-5 were thanked for their time and made aware they were not eligible to participate in the main experiment. Participants who responded “no” to all items on the PC-PTSD-5 were subsequently invited to take part in session 1.

Session 1

At the beginning of the session, participants were given the opportunity to read the information sheet again and were asked to indicate their consent to participate in the study. During this process, participants were made aware that participation in the study involved viewing a 10-minute video of a fictional school shooting. They were told they could stop viewing the video at any time, without penalty, if the video became too distressing.

Participants who consented to participate in session 1 began the study by answering demographic questions. Participants’ unique Prolific ID was automatically recorded using Qualtrics software and participants were asked to check this was recorded correctly before viewing the trauma film. Following this, participants rated how emotional and vivid they found the trauma video and completed the two attention checks. All participants viewed the post-trauma debrief video, and then participants in the debrief-only control group completed the filler task while participants in the experimental group received positive CBM-App training. The CBM-App training task began by presenting instructions on the screen. Participants completed four practice trials before completing the main trials.

After completing the intervention tasks, all participants again rated the emotionality and vividness of their memory for the traumatic video and then completed the encoding recognition task (cognitive bias manipulation check). The encoding recognition task began by presenting instructions on the screen. Participants were given the opportunity to complete a practice trial of the task before completing the main trials. After completing the encoding recognition task, participants were given the opportunity to comment on their experience of participating in the study, including any issues they encountered. At the end of the session, participants were directed to a screen that displayed a thank you message and reminded participants they would be invited to take part in session 2 in five days' time. Participants were informed session 2 would remain open for five days. The researcher's contact details and phone numbers for local mental health hotlines in Australia, Canada, New Zealand, UK and USA were also listed on the final screen. Participants were paid and thanked for their time through the Prolific payment system.

Session 2

Participants who completed session 1 and passed both attention checks were invited via Prolific to complete session 2 five days later. Session 2 remained open to participants for five days; therefore, session 2 was completed five to ten days after participants completed session 1. Participants used their own computers to complete session 2 online using Qualtrics software. Again, participants were given the opportunity to read through the information sheet before beginning the experiment. Participants' unique Prolific ID was automatically recorded, and they were asked to check this was correct before continuing to the first task. This was to ensure participants' data from session 1 could be matched to their session 2 data.

Participants first read the misinformation narrative. They were instructed to read the narrative carefully and click "next" once they had finished reading. Participants could not continue to the next task until 90 seconds had elapsed to ensure participants read the narrative. Next, participants rated the emotionality and vividness of their memory for the trauma film they viewed during session 1. After completing a filler task for approximately 10 minutes, participants completed the recognition test for their memory of the trauma film. They were informed they would be presented with some statements about the film and were asked to rate how confident they were that each statement is true or false on a 6-point scale. Participants were instructed to respond as accurately as possible based on what they remembered from the video.

At the end of the session, participants were again given the opportunity to comment on their experience of participating in the study, including if they encountered any issues. They were then automatically directed to a screen that had a brief thank you message and explained the purpose of the experiment. Contact details of the researcher and phone numbers for local mental health hotlines were listed again, and participants were encouraged to seek support if they felt any distress after completing the study. Lastly, participants were informed they could access a summary of the findings from the present research by saving a link to a Google document. Participants were paid and thanked for their time through the Prolific payment system.

CHAPTER 8

Experiment Two Results

This chapter describes the results of Experiment Two outlined in the previous chapter. The first two sections explain the processes used to prepare the data for analysis and check assumptions of the parametric tests outlined in the pre-registration. The third section outlines the criteria used for making decisions about the statistical and practical significance of results from inferential tests. In the fourth section, patterns that emerged from the data are described, along with the results of confirmatory tests used to investigate the hypotheses outlined earlier in this chapter. The results chapter ends with the results of exploratory analyses examining additional patterns in the data that were not pre-registered.

Data Preparation

Raw data collected during sessions 1 and 2 were stored in a Microsoft Excel spreadsheet, downloaded via Qualtrics software (Qualtrics, Provo, UT). Data from 47 participants were excluded due to failing attention checks (n = 34) or failing to complete both sessions of the experiment (n = 13). Indices were created for memory discrimination accuracy, response bias, and cognitive appraisal bias, as outlined in the following subsections.

Memory Discrimination Accuracy and Response Bias

For the memory discrimination accuracy indices, standard signal detection procedures were used to calculate d-prime (Stanislaw & Todorov, 1999). Adjusted hit rates and false alarm rates were used in the d-prime and response bias calculations. The adjusted hit rate for correctly-identified items on the recognition test was calculated using the loglinear approach by dividing the total number of "true" test items + 1 by the number of hits + 0.5 as a flattening constant[2]. Hits were classified as confidence ratings > 3. The adjusted false alarm rate for misinformation items incorrectly identified as "true" was calculated using the loglinear approach by dividing the total number of misinformation items + 1 by the number of false alarms + 0.5 as a flattening constant. False alarms were classified as confidence ratings > 3.

Area under the ROC curve was calculated using the inverse of the trapezoidal rule (Yeh, 2002): The area above each curve was divided into multiple trapezoids and the area of each trapezoid was summed. This sum was subtracted from 1 to give the area under the ROC curve for each experimental group. Response bias (*c*) was also calculated using standard signal detection procedures (Stanislaw & Todorov, 1999).

Cognitive Appraisal Bias

The cognitive appraisal bias index indicated the degree to which participants appraised ambiguous sentences as having positive or negative meaning during the encoding recognition task. This was calculated using the same method employed by Woud et al. (2012; 2018); by subtracting the mean ratings for negative targets from positive targets on the encoding recognition task.

[2] The purpose of the flattening constant was to account for floor and ceiling effects for hit and false alarms (i.e., where hits or false alarms are equal to 0 or 1.

Positive cognitive bias was indicated by a positive score on this index, and negative cognitive bias was indicated by a negative score on this index.

Memory Emotionality and Vividness Ratings

Emotionality ratings at T1, T2, and T3 were reverse coded for ease of interpretation, so that higher ratings corresponded to more negative emotionality. Six new variables were created to show the change in memory vividness and memory emotionality across the three time points at which participants provided ratings. One variable each for vividness and emotionality were created for changes between T1 and T2; one variable each for changes between T2 and T3; and one variable each for the overall changes between T1 and T3.

Means, standard deviations, and confidence intervals were calculated for post-trauma cognitive bias, d-prime (*d'*), response bias, area under the ROC curve, emotionality ratings, and vividness ratings for both experimental groups.

Assumption Checks

Assumption checks were carried out prior to confirmatory analyses to determine the appropriate parametric tests for analysing the data. The parametric tests used to address the research hypotheses included: (1) independent samples t-tests to compare group differences in post-trauma cognitive bias, d-prime, response bias, hit rate, false alarm rate, and area under the ROC curve, and (2) mixed-model Analysis of Variance (ANOVA) to compare group differences in memory emotionality and memory vividness at T1, T2, and T3.

The assumption of normality was assessed for each dependent variable (DV) using visual inspection of boxplots and the Shapiro-Wilk test of normality. The boxplots for each DV are displayed in Figures F1-F12, Appendix F. Table 2 shows the results of the Shapiro-Wilk tests of normality for the cognitive bias index, d-prime, response bias, hit rate, false alarm rate, area under the ROC curve, and memory emotionality and vividness ratings. The assumption of normality was met for the cognitive bias index. Results of the Shapiro-Wilk tests and visual inspection of the boxplots suggest the assumption of normality was violated for all other variables. Therefore, Mann-Whitney *U* tests were used to compare group differences in d-prime, response bias, hit rate, false alarm rate, and area under the ROC curve.

Shapiro-Wilk *p*-values for ratings of emotionality and vividness at T1, T2, and T3 were also all below .05, indicating violation of normality. Since the mixed-model ANOVA is a robust test against the assumption of normality (Glass et al., 1972; Harwell et al., 1992), no corrections were made to account for non-normality of ratings of memory emotionality and vividness. However, Mann-Whitney *U*-tests were used to compare group differences in the changes in memory emotionality and vividness over time (change scores from T1 to T2, T2 to T3, and T1 to T3).

Table 2

Results of the Shapiro-Wilk Tests of Normality for the Dependent Variables.

Variables		W	p
Cognitive bias index		0.99	.54
d-prime		0.98	.01
Response bias (*c*)		0.97	<.001
Hit rate		0.86	<.001
False alarm rate		0.97	<.001
Area under the ROC curve		0.99	.04
Memory emotionality	T1	0.85	<.001
	T2	0.92	<.001
	T3	0.97	<.001
Memory vividness	T1	0.91	<.001
	T2	0.95	<.001
	T3	0.96	<.001

Note. A *p*-value < .05 indicates a violation of the assumption of normality.

Homogeneity of variances was tested using Levene's test for equality of variances. Table 3 shows that the assumption of homogeneity was met for all of the dependent variables. The assumption of sphericity was met for memory emotionality ratings at T1, T2, and T3, Mauchly's W = 0.74, $p < .001$. Sphericity was also found for memory vividness ratings at T1, T2, and T3, Mauchly's W = 0.80, $p < .001$.

Table 3

Results from Levene's Test of Equality of Variances for the Dependent Variables

Variable		F	p
Cognitive bias index		2.56	.11
d-prime		0.42	.52
Response bias (c)		0.41	.52
Hit rate		1.03	.31
False alarm rate		0.63	.43
Area under the ROC curve		2.13	.15
Memory emotionality	T1	0.13	.72
	T2	2.59e-8	1.00
	T3	0.54	.46
Memory vividness	T1	0.33	.57
	T2	0.25	.62
	T3	0.31	.58

Note. Homogeneity of variances satisfied if $p > .05$; df= (1, 208) for all dependent

Inference Criteria

Null hypothesis testing was used to compare the effects of the two levels of the independent variable (positive CBM-App training and debrief-only control) on the dependent variables (cognitive bias, recognition discrimination accuracy [d-prime and area under the ROC curve], response bias, memory emotionality ratings, and memory vividness ratings). Null hypotheses (H_0) represented no effect of the positive CBM-App training intervention on the DVs compared to the debrief-only control intervention. Alternative hypotheses (H_1) represented an effect of the positive CBM-App training intervention on the DVs. As outlined in the pre-registration, statistical significance was observed at $p < .05$, therefore H_0 was rejected when $p < .05$. However, H_0 cannot be rejected when $p > .05$ because this only indicates *insufficient* evidence that there is an effect present, rather than demonstrating evidence in *support* of H_0.

To overcome this limitation of null-hypothesis testing, Bayes factors (BF) were used to compare the relative evidence in support of H_0 and H_1. BFs quantify the likelihood of the data under the null hypothesis (which presumes zero effect of the IV on the DV) or the alternative hypothesis (which presumes a non-zero effect of the IV on the DV) (Williams et al., 2017). A hypothesis is considered more credible when the observed data is more likely under one hypothesis than the other. For the present research, BF_{10} quantifies the likelihood of the data under H_1 relative to H_0. BF_{10} greater than 1 indicates evidence in favour of the alternative hypothesis. BFs further from 1 provide stronger evidence, while BFs closer to 1 indicate ambiguous evidence, which means there is reduced certainty that the alternative hypothesis is supported more than the null hypothesis (Wagenmakers et al., 2015). On the other hand, BF_{10} less than 1 indicates evidence in favour of the null hypothesis, with smaller BFs providing stronger evidence in favour of H_0. In the present research, $BF_{10} > 3$ was considered moderate evidence in favour of the alternative hypothesis and $BF_{10} < 0.30$ was considered moderate evidence in favour of the null hypothesis (Jeffreys, 1961).

Unlike frequentist null-hypothesis significance testing (NHST) approaches, BFs take into account prior information regarding the effect under investigation. This requires the researcher to make decisions about which effect sizes are more or less likely if the true effect is non-zero; referred to as the ‘prior’ (Williams et al., 2017). The present experiment employs a default Cauchy prior (0, 0.707) which presumes there is a 50% chance that the effect of the IV on the DV is larger than $d = 0.707$ in absolute value if we assume the true effect is non-zero. A default prior was used in the present research since, to the researcher’s knowledge, this is the first study to examine misinformation effects in CBM-App training in an online experiment.

A key advantage of the Bayesian approach is that BFs allow us to calculate the likelihood that either the null or alternative hypothesis is correct given the observed data (Williams et al., 2017). Importantly, BFs also enable us to determine the *strength* of evidence in favour of one hypothesis over another. Traditional NHST approaches simply dichotomise effects into either “significant” or “nonsignificant”, which provides no indication of the *extent* to which an effect is likely to be ‘true’.

A Bayesian approach also affords the opportunity to investigate hypotheses predicting zero effect (Williams et al., 2017). This is important in the present experiment, as two research hypotheses make predictions of no difference between groups (i.e., for hit rates and response bias on ‘true’ items). To test these hypotheses, we need to be able to quantify the extent to which the null hypothesis is supported by the observed data. This cannot be achieved using a frequentist NHST approach because *p*-values can only provide evidence against the null hypothesis, not in support of it (Wagenmakers, 2007).

Another advantage of BFs is that they do not reject the null hypothesis as readily as *p*-values, and are therefore less vulnerable to Type 1 error. This is because BFs can take into account whether the alternative hypothesis provides a fit that is just as poor, or worse, than the null hypothesis (Verhagen & Wagenmakers, 2014; Wagenmakers, 2007). BFs are also independent of the researcher's intentions of data collection, thereby minimising opportunity for 'data fishing' (Wagenmakers, 2007).

Region of practical equivalence (ROPE) analyses quantified the size and direction of effects in the present experiment. The ROPE defines a small range of effect sizes that are considered to be practically equivalent to zero (Kruschke & Liddell, 2018; Rouder et al., 2018). In this experiment, effect sizes (δ) within the range -0.2 to 0.2 were considered to be practically equivalent to no effect. Each ROPE was compared against a 95% highest density interval (HDI), which specifies the effect size values for the 95% most credible effect sizes. Effect sizes that fall within the 95% HDI are considered to have higher credibility than effect sizes that fall outside of the 95% HDI (Kruschke & Liddell, 2018). The 95% HDI indicates the degree of certainty about a particular effect size estimate. A higher degree of certainty is attained when the 95% HDI is spread over a narrow range of effect size values (Kruschke & Liddell, 2018).

Decisions regarding the size and direction of each effect were determined by the amount of overlap between the ROPE and 95% HDI. For each comparison, the null hypothesis was supported if the entire 95% HDI fell within the defined ROPE, demonstrating that all of the 95% most credible values are practically equivalent to no effect. The alternative hypothesis was supported if the entire

95% HDI fell outside the defined ROPE, demonstrating that all of the 95% most credible values are practically equivalent to a nonzero effect (Kruschke & Liddell, 2018). Where only some of the 95% HDI fell inside the ROPE, Kruschke and Liddell (2018) recommend remaining undecided about the size and direction of the effect.

Hypothesis Testing

This section describes the results of the confirmatory data analyses used to test the research hypotheses specified in Chapter 6. The analyses described in this section were pre-registered prior to data collection, which can be found at https://osf.io/acfm3. The confirmatory results for the present experiment are divided into 4 sections according to the key research hypotheses: posttraumatic cognitive bias, recognition discrimination accuracy, memory emotionality ratings, and memory vividness ratings.

Posttraumatic Cognitive Bias

The first aim of the present experiment was to test whether the positive CBM-App training intervention successfully instilled a positive posttraumatic cognitive bias. I predicted that participants who received positive CBM-App training would have a greater positive cognitive bias (indicated by a more positive score on the encoding recognition task) than participants who received the debrief-only intervention (H_1).

In line with the predicted effect, an independent samples t-test showed participants who received CBM-App training had more positive cognitive bias scores (*M* = 8.55, SD = 5.45) than participants who received the debrief-only control intervention (*M* = 4.69, SD = 4.89), $t(208) = 5.38$, $p < .001$, Cohen's d = 0.74. Post-hoc BFs (with a default Cauchy prior of 0.707 on effect size on the alternative hypothesis) compared the relative evidence in favour of the null or alternative hypotheses. As expected, the BF_{10} = 62,167 indicated very strong evidence in favour of the alternative hypothesis that participants who received CBM-App training would have a stronger positive cognitive bias than participants who received the control intervention.

The effect was further supported by results from a ROPE analysis with a 95% HDI (0.44, 1.00), which found that none of the 95% most credible effect sizes were practically equivalent to zero effect. This presents very strong evidence to accept the alternative hypothesis that the CBM-App training intervention led to a greater positive cognitive bias than the control intervention. Therefore, the cognitive bias manipulation was successful in instilling a positive posttraumatic cognitive bias.

Recognition Discrimination Accuracy

Given that CBM-App training caused a positive shift in participants' post-trauma appraisals, I sought to address whether this shift might impact the accuracy of participants' memory for the trauma film. Specifically, I examined whether CBM-App training increased participants' susceptibility to post-event misinformation during a recognition test. Between-groups differences in recognition discrimination accuracy were tested using signal detection measures;

including d-prime (*d'*), area under the ROC curve, bias (*c*), hit rates, and false alarm rates.

Several predictions were made regarding the effect of CBM-App training on recognition discrimination accuracy. Firstly, I hypothesised participants who received CBM-App training would have poorer discrimination accuracy (indicated by lower *d'* and a smaller area under the ROC curve) compared to participants who did not receive CBM-App training. Secondly, due to the hypothesised effect of CBM-App training on misinformation acceptance, I expected participants in the CBM-App training group would have a stronger bias towards responding "true" (higher value of *c*) on the recognition test compared to participants in the control group. Lastly, I predicted between-group differences in recognition discrimination accuracy would be due to higher false alarm rates (i.e., acceptance of misinformation) for participants who received CBM-App training than for participants in the control group. In line with this, I expected there would be no between-group differences in hit rates.

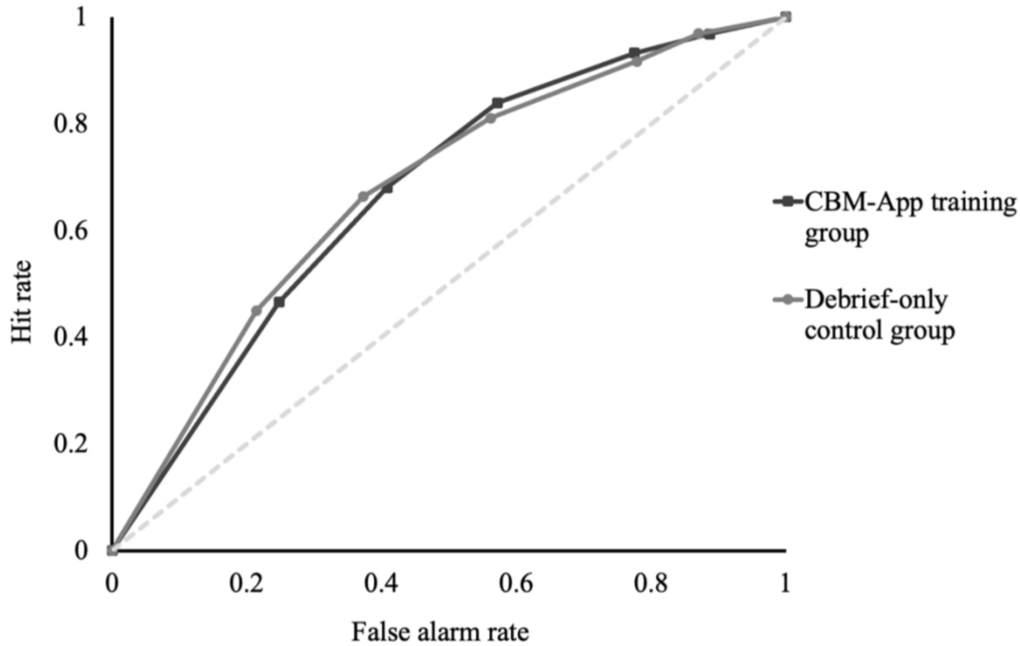
D-prime (d'). Contrary to the predicted effect, results comparing discriminability (*d*') showed there was no significant effect of CBM-App training on participants' ability to discriminate between misinformation details and original event details $U = 5121$, $p = .38$, Cohen's $d = .12$, 95% CI [-0.16, 0.39]. Although no statistically significant difference was found, participants who received CBM-App training were descriptively better at discriminating between misinformation details and original event details ($M = 0.81$, SD = 0.60) than participants who did not receive CBM-App training ($M = .74$, SD = 0.59).

A post-hoc BF (with a default Cauchy prior of 0.707 on effect size) was calculated to further clarify the effect of CBM-App training on discriminability. The between group comparison established $BF_{10} = 0.211$, indicating the observed data is approximately five times more likely under the null hypothesis than under the alternative hypothesis. Jeffreys (1961) suggests a BF of this magnitude can be interpreted as moderate evidence in favour of the null hypothesis; that is, there is no difference in recognition discrimination accuracy between the CBM-App training group and control group. A ROPE analysis with a 95% HDI (-0.25, 0.03) found that most of the 95% most credible effect sizes were practically equivalent to zero effect. Therefore, the BF for this comparison was not supported as the ROPE analysis indicated insufficient evidence to accept or reject the null hypothesis.

Receiver Operating Characteristic (ROC) Curves. To further clarify the effect of CBM-App training on recognition discrimination accuracy, ROC curves were used as a more sensitive estimate of discriminability. Hit rates were plotted against false alarm rates for the CBM-App training group and the control group at each level of certainty (from “certain this is false” through to “certain this is true”). Figure 14 displays the ROC curves for the CBM-App training group and the debrief-only control group. The ROC curves demonstrate a pattern consistent with results from the above signal detection analyses. The two curves are close together, indicating both groups had a similar ability to discriminate between misinformation details and original event details. The curves also indicate both groups performed better than chance level at discriminating between misinformation details and original event details, though discriminability was relatively low for both groups. Exploratory analyses (described later in the results section) were conducted to test for potential floor effects on the recognition test by comparing participants' performance to chance-level.

Figure 45.

A comparison of receiver operating characteristic (ROC) curves plotting hit rates against false alarm rates between the CBM-App training group and the debrief-only control group



Note. The grey dotted line denotes chance performance, where the hit rate is equivalent to the false alarm rate. Each data point along the curve represents a different certainty level for recognition responses, ranging from "certain this is false" to "certain this is true".

The area under each ROC curve was calculated to quantify discrimination accuracy for the CBM-App training group and debrief-only control group. Consistent with the signal detection analyses, a Mann-Whitney *U*-test comparing area under the ROC curve for the CBM-App training group (M = 0.68, SD = 0.13) and debrief-only control group (M = 0.68, SD = 0.11) revealed there was no significant difference between groups in discrimination accuracy, $U = 5445$, p =.89, $d = 0.002$, 95% CI [-0.27, 0.27]. Participants who received the CBM-App training and participants who only received the debrief-only control intervention correctly determined whether a statement was true or false 68% of the time when true statements and misinformation statements were presented during the recognition test.

A post-hoc $BF_{10} = 0.15$ (with a default Cauchy prior of 0.707 on effect size) for the between-groups comparison of area under the ROC curve indicated moderate evidence in favour of the null hypothesis; that is, there is no difference between groups in recognition discrimination accuracy. This was further confirmed by a ROPE analysis with a 95% HDI (-0.11, 0.14), which found that all of the 95% most credible effect sizes were practically equivalent to zero effect. Therefore, there is sufficient evidence to accept the null hypothesis that the CBM-App training intervention had no effect on participants' discrimination accuracy compared to the control intervention.

Response Bias (c). Estimates of response bias (*c*) were calculated for each group to determine whether the groups differed in participants' tendency to systematically respond "true" or "false" on the recognition test. Since participants in the CBM-App training group were expected to respond "true" to misinformation items more frequently than participants in the control group, it was predicted the CBM-App training group would have a stronger positive bias.

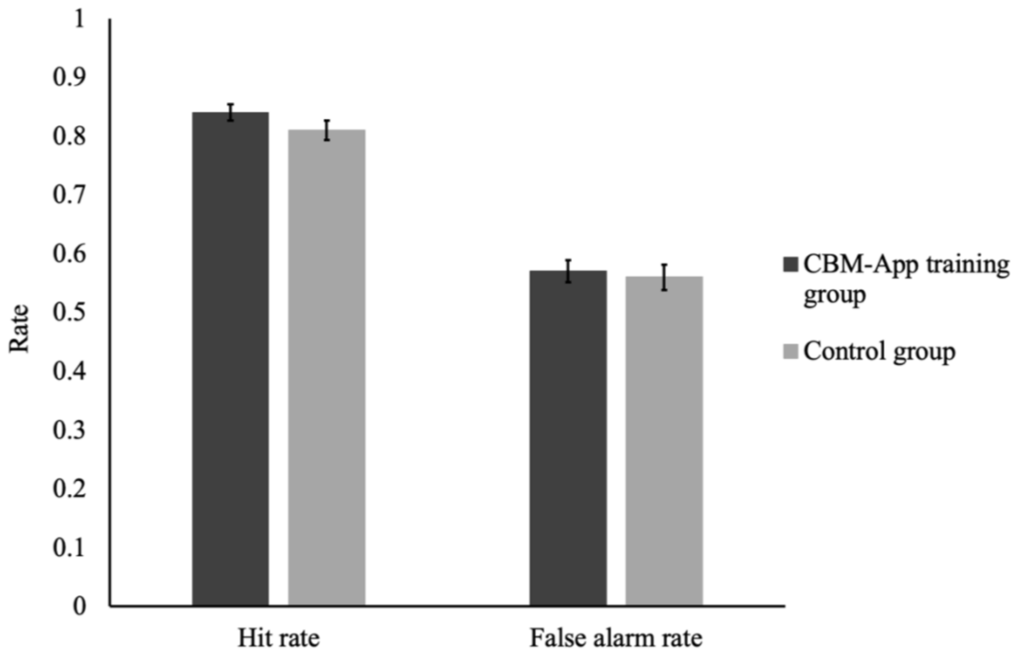
Results showed participants in the CBM-App training group ($M = 0.59$, SD = 0.42) and the control group ($M = 0.52$, SD = 0.48) were biased towards responding “true” across all test items. However, there was no difference between the groups in participants’ tendency to respond “true” across all items on the recognition test, $U = 5189$, $p = .47$, $d = 0.15$, 95% CI [-0.12, 0.42]. A post-hoc $BF_{10} = 0.26$ (with a default Cauchy prior of 0.707) established moderate evidence in favour of the null hypothesis; that is, there is no difference between groups in response bias on the recognition test. However, this finding was not supported by a ROPE analysis with a 95% HDI (-0.26, 0.05), which showed only some of the 95% most credible effect sizes were practically equivalent to zero effect. Therefore, there is insufficient evidence to accept or reject the null hypothesis for the effect of CBM-App training on response bias.

Hit and False Alarm Rates. The discriminability measures described so far take into account both the hit rate (true items correctly identified as true) and false alarm rate (misinformation items incorrectly identified as true). To determine whether any difference in discriminability can be attributed to increased acceptance of misinformation, false alarm rates and hit rates for each group were compared. If CBM-App training increased participants’ susceptibility to misinformation, we would expect to see a higher false alarm rate for the CBM-App training group, but no difference between groups in hit rates.

Figure 15 displays the hit rates and false alarm rates for the CBM-App training group and the debrief-only control group. Participants in the CBM-App training group had a descriptively higher hit rate than participants in the debrief-only control group, though this difference was non-significant, $U = 4893$, $p = .16$, $d = 0.19$, 95% CI [-0.08, 0.46]. As predicted, a post-hoc $BF_{10} = 0.36$ (with a default Cauchy prior of 0.707) indicated weak-to-moderate evidence in favour of the null hypothesis; that is, there is no difference between groups in hit rates on the recognition test. However, a ROPE analysis with a 95% HDI (-0.35, 0.01) was unable to definitively support the null hypothesis, as only some of the 95% most credible effect sizes were practically equivalent to zero effect. This means there is insufficient evidence to accept or reject the null hypothesis for the hit rates.

Figure 65.

Recognition test hit rates and false alarm rates for the CBM-App training group and debrief-only control group



Note. Error bars demonstrate standard error of the mean (SEM) for hit and false alarm rates.

Contrary to the predicted effect, false alarm rates (or the rate at which participants accepted misinformation statements as "true") were very similar for the CBM-App training group and the debrief-only control group, $U = 5400$, $p = .81$, $d = 0.05$, 95% CI [-0.22, 0.32]. Post-hoc $BF_{10} = 0.16$ (with a default Cauchy prior of 0.707) indicated moderate evidence in favour of the null hypothesis; that there is no difference between groups in false alarm rates. A ROPE analysis with a 95% HDI (-0.16, 0.06) found that all of the 95% most credible effect sizes were practically equivalent to zero effect. Therefore, there is sufficient evidence to accept the null hypothesis and conclude that CBM-App training did not lead to higher false alarm rates than the debrief-only control intervention.

Memory Emotionality Ratings

Another aim of the present experiment was to investigate the effectiveness of positive CBM-App training as a computerised therapy for reducing the emotional intensity of memories for a traumatic video. Ratings of memory emotionality were measured at three time points during the experiment: immediately after participants viewed the traumatic video (T1), immediately after participants completed either the CBM-App training intervention or the post-trauma debrief-only control intervention (T2), and one week later during session two (T3). Two key hypotheses were made; firstly that there would be greater decreases in emotionality ratings from T1 to T2 for the CBM-App training group than for the control group. Secondly, that there would also be greater decreases in emotionality ratings from T2 to T3 for the CBM-App training group than for the control group.

Figure 16 shows the changes in memory emotionality ratings across the three time points for participants who received the CBM-App training intervention and the debrief-only control intervention. Participants in the CBM-App training group rated their memory for the traumatic video more negatively at T1 and T2 compared to participants in the control group. However, at T3 participants in the CBM-App training group rated their memory emotionality less negatively than the control group. Overall, it appears both groups experienced decreases in memory emotionality over time, with the largest decrease occurring between T2 and T3 (after a 5-day delay) for the CBM-App training group.

A mixed-model analysis of variance (ANOVA) comparing emotionality ratings over time for the CBM-App training group and control group revealed a significant main effect of time on emotionality ratings, $F(2,416) = 88.40$, $p < .001$, $\eta^2 = 0.15$. This indicates memory emotionality ratings decreased from T1 to T3 to become less negative across both experimental groups. Although the effect of experimental intervention on emotionality ratings was non-significant, $F(1,208) = 0.63$, $p = .43$, $\eta^2 = 0.01$, there was a significant time × intervention interaction effect, $F(2,416) = 3.06$, $p = .048$, $\eta^2 = 0.002$. The interaction suggests the effect of time on participants' emotionality ratings was stronger for participants who received CBM-App training than for participants who received the debrief-only control intervention.

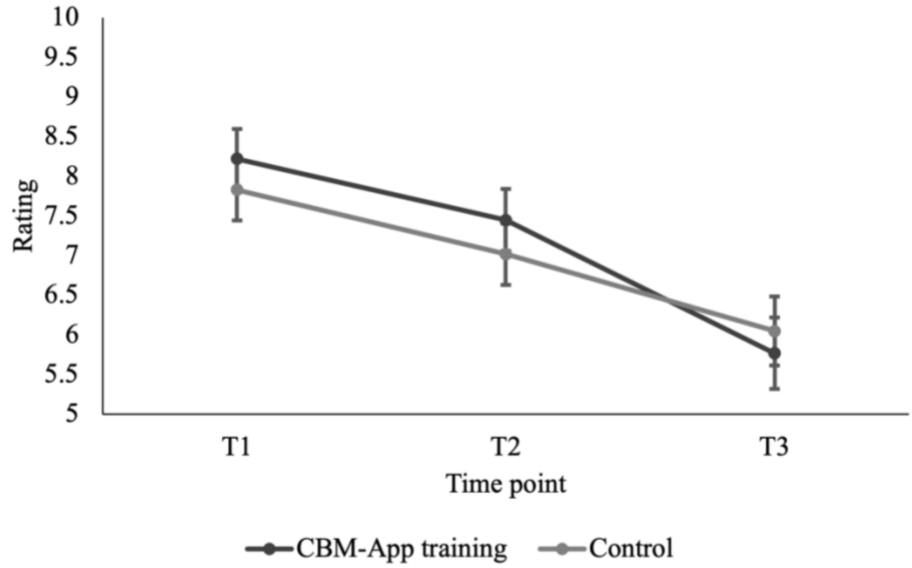
Follow-up Mann-Whitney *U*-tests compared group differences in change scores for emotionality ratings between each time point (T1 – T2, T2 – T3, and T1 – T3). There was no significant difference between the CBM-App training group and the control group in the amount of change in emotionality ratings from T1 to T2, $U = 5237$, $p = .53$, $d = 0.03$, 95% CI [-0.25, 0.30]. Post-hoc BFs with a default Cauchy prior (0, 0.707) resulted in $BF_{10} = 0.15$. The BF indicates moderate evidence in favour of the null hypothesis that there was no difference between groups in the amount of change in memory emotionality from pre-intervention (T1) to immediately post-intervention (T2). Consistent with this, a

ROPE analysis with a 95% HDI (-0.12, 0.12) found that all of the 95% most credible effect sizes were practically equivalent to zero effect. Therefore, there is strong evidence to accept the null hypothesis.

As depicted in Figure 16, participants who received CBM-App training experienced a greater decrease in memory emotionality from T2 to T3 than participants who received the debrief-only intervention, $U = 4636$, $p = .045$, $d = 0.28$, 95% CI [0.005, 0.55]. A $BF_{10} = 1.01$ (with a default Cauchy prior of 0.707 on effect size) indicated the data was only 1.01 times more likely under the alternative hypothesis than under the null hypothesis. This can be interpreted as equivalent evidence for the null and alternative hypotheses. A ROPE analysis with a 95% HDI (-0.46, 0.00) found that some of the 95% most credible effect sizes were practically equivalent to zero effect. This means there is insufficient evidence to definitely accept or reject the alternative hypothesis that the CBM-App training intervention caused a greater change in memory emotionality ratings from T2 to T3.

Figure 85.

Memory emotionality ratings at T1, T2, and T3 for the CBM-App training group and debrief-only control group



Note. Error bars demonstrate standard error of the mean (SEM) for memory emotionality ratings. The emotionality scale has been restricted to better illustrate the pattern of effects.

Lastly, the overall change in memory emotionality ratings from pre-intervention (T1) to one week post-intervention (T3) was greater for the CBM-App training group than for the control group, $U = 4604$, $p = .04$, $d = 0.25$, 95% CI [0.02, 0.52]. However, a $BF_{10} = 0.70$ suggests very weak evidence in favour of the null hypothesis: that there was no difference between groups in the overall change in memory emotionality ratings. The uncertainty of this effect was further supported by a ROPE analysis with a 95% HDI (-0.44, 0.00), showing that some of the 95% most credible effect sizes were practically equivalent to zero effect. Therefore, there is insufficient evidence to accept or reject the null hypothesis for the between-group comparison of overall change in emotionality.

Exploratory analyses were conducted to investigate the extent of the changes in emotionality ratings over time *within* each group. These results are described in the Exploratory Analyses section of this chapter.

Memory Vividness Ratings

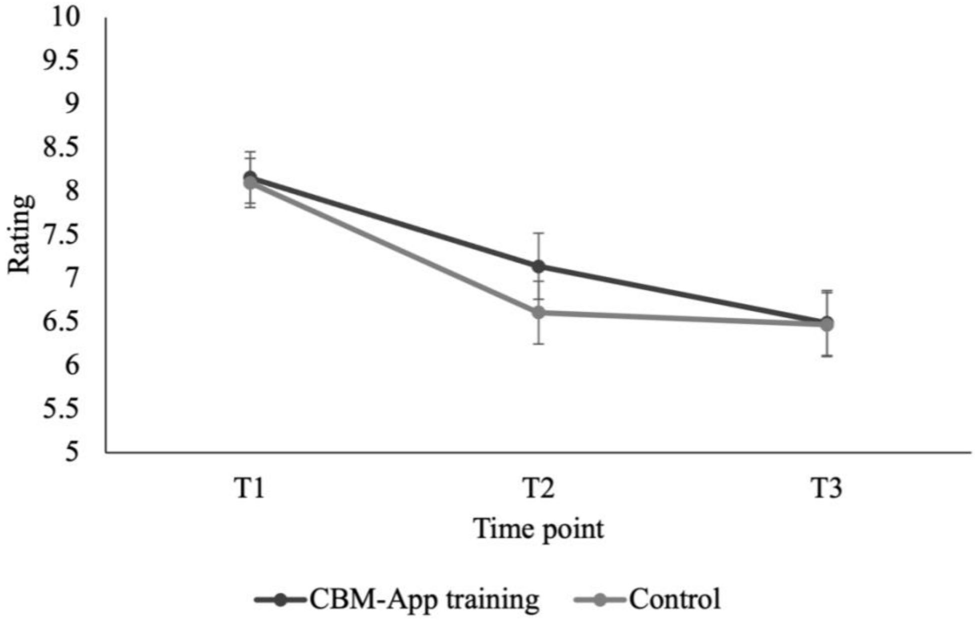
In addition to memory emotionality, the present experiment aimed to investigate the effect of positive CBM-App training on participants' vividness ratings of their memory for a traumatic video. Ratings of memory vividness were measured at three time points during the experiment: immediately after participants viewed the traumatic video (T1), immediately after participants completed either the CBM-App training intervention or the post-trauma debrief-only control intervention (T2), and one week later during session two (T3). Two focal predictions were made; firstly that there would be greater decreases in memory vividness ratings from T1 to T2 for the CBM-App training group than for the control group. Secondly, that there would also be greater decreases in memory vividness ratings from T2 to T3 for the CBM-App training group than for the control group.

Figure 17 presents the changes in participants' memory vividness ratings across the three time points for the CBM-App training group and the debrief-only control group. The graph shows that pre-intervention (T1) vividness ratings were very similar for the CBM-App training group and the control group. Although both groups showed a decrease in memory vividness ratings immediately post-intervention (T2), this decrease appeared to be descriptively larger for the control group than for the CBM-App training group. Participants in the CBM-App training group showed a further decrease in vividness ratings after one week (T3), while vividness ratings remained relatively consistent for the control group.

Figure 17 indicates both groups experienced similar overall decreases in memory vividness, with this decrease occurring more rapidly for the control group and a more gradual decrease over time for the CBM-App training group.

Figure 93.

Memory vividness ratings at T1, T2, and T3 for the CBM-App training group and debrief-only control group



Note. Error bars demonstrate standard error of the mean (SEM) for memory vividness ratings. The vividness scale has been restricted to better illustrate the pattern of effects.

A mixed-model ANOVA comparing memory vividness ratings over time for the CBM-App training group and control group established a main effect of time on vividness ratings, $F(2,416) = 83.82$, $p < .001$, $\eta^2 = 0.13$. This suggests participants in both groups experienced a reduction in memory vividness over time. However, the main effect of condition on memory vividness ratings was non-significant, $F(1,208) = 1.04$, $p = .31$, $\eta^2 = 0.003$. There was also no significant time × condition interaction for vividness ratings, $F(2,416) = 2.22$, *p*

=.11, η^2 = 0.003. This indicates the CBM-App training intervention did not lead to greater reductions in memory vividness over time than the debrief-only control intervention.

Follow-up Mann-Whitney *U*-tests compared group differences in change scores for vividness ratings between each timepoint (T1 – T2, T2 – T3, and T1 – T3). Unexpectedly, participants in the control group experienced a greater reduction in memory vividness from pre-intervention (T1) to immediately post-intervention (T2) than participants in the CBM-App training group, $U = 4441$, $p = .01$, Cohen's $d = 0.32$, 95% CI [0.04, 0.59]. Post-hoc BFs with a default Cauchy prior of 0.707 on effect size resulted in $BF_{10} = 1.81$, indicating weak evidence in favour of the alternative hypothesis: that there is a difference between groups in the change in memory vividness from T1 to T2. As indicated above, this difference was not in the expected direction, with the control group demonstrating a greater reduction in vividness than the CBM-App training group. However, this effect was not supported by a ROPE analysis with a 95% HDI (-0.00, 0.51), which found that some of the 95% most credible effect sizes were practically equivalent to zero effect. Therefore, there is insufficient evidence to accept or reject the alternative hypothesis that the control group experienced a greater decrease in memory vividness from pre-intervention to immediately post-intervention.

In contrast, between T2 and T3 the decrease in memory vividness was greater for the CBM-App training group than for the control group, $U = 4622$, $p = .04$, $d = 0.23$, 95% CI [0.05, 0.50]. However, this effect was also not supported by the post-hoc $BF_{10} = 0.52$ (with a default Cauchy prior of 0.707), which indicates weak evidence in favour of the null hypothesis; that is, there is no difference between groups in the change in memory vividness from immediately post-intervention (T2) to one week later (T3). Consistent with this, a ROPE analysis with a 95% HDI (-0.39, 0.00) found that some of the 95% most credible effect sizes were practically equivalent to zero effect, therefore there is insufficient evidence to accept or reject the null hypothesis.

Regarding overall change in memory vividness from pre-intervention (T1) to one-week post-intervention (T3), there was no difference between the CBM-App training group and the control group, $U = 5312$, $p = 0.66$, $d = 0.02$, 95% CI [-0.29, 0.25]. A post-hoc $BF_{10} = 0.15$ (with a default Cauchy prior of 0.707) provided moderate evidence in favour of the null hypothesis, indicating no effect of the CBM-App training intervention on overall change in memory vividness. This was further supported by a ROPE analysis with a 95% HDI (-0.13, 0.11), which established all of the 95% most credible effect sizes were practically equivalent to zero effect. These findings provide sufficient evidence to accept the null hypothesis for the overall change in memory vividness from pre-intervention to one week post-intervention.

Exploratory analyses were conducted to investigate the extent of the changes in vividness ratings over time *within* each group. These results are described in the following section describing the Exploratory Analyses for the present research.

Exploratory Analyses

The analyses in this section were not outlined in the present experiment's pre-registration and are therefore exploratory rather than confirmatory.

To explore whether the lack of between-group differences in discriminability were due to floor effects on the memory test, area under the ROC curve for each group was compared against chance-level performance (AUC = 0.50). Results from a Mann-Whitney *U*-test showed participants in the control group performed significantly better than chance, $U = 1426$, $p < .001$, Cohen's $d = 1.53$, 95% CI [1.17, 1.87]. Post-hoc BFs with a default Cauchy prior of 0.707 on effect size established $BF_{10} = 3.35E^{+10}$. This indicates very strong evidence in favour of the alternative hypothesis; that participants in the control group performed better than chance-level on the recognition test. The effect was further supported by a ROPE analysis with a 95% HDI (-1.81, -1.18), which found none of the 95% most credible effect sizes were practically equivalent to zero effect. Therefore, there is substantial evidence to accept the alternative hypothesis.

Similarly, participants in the CBM-App training group also performed significantly better than chance on the recognition test, $U = 1694$, $p < .001$, $d = 1.43$, 95% CI [1.09, 1.76]. A post-hoc $BF_{10} = 1.62E^{+17}$ (with a default Cauchy prior of 0.707) provided strong evidence in favour of the alternative hypothesis. This was supported by a ROPE analysis with a 95% HDI (-1.70, -1.07), which established none of the 95% most credible effect sizes were practically equivalent to zero effect. In sum, results from these exploratory analyses suggest participants in both the CBM-App training group and the control group performed better than chance-level on the recognition test.

Confirmatory analyses also established the CBM-App training intervention was no more effective in reducing the emotionality or vividness of participants' memory for a traumatic video than the debrief-only control intervention. However, ANOVAs comparing between-group differences in emotionality and vividness provided some evidence that both groups experienced reductions in memory emotionality and vividness over time. Exploratory analyses were carried out to further examine these changes in memory emotionality and vividness over time *within* each group.

Table 4 displays the results of post-hoc comparisons for the time × intervention interaction for memory emotionality ratings. A Bonferroni correction was used to account for multiple comparisons. Decreases in memory emotionality were significant for the CBM-App training group and control group both immediately following the intervention (T1 – T2) and one week later (T2 – T3). In line with this, both groups experienced a significant overall reduction in emotionality from pre-intervention (T1) to one-week post-intervention (T3).

The same post-hoc analyses were carried out for the time × intervention interaction for memory vividness ratings. Results from these analyses are presented in Table 5, with a Bonferroni correction to account for multiple comparisons.

Table 4

Post-Hoc Comparisons of Within-Group Changes in Memory Emotionality Over Time for the CBM-App Training Group and the Control Group

Condition	Time comparison	Mean difference	*t*	$p_{bonferroni}$
CBM-App training				
	T1 – T2	0.77	3.46	.01
	T2 – T3	1.68	7.53	< .001
	T1 – T3	2.45	10.98	< .001
Control				
	T1 – T2	0.81	3.50	.01
	T2 – T3	0.97	4.19	< .001
	T1 – T3	1.78	7.69	< .001

Note. Degrees of freedom (df) = 416 for all comparisons.

As Figure 17 (presented earlier) indicated, the CBM-App training group and control group experienced very similar overall decreases in memory vividness, but demonstrated different patterns over time. Participants in the CBM-App training group experienced significant reductions in memory vividness both immediately post-intervention (T1 – T2) and one-week post-intervention (T2 – T3). However, participants in the control group only experienced significant reductions in memory vividness immediately post-intervention, with no significant changes in vividness ratings one week post-intervention.

Table 5

Post-Hoc Comparisons of Within-Group Changes in Memory Vividness for the CBM-App Training Group and Control Group

Condition	Time comparison	Mean difference	t	$p_{bonferroni}$
CBM-App training				
	T1 – T2	1.02	5.16	< .001
	T2 – T3	0.65	3.53	.01
	T1 – T3	1.67	9.04	< .001
Control				
	T1 – T2	1.49	7.74	<.001
	T2 – T3	0.15	0.77	1.00
	T1 – T3	1.63	8.53	< .001

Note. Degrees of freedom (df) = 416 for all comparisons.

Brief Summary of Findings

Confirmatory data analyses revealed that positive CBM-App training successfully instilled a positive cognitive bias in participants' appraisals of their ability to cope after a traumatic event. Despite having a positive impact on participants' post-trauma appraisals, CBM-App training failed to reduce the emotional intensity and vividness of memory for a traumatic video, although Bayesian analyses indicated some ambiguity about this conclusion. CBM-App training also had no effect on recognition discrimination accuracy or acceptance of post-event misinformation. Therefore, the key hypotheses for the present experiment were either unsupported or unable to be definitively disconfirmed. The implications of these findings are discussed in Chapter 9.

CHAPTER 9

General Discussion

As noted in the introduction of this thesis, people are increasingly exposed to distressing material online. Research indicates viewing such material can lead to the development of posttraumatic stress symptoms, as well as other mental health difficulties (Pfefferbaum et al., 2019, 2021). Although psychotherapy is an effective means for addressing these difficulties, it is a limited resource that may not be accessible to all those who are psychologically impacted by distressing material they come across. Moreover, recent COVID-19 pandemic health guidance has further restricted access to face-to-face mental health services for many people around the world (World Health Organisation, 2022). As a result, people are turning to self-help, telehealth, and online interventions that require minimal therapist input. One aim of Experiment Two was to investigate the effectiveness of a computerised intervention (CBM-App training) for reducing the vividness and emotional intensity of people's memory for a viral-style video of a traumatic event.

However, recent research has raised concerns about the effects of some therapeutic techniques on susceptibility to misinformation (e.g., Houben et al., 2018). Given that CBM-App training involves shifting one's interpretation of a traumatic event, it is possible this could lead to memory distortion. Memory distortion may, in turn, increase vulnerability to misinformation effects when exposed to misleading information about the traumatic event. Therefore, in Experiment Two I investigated whether the CBM-App training intervention increased people's susceptibility to post-event misinformation compared to a trauma debrief control. Importantly, I employed novel misinformation materials and procedures (developed in Experiment One) and sensitive estimates of misinformation effects (using SDT and ROC analyses) to address these aims.

Summary of Key Findings

Results from Experiment Two suggested the CBM-App training intervention was successful in instilling a positive cognitive bias. Specifically, participants who completed the CBM-App intervention had significantly stronger positive cognitive appraisals about their ability to cope in the aftermath of a traumatic event than participants in the control group. However, contrary to hypotheses, the effect of the CBM-App intervention did not translate to greater reductions over time in trauma memory vividness or emotional intensity compared to the post-trauma debrief. Although the *between*-groups comparisons for change in memory vividness and emotionality were non-significant, exploratory analyses demonstrated significant reductions *within* groups. This suggests the passage of time, alone, may reduce the vividness and negative emotionality associated with a traumatic memory.

Also counter to hypotheses, results from SDT and ROC analyses demonstrated no effects of the CBM-App intervention on participants ' recognition discrimination accuracy compared to the post-trauma debrief only. There was also no evidence to suggest participants in the CBM-App training group endorsed more false alarms or had fewer hits on the recognition test than participants in the control group. Although both experimental groups demonstrated bias towards responding "true" across all items on the recognition test, response bias was no stronger for participants who received the CBM-App intervention than for those who received the post-trauma debrief only. Importantly, these results indicate CBM-App training does not distort memory for a traumatic video or increase people's susceptibility to post-event misinformation.

Taken together, findings from Experiment Two demonstrate that while CBM-App training had a strong effect on instilling positive posttraumatic cognitive appraisals, this shift in cognitive appraisal had no impact on memory for a traumatic video in terms of memory emotionality, vividness, discrimination accuracy, or susceptibility to misinformation. These results raise important questions regarding the effectiveness of computerised trauma interventions and the scepticism of some researchers about the potential drawbacks of certain therapeutic techniques. Nuances and implications of these results for CBM-App training, understandings of trauma, and misinformation research are discussed in the following sections.

Implications for CBM-App Training

Trauma Memory Vividness and Emotionality

The present research builds on a body of work demonstrating the effectiveness of computerised CBM-App training for modifying dysfunctional appraisals. Experiment Two was successful in conceptually replicating previous findings of the effect of CBM-App training on appraisals (e.g., Woud et al., 2012, 2021; Woud, Zlomuzica, et al., 2018). Similar to previous CBM-App studies, positive CBM-App training led to more positive trauma-related appraisals.

However, distinct from previous studies, the present experiment compared positive CBM-App training to a neutral control condition (the post-trauma debrief), rather than to *negative* CBM-App training. Comparing positive CBM-App training to a neutral control means more precise conclusions can be drawn about the effect of CBM-App training on appraisal style. In other words, between-group differences in appraisal style can be more clearly attributed to the effects of *positive* CBM-App training, rather than to the negative effects of *negative* CBM-App training.

The finding that CBM-App training induced a positive appraisal style has important implications for the treatment of post-traumatic stress symptoms. The cognitive model of PTSD suggests dysfunctional appraisals about the trauma and its sequelae, along with poor elaboration and contextualisation of the event in autobiographical memory, are the underlying factors in PTSD symptoms and contribute to symptom severity and maintenance (Ehlers & Clark, 2000). Therefore, targeting dysfunctional appraisals through CBM-App training should, theoretically, lead to decreases in post-trauma symptoms.

Although decreases in post-trauma symptoms following CBM-App training have been demonstrated in some previous studies (Woud et al., 2012, 2021; Woud, Zlomuzica, et al., 2018), there was no effect of CBM-App training on trauma memory vividness or emotional intensity in the present experiment. This null effect is similar to the findings of Würtz et al. (2022), who failed to find any effect of CBM-App training on analogue trauma symptoms (measured by intrusion frequency/distress and Impact of Events Scale [IES-R] scores). While the outcome measures used by Würtz et al. (2022) differed from the current study, the general pattern of findings indicate some inconsistency in the ability of CBM-App training to reduce post-trauma symptoms. This may be due to different processes occurring for imaginal vicarious trauma (i.e., watching a video of a traumatic event occurring to someone else) and in-vivo personal exposure to trauma. CBM-App training may be more effective for the latter type of trauma due to the greater relevance of appraisals for events that are personally experienced.

There are a number of possible explanations for the failure to find any impact of appraisal modification on trauma memory vividness and emotional intensity. First, it might be that no direct causal relationship exists between trauma appraisals and trauma symptoms. However, this seems unlikely given the wealth of research highlighting the interconnectedness of negative trauma appraisals and PTSD symptoms (e.g., Barlow et al., 2017; DePrince et al., 2011; Martin et al., 2013; McLean et al., 2015). A more likely explanation may be that the CBM-App training in this study was not effective enough to produce changes in trauma symptoms in this study. Although the effect size for the effect of CBM-App training on positive cognitive bias in Experiment Two was large (d = 0.74), the effect was smaller than those found in previous studies (d ranging 1.22 to 1.96) that have demonstrated associated reductions in trauma symptoms (Woud et al., 2012, 2021; Woud, Zlomuzica, et al., 2018). Therefore, it is possible that a larger effect of CBM-App training is required for changes to occur in trauma symptoms.

A second related possibility is that no effect was observed because positive changes in trauma appraisals did not generalise to memory emotionality or vividness. CBM-App training relies on the ‘far ’transfer of learning from the bias modification training task to later situations through repeated practice on a task designed to induce change in appraisal bias (Hertel & Mathews, 2011). Although transfer effects were observed on the encoding recognition task, it may be that participants ’learning from the positive appraisal training could not be transferred as far as to their perception of the vividness and emotional intensity of the trauma memory. Transfer issues are common in cognitive bias modification research, particularly for transfer to emotional challenges (Salemink et al., 2007; Teachman & Addison, 2008; Würtz et al., 2022). However, other CBM-App studies have demonstrated successful transfer of positive appraisal bias to post-trauma symptoms when using alternative outcome measures, including intrusion diaries, Posttraumatic Cognitions Inventory (PTCI), PTSD Checklist for DSM-5 (PCL-5), and Impact of Events Scale-Revised (IES-R; Woud et al., 2012, 2021; Woud, Zlomuzica, et al., 2018). It is therefore possible CBM-App training targets trauma symptoms that were simply not measured in this study, such as hypervigilance, hyperarousal, and avoidance.

So why were transfer effects not observed for memory emotionality and vividness in this study? One reason may be that ratings of memory vividness and emotionality are less closely related to the CBM-App training task than other PTSD measures. Transfer of cognitive bias modification typically requires a close match between cognitive processes employed during both the training phase and transfer phase, known as transfer-appropriate processing (Blaxton, 1989; MacLeod & Mathews, 2012). The training task was based on items from the PTCI, for this reason it would be expected that the transfer of learning from the training task should occur more easily for the PTCI than for measures less similar to the training task. Likewise, the PCL-5 and IES-R are measures that closely map on to items in the CBM-App training task and likely invoke the same cognitive processes in training and transfer. On the other hand, continuous ratings of trauma memory vividness and emotionality may invoke different cognitive processes than the training task, and this may impede transfer of positive appraisal bias to this particular outcome measure.

Lastly, the failure of CBM-App training to reduce memory vividness and emotionality may have been due to differences in the CBM protocol used in this study. These differences included a shortened version of the CBM-App training, exclusion of response accuracy feedback, and at-home delivery of the training. A meta-analysis of general cognitive bias modification protocols found that CBM is most effective when delivered in the laboratory (Jones & Sharpe, 2017). The authors suggested this may be due to greater attention and adherence to the training task when completed in the laboratory than when completed at home, where distractions are likely present (Jones & Sharpe, 2017). This brings into question the appropriateness of CBM-App training as a home-based self-directed therapeutic intervention. Furthermore, the meta-analysis revealed greater CBM efficacy for studies that included response accuracy feedback during training. These differences, as well as the shorter training phase, may explain the smaller effect of training on cognitive bias and the possible associated null effects on memory vividness and emotionality observed in this study.

Although the CBM-App protocol used in this study was unsuccessful in stimulating reductions in memory vividness and emotionality beyond that of a trauma debrief, overall changes in memory vividness and emotionality were observed for both groups, with some notable patterns. Regarding changes in memory vividness, patterns observed in Experiment Two suggested CBM-App training may have less immediate impact but may lead to continued reductions in memory vividness over time. This partly fits with previous findings by Vermeulen et al. (2019), which showed delayed effects of CBM-App training on posttraumatic cognitions after one week. The authors suggested the effects of induced cognitive biases may only manifest themselves in emotional, behavioural, and cognitive changes after a delay because participants need time to consolidate and practice bias modification in everyday life (Vermeulen et al., 2019). However, it is difficult to distinguish in the current study whether this pattern of effects can be attributed to the delayed effects of CBM-App training or simply to the passage of time leading to reduced memory vividness.

On the other hand, patterns for emotionality indicated the passage of time, alone, may reduce negative emotionality associated with a traumatic memory. Experiment Two results established significant reductions in memory emotionality across the three time points, regardless of whether participants received CBM-App training. Verduyn and colleagues (2012, 2015) identified several factors that influence emotion intensity and duration following an event. They suggest positive and negative emotions are most intense when the eliciting event is perceived as highly important to the individual, or, for negative emotions, when the eliciting event is perceived as uncontrollable (Verduyn et al., 2012). In the context of the present study, it is possible negative emotionality decreased over time for both groups because the importance or relevance of the trauma film to participants decreased over time. Unlike a first-hand experience of real-life trauma, the trauma film carried no significant consequences for participants, thereby providing less reason for intense negative emotions to endure.

Other factors that influence emotion duration include duration of the eliciting event and individual differences in emotion regulation or coping strategies (Verduyn et al., 2015). Decreases in negative emotionality over time for both groups in the present study may have been due to the short length of the trauma film, causing the emotional effects of the trauma film to wear off faster. This idea fits with general understandings of trauma, whereby repeated or prolonged exposure to trauma increases the likelihood of more severe or pervasive PTSD symptoms (Goral et al., 2017; Hyland et al., 2017). In addition, emotionality decreases observed for both groups could be attributed to the effects of the post-trauma debrief. It is possible the debrief equipped participants with the emotion regulation or coping skills necessary to process the trauma video effectively, leading to subsequent decreases in negative emotionality. The possible effects of the post-trauma debrief are discussed later in this chapter.

Overall, while CBM-App training is effective for inducing positive appraisals, it does not appear to lead to reductions in the vividness or emotional intensity of memory for a traumatic video. A number of possible explanations for the lack of effects observed in this study were explored, including smaller effect size for the appraisal manipulation, bias transfer issues, and differences in the CBM-App protocol used. CBM-App research is still in its infancy and the current study highlights some potential limitations of this intervention. However, further research is required to determine the efficacy of CBM-App training in a therapeutic context, and to identify the mechanisms responsible for reductions in PTSD symptoms. Next, the impact of CBM-App training on susceptibility to misinformation is discussed.

Susceptibility to Misinformation

In recent years, questions have been raised about the potential for some therapeutic techniques to distort memory and increase susceptibility to post-event misinformation (e.g., Houben et al., 2018; Wilson et al., 2015). To my knowledge, this is the first study to specifically examine the misinformation potential of CBM-App training. Experiment Two results suggest CBM-App training (and subsequent changes in appraisal bias) may not increase people's susceptibility to misinformation. This finding is consistent with recent research arguing against the misinformation potential of therapeutic techniques used to treat trauma symptoms (Calvillo & Emami, 2019; van Schie & Leer, 2019). However, these studies have largely focused on the misinformation potential of eye movements in EMDR therapy, with significantly less known about the impact of CBM. This section discusses possible explanations for the lack of effect of CBM-App training on susceptibility to misinformation, including the temporal order of the experimental procedure and the role of memory vividness as a mechanism for misinformation effects.

Despite an apparent dearth of research exploring the misinformation potential of CBM-App training, one study has explored the impact of CBM for trauma interpretations (CBM-I) on positive and negative false memory ‘intrusions '(Tran et al., 2011). In Tran et al.'s (2011) study, participants first completed either positive or negative CBM-I training. They were then presented with 20 novel ambiguous scenarios, each containing a word fragment which participants had to resolve. After completing a cognitive bias manipulation check, participants were asked to recall details from the 20 novel ambiguous scenarios presented earlier. Results showed participants who completed positive CBM-I training reported more positively-valenced memory intrusions, while participants who completed negative CBM-I training reported more negatively-valenced memory intrusions (Tran et al., 2011). The authors concluded interpretation biases arising from CBM can lead to memory distortions (Tran et al., 2011).

Results from the current study contradict the findings of this research, which demonstrated interpretation biases induced through CBM-I increased false memories during recall (Tran et al., 2011). However, in contrast to the current study, participants completed CBM-I *before* being exposed to the to-be-remembered events. The authors argued that it is how events are *initially* interpreted that affects how they are subsequently recalled (Tran et al., 2011). Therefore, it is possible that CBM-App training did not affect misinformation endorsement in the present study because participants had already formed their appraisal of the event before they completed CBM-App training. As a result, their memory for the event during the recognition test was based on this initial appraisal and may have been unaffected by appraisal modification training. Future research can bring more clarity to the effects of CBM-App training on susceptibility to misinformation by investigating whether appraisal bias modification prior to trauma exposure may have a larger impact on event appraisals and subsequent misinformation endorsement.

Another potential explanation for the lack of increased susceptibility to misinformation observed for the CBM-App training group in this study is due to the lack of impact on memory vividness. Houben et al. (2018) hypothesised a person is more susceptible to misinformation when their memory for the original event is less vivid. This also fits with the tenets of multiple false memory theories, including fuzzy-trace theory, discrepancy detection, and source monitoring (Brainerd & Reyna, 2002; Johnson et al., 1993a; Tousignant et al., 1986). Since both groups in the current study demonstrated similar decreases in memory vividness over time, it is difficult to distinguish whether there was a link between memory vividness and susceptibility to misinformation in this study. It could be speculated that the observed reductions in vividness led to increased misinformation endorsement for both groups. However, other studies in this field have found no such relationship exists between memory vividness ratings and misinformation endorsement (Calvillo & Emami, 2019; van Schie & Leer, 2019). This supports the idea that therapeutic techniques which aim to reduce trauma memory vividness do not necessarily have the unintended drawback of distorting memory and increasing susceptibility to misinformation.

The lack of impact of CBM-App training on misinformation endorsement in this study holds importance for real-world applications of support for trauma symptoms at a community level. Experiment Two results provide preliminary evidence that it may be possible to work with cognitive symptoms of trauma without significantly distorting memory. This is important because, from a therapeutic and forensic perspective, it suggests people may be able to access support for symptom management in the aftermath of a traumatic event without concerns about the impact on the veracity of their memory. Moreover, in the context of community-level vicarious and online trauma, this study has shown CBM-App training can be administered on mass as a computerised intervention to modify maladaptive trauma-related appraisals. Although it is not yet clear what the impact of this is on other trauma symptoms, this study in conjunction with other CBM-App research provides some preliminary evidence of CBM-App training as a wide scale, self-directed trauma intervention in the context of vicarious and online exposure to trauma.

Implications for Trauma Research

The present research provides a unique perspective within the trauma literature in that it primarily focuses on *internet media* exposure to trauma. As highlighted throughout this thesis, media exposure to trauma has become an issue of increasing concern in recent years following the release of uncensored viral-style video footage of traumatic events. This has meant video footage of traumatic material has the potential to reach and impact large numbers of people. However, little is understood about the impact of this type of exposure to traumatic content on people's mental wellbeing or how potential resulting symptoms can be managed. Therefore, one of the aims of the present research was to explore the impact of a seemingly realistic viral-style video of a school shooting on the vividness and emotional intensity of event memory.

There is ongoing debate about whether media exposure to a traumatic event can produce symptoms of PTSD (Hopwood & Schutte, 2017). The results for memory emotionality and vividness ratings in both Experiment One and Two provide some indication of the impact of media exposure to traumatic events. The results showed that after viewing a seemingly realistic traumatic video, participants experienced highly vivid and negatively-valenced memories of the event. In addition, multiple participants commented they found the trauma film “disturbing”, “shocking”, and “difficult to watch” despite being made aware prior to watching it that the film was fictional. Although the current research did not specifically measure mental health symptomology, the results indicate viewing the trauma film produced symptoms similar to subclinical levels of posttraumatic stress, including intrusive mental imagery and emotional distress. This fits with the growing body of research demonstrating the negative effects of media exposure to traumatic content on mental health outcomes (Feinstein et al., 2014; Pfefferbaum et al., 2021; Thompson et al., 2019). However, it should be noted that the trauma film paradigm and the sub-clinical effects it produces do not pose actual harm to participants (Stirling et al., 2021).

These findings have important implications for how trauma is defined. The current DSM-5 definition for a traumatic event excludes media exposure, unless the exposure occurs in the line of work (APA, 2013). However, the present research further contributes to the argument that people can experience high levels of distress in response to traumatic media footage, even when exposure occurs outside of the workplace. As evidence of the impact of media exposure to trauma continues to grow, current definitions of trauma may need to be updated to better reflect the experiences of those affected by traumatic media content (Shin & Sommers, 2021). Importantly, broader acknowledgement of the impact of media exposure to trauma may help to validate people's experiences and may have implications for public health, including access to support.

Early Trauma Interventions

Another point of tension in the trauma literature is the effectiveness of early trauma interventions and post-trauma debriefing. In Experiment Two, a post-trauma debrief video was used as a control comparison for the CBM-App training intervention. Based on Whitworth's (2016) recommendations, the debrief included information about common reactions to trauma, simple coping strategies, and contact details for local mental health hotlines. While investigating the effectiveness of the post-trauma debrief was not a direct aim of the experiment, results indicated that participants who received only the post-trauma debrief experienced similar (and significant) reductions in trauma memory vividness and emotional intensity as those who also received CBM-App training.

These findings contradict initial research in the field of psychological debriefing, which suggested early interventions after trauma are ineffective and, in some cases, can worsen symptoms of PTSD (Bisson et al., 1997; Hobbs et al., 1996). Furthermore, a Cochrane Review concluded that trauma debriefs are not a useful intervention for the prevention of PTSD and called for the cessation of routine psychological debriefing in the aftermath of trauma (Rose et al., 2002). However, psychological debriefing in these early studies was largely focused on emotional ventilation and processing by encouraging detailed recollection of the traumatic event. Some researchers have suggested psychological debriefing involving premature re-exposure and emotional processing of the traumatic event can elevate psychological distress and can potentially reinforce maladaptive trauma appraisals and interfere with natural recovery (Bisson et al., 1997; Dyregrov & Regel, 2012; Rose et al., 2002).

Alternatively, the post-trauma debrief used in Experiment Two employed a psychoeducation approach to early intervention. Research investigating the efficacy of psychoeducation as a standalone early trauma intervention is relatively scarce. However, the extant findings suggest psychoeducation is effective when it includes a combination of information about common symptoms, emphasises human resilience, and is delivered with information for seeking additional support (Wessely et al., 2008; Whitworth, 2016). Psychoeducation as an early intervention is thought to help normalise and validate people's experiences, thereby reducing distress about symptoms (Wessely et al., 2008). It is also thought to improve a sense of self-efficacy and facilitate further help-seeking where required (Wessely et al., 2008).

Experiment Two findings provide tentative support for the clinical utility of psychoeducation-focused approach to post-trauma debriefing as an early intervention. More importantly, the findings provide a foundation for further exploration of early trauma interventions, particularly in the context of mass trauma exposure. Developing early intervention is essential because it can assuage common trauma reactions and prevent later development of PTSD (Kearns et al., 2012). In the context of mass trauma and media exposure to trauma, video debriefing (such as that used in the current research) affords the opportunity to reach a wide audience and potentially minimise rates of PTSD in a cost-effective manner. Trauma debriefing also has implications for people who routinely work with traumatic material (e.g., journalists, social media content moderators, emergency service workers, and child exploitation investigators), where debrief-type techniques are often used to help employees with symptom management related to their work (Devilly et al., 2006; Wortley et al., 2014). Furthermore, in legally-sensitive situations, psychological intervention initially focuses on symptom management until the legal process is completed. Therefore, understanding the effective components of early trauma intervention has implications for a broad scope of individuals.

Veracity of Trauma Memory

Controversy also exists regarding the veracity of trauma memory. One of the reasons this thesis specifically examined misinformation effects in the context of media exposure to trauma was due to the claim that trauma memory is more fragmented, less reliable, and therefore more susceptible to distortion than non-trauma memory (e.g., Brewin, 2016; Ehlers et al., 2004; Foa et al., 1995). Results from Experiment Two demonstrated, on average across both groups, participants achieved approximately 70% accuracy on the recognition test. This indicates participants 'memory for the trauma video was relatively accurate after a five-day delay. Although no comparison was made between trauma and non-trauma memory, to the extent that the sub-clinical trauma film paradigm mimics genuine trauma, the findings provide some evidence that trauma memory may not be as unreliable as some researchers claim.

This finding is consistent with a growing body of literature suggesting traumatic memories are no less coherent or fragmented than non-traumatic memories (e.g., Bedard-Gilligan et al., 2017; Rubin et al., 2016; Taylor et al., 2021). While objective measures of memory incoherence do not appear to demonstrate any differences for traumatic and non-traumatic memories, the subjective *feeling* of incoherence may have important consequences for trauma symptomology and memory veracity. A study by Segovia et al. (2016) demonstrated that people's subjective feelings of memory disorganisation were associated with greater trauma intrusion symptoms and more memory distortion.

However, Taylor et al. (2020) noted no difference in subjective feelings of memory incoherence for trauma and non-traumatic memory. Instead, the authors suggested it is the conditions under which the memory is retrieved that may impact subjective memory coherence (regardless of whether the memory is traumatic), with probing questions leading to greater judgements of incoherence (Taylor et al., 2020). Taken together, these findings indicate that the nature of questioning about a traumatic memory can impact a person's sense of memory coherence, and this, in turn, can increase trauma symptomology and memory distortion. Therefore, careful consideration of methods for exploring traumatic memories in therapeutic and forensic contexts is imperative to ensure optimal memory veracity and to minimise further distress.

Implications for Misinformation Research

A key aim of this thesis was to develop a novel experimental materials and procedures for investigating misinformation effects in the context of trauma therapy. These materials and procedures included a new (and ostensibly realistic) trauma film, longer retention interval between event encoding and misinformation exposure, and a recognition test requiring participants to make a categorical confidence judgement about whether an item was present in the trauma film. The purpose of this novel paradigm was to improve ecological validity, reduce the likelihood of floor effects, and increase the ability to detect subtle changes in memory strength due to misinformation. Results from the two experiments in this thesis demonstrate the utility of this novel paradigm for instilling misinformation effects and exploring their moderating factors.

As discussed earlier in this thesis, a criticism of previous studies investigating misinformation effects in the context of trauma (e.g., Calvillo & Emami, 2019; Houben et al., 2018; van Schie & Leer, 2019) is that null effects of the intervention on misinformation endorsement are due to floor effects for misinformation acceptance. In most of the previous studies, participants, on average, endorsed fewer than one misinformation item (Calvillo & Emami, 2019; van Schie & Leer, 2019). However, in both experiments of this thesis, on average participants endorsed approximately half of the misinformation items (at least four items). This comparison provides evidence that the misinformation paradigm employed in the present research is effective and appeared to instill a stronger misinformation effect than the original paradigm employed in previous studies. Since the null effects found in Experiment Two cannot be attributed to floor effects for the misinformation effect, the present research also gives additional weight to the conclusion that cognitive-based therapeutic interventions, such as CBM-App training, are unlikely to have the unintended consequence of increasing susceptibility to misinformation.

Another important consideration in this research was the ecological validity of materials and procedures. Rather than using a trauma film of a car accident (e.g., Houben et al., 2018) or burglary (e.g., Takarangi et al., 2006) used in traditional studies, the current research employed a video of a fictional school shooting. Participants also completed the entire procedure online on their own computers, rather than in a laboratory. The purpose of this was to capture experiences analogous to viewing viral footage of traumatic content online. In addition, the present research implemented a longer retention of approximately five days between viewing the traumatic video and introducing misinformation, where traditional studies commonly introduce misinformation only minutes later (e.g., Crozier & Strange, 2018; Houben et al., 2018; Monds et al., 2017). This was to mimic retention intervals in therapeutic and forensic contexts, where people may be exposed to misinformation and asked to recall a traumatic event some time after the event occurred.

Through implementing an ecologically valid and novel misinformation paradigm, this research demonstrated the generalisability of misinformation effects to an online, viral-style trauma context. Studies demonstrating the generalisability of misinformation effects across contexts are important because they strengthen the evidence for misinformation effects in the real-world. Moreover, the novel procedure used here provides a foundation for further research on misinformation effects in an online trauma context. This is important given the opportunity social media affords for vicarious exposure to viral traumatic content (Lowe & Galea, 2017; Redmond et al., 2019; Reeve, 2020;

Tynes et al., 2019) and for misinformation to circulate online (Allcott et al., 2019; Metzger et al., 2021; Wang et al., 2019).

The present research also has implications for the way misinformation effects are measured and quantified in experimental studies. Instead of adopting a 2AFC recognition test commonly used in misinformation studies, participants were asked to make a categorical judgement about the strength of their memory for a particular item that may or may not have been presented during the trauma video. This process enabled signal detection and ROC analyses to be conducted to investigate subtle differences in memory strength that may have occurred through exposure to misinformation and/or completing CBM-App training. Experiment One, in particular, reinforced the findings of previous research showing the value of ROC and SDT analyses in misinformation research (Horry et al., 2012, 2014; Sauer et al., 2008, 2012). Despite some ambiguity of the misinformation effect in the SDT analyses, ROC analyses showed a clear effect of misinformation on memory. Specifically, results showed that being exposed to misinformation led participants to be more certain that a misinformation item was present. However, this increased certainty did not necessarily lead to endorsement or acceptance of misinformation. This finding highlights the subtle effects misinformation exposure can have on memory, which may go undetected when using traditional methods of analysis.

This research further contributes to the literature in favour of moving away from binary assessments of memory in forensic contexts (see Sauer et al., 2012). The ability to detect subtle changes in memory strength is important in a forensic eyewitness context because weakened confidence in memory can thereby decrease a jury's perception of the credibility of the evidence (Cash & Lane, 2017; Slane & Dodson, 2022). So, even if the memory itself is unchanged, changes in confidence in that memory can have significant consequences. This may also have implications in a therapy context since doubt in memory (or the feeling of trauma memory incoherence) might increase trauma-related distress. Therefore, ROC analyses allow us to investigate the intricacies of memory and the real-world consequences of these subtle shifts.

ROC analyses used in this research also highlight the relative strength of memory. Across both experiments, participants' discrimination accuracy ranged from 68-77%. Moreover, CBM-App training (which is designed to target trauma memory intrusions) did not have any impact on memory accuracy. These findings suggest memory for traumatic events may not be quite as fallible, error-prone, or easily manipulated as some people believe (e.g., Loftus, 2005; Wake et al., 2020). Rather, the results from this study suggest participants' memory was mostly accurate but still susceptible to decay and contamination under certain circumstances. Therefore, while it is important to be aware of the limitations to memory, these limitations may not be sufficient to completely disregard the credibility of memory in therapy or forensic contexts.

Limitations and Future Directions

Due to the novel nature of materials and procedures used here, there are several limitations to the current research that may impede the generalisability of findings. One limitation of the present research is that critical items in the novel misinformation paradigm were not counterbalanced. Many researchers recommend misinformation studies should have a set of critical items, half of which participants are misinformed about and the other half being control items. These items should be counterbalanced across misinformation and control conditions (e.g., Takarangi et al., 2006). Counterbalancing helps to increase certainty that misinformation effects are not specific to certain items. Details can differ in susceptibility to misinformation (e.g., Takarangi et al., 2006), so the specific items chosen as misinformation items in this research may have influenced the strength of the misinformation effect observed.

Counterbalancing was not employed in the current research for two reasons: First, to my knowledge this was the first study to look at the issue of misinformation effects for traumatic media content using this particular trauma film. Since the film was cut as one coherent story, it was unknown how editing it for counterbalancing purposes might affect factors such as cognitive fluency and coherence of memory. Second, the purpose of this study was to compare misinformation effects across the two conditions (CBM-App training and debrief-only control), and these conditions should have been affected equally by the lack of counterbalancing. Therefore, the lack of counterbalancing should not discredit the findings, but may produce concerns regarding the generalisability of effects to other to-be-remembered events.

Another possible limitation is that the experiments may have been under-powered for ROC analyses. Since ROC estimates discriminability at multiple levels of decision criteria, ROC analyses require a large number of data points for accurate estimation of parameters (Kovera & Evelo, 2021). This can be achieved through large sample sizes and/or a substantial number of trials. Kovera and

Evelo (2021) noted the mean sample size for psychological studies using ROC was approximately 627 participants (*SD* = 1118.35). This is more than twice the number of participants (*n* = 257) in Experiment Two of the present research. It is therefore possible the present research was under-powered to detect statistically significant differences between groups in misinformation discriminability. Alternatively, it may be that even if the research were adequately powered, the effect of CBM-App training on misinformation endorsement may be so small that it is not meaningful and holds limited implications for real-world settings. Future studies should aim to replicate this research with a larger sample size and/or more recognition test items to increase statistical power and further clarify the effects of CBM-App training on susceptibility to misinformation.

The therapeutic outcome measures used in this study may also limit the generalisability of findings. The primary therapeutic outcome measures (i.e., memory vividness and emotionality ratings) were chosen due to their use in similar research and their theoretical mediating effect on susceptibility to misinformation. However, there currently appears to be no applications of memory vividness or emotionality in CBM-App training research, with most previous applications occurring the context of eye movements in EMDR therapy (e.g., Houben et al., 2018). As a result, there is currently no literature validating these scales as a measure of CBM-App training outcomes. It is recommended that future researchers investigate the effectiveness of CBM-App training, and its potential drawbacks, using measures that more closely reflect the mechanism being targeted, such as the PTCI and IES-R.

The present research was also limited in its ability to isolate the effects of CBM-App training on memory vividness/emotionality or susceptibility to misinformation. This is because participants in the CBM-App training group also received a post-trauma debrief. Therefore, from a mechanism perspective, the true effect of CBM-App training was not observed, but rather its combined effect with other interventions. To examine the effects of CBM-App training more clearly, future studies would benefit from applying CBM-App training as a standalone intervention in comparison to a stringent control condition.

Lastly, while this research aimed to improve the ecological validity of misinformation studies in a trauma context, there remain some limitations in this area. First, in real-world situations it is unlikely people would seek support immediately after being exposed to a traumatic event or to distressing video footage, as occurred in this study. Instead, it is more likely support would be sought sometime after the initial event, and only if the person experienced ongoing trauma-related difficulties. Therefore, the effects of CBM-App training in this study cannot be generalised to situations where the intervention is applied some time after the initial traumatic event. Second, the delay between completing the intervention and exposure to misinformation does not mimic what might occur if misinformation were introduced within the therapeutic setting. This delay may also have dampened the effects of the intervention on misinformation susceptibility. Future studies could therefore explore the effects of CBM-App training (and other therapeutic interventions) on misinformation effects at varying time points in relation to misinformation exposure.

Concluding Comments

This research investigated the effects of CBM-App training on trauma memory vividness and emotionality and susceptibility to misinformation. The novelty of this research is that it demonstrates the application of a computerised trauma intervention and misinformation paradigm in an online trauma context. In an initial study, I demonstrated the effectiveness of novel materials and procedures for investigating misinformation effects in an online context. These materials and procedures were then employed in a second experiment investigating the misinformation potential of a computerised trauma intervention: CBM-App training. Findings showed an intervention designed to modify negative cognitive appraisals (CBM-App training) did not produce subsequent reductions in the vividness or emotional intensity of memory for a traumatic viral-style video. Modifying negative cognitive appraisals did not increase susceptibility to post-event misinformation about the traumatic video.

This research sheds further light on the potential impact of online exposure to traumatic material on trauma-related distress and highlights the need to develop effective trauma interventions that can be readily dispersed to large groups. Although the findings raise questions about the efficacy of CBM-App training for reducing trauma-related distress, the results also indicate memory distortion and increased susceptibility to misinformation may not be a drawback of this intervention. Moreover, the results provide initial evidence for the possible effectiveness of post-trauma debriefing. This has implications not only for those who work with traumatic media content (e.g., child exploitation investigators, journalists, social media content moderators), but also the general public, who are increasingly at risk of being exposed (wittingly and unwittingly) to distressing material online. Additional research is needed to understand the effectiveness of CBM-App training and post-trauma debriefing for reducing trauma-related distress in this emerging context.

The current research provides an important contribution to the misinformation literature, as it is one of few studies to investigate misinformation effects specifically in an online trauma context. This research has also reinforced past research demonstrating the value of ROC analyses for increasing detection of subtle effects of misinformation on recognition confidence judgements (Horry et al., 2012, 2014; Sauer et al., 2008, 2012). As such, the present research develops a foundation for follow-up studies in this field. Given the increasing potential for traumatic content and misinformation to circulate online, additional research is imperative to understand the factors that may contribute to or mitigate trauma-related distress and memory distortions in this context. Further research using alternative outcome measures, improved statistical power, and a longer intervention phase would provide further insights to the effectiveness of CBM-App training and to the validity of the misinformation paradigm developed here. It is hoped the work conducted in this thesis to develop these novel misinformation materials for an online context will facilitate further research in this area.

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Appendices

Appendix A

Post-Trauma Debrief Video Script

The target event in our study was a 10-minute video comprised of excerpts from the fictional film 'Zero Day', directed by Ben Coccio (2002). The film depicts two teenage boys planning, organising, and carrying out a school shooting using ostensible home-video footage and school security camera recordings. Further information about the film can be accessed at: https://en.wikipedia.org/wiki/Zero_day

For use of this video in research, we recommend purchasing a copy of the original film.

Using iMovie, we extracted clips from the film at the following times:

2:29 – 3:07

15:39 – 16.26

1:08:01 – 1:19:20

These clips showed the two teenage boys introducing themselves and their plan to attack their school using a home-video camera. One of the boys was shown driving a car and talking about his hatred for his classmates and how the boys were going to get away with the attack. In the final series of scenes, the boys are shown parking up at their school, loading some guns, and entering the school. The video then switches to security camera footage inside the school, where the boys begin shooting at other students. One student calls emergency services but gets shot and one of the shooters picks up the phone. For the remainder of the video, audio is portrayed through the phone call to the emergency dispatch officer, who pleads with the two shooters. After shooting multiple students, the video ends with the two shooters killing themselves. We shortened the above excerpts to fit within 10-minutes, editing out some conversations between the two shooters, and footage of the shooters walking through the school.

Appendix B

Instructions for the Positive Cognitive Bias Modification – Appraisal Training Task

The following instructions were provided to participants assigned to the CBM-App training condition in Experiment Two.

<u>Sentence Completion Task</u>

In this task you will be asked to read a series of statements.

As you will see, most of these statements relate to thoughts and feelings that people have had if they have been through a traumatic experience.

As you read these descriptions, we want you to think back to the video of the school shooting and try to imagine again what it was like.

Then, when you read these statements your task if to imagine yourself in the situation described, keeping in mind the images from the video.

The last word of the statement will be incomplete (like this: w – r - : both the 'o' and the 'd' are missing from 'word').

Your job is to use the statement you have just read to help you complete the word correctly, so that it finishes the statement.

When you know what the incomplete word is, type the FIRST missing letter into the empty box below the sentence and click "next" (this would be 'o' in the example above). The correct word will be displayed on the next screen for practice statements, but not during the real task.

After a number of such statements, you will be asked a yes/no question about a randomly selected statement to see if you have understood it.

Don’t worry if this seems complicated – you will do some practice first with neutral statements which should make it clearer. To try an example, click the “next” button below.

End of practice

You will now begin the main task for the Sentence Completion Task.

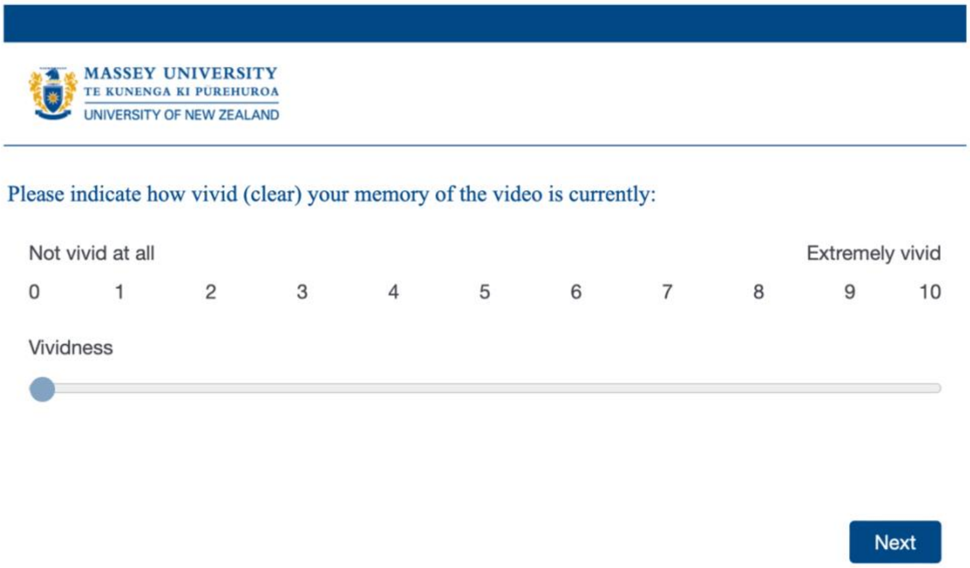
Remember to only type the first missing letter for each incomplete sentence. Correct answers will not be displayed after each sentence during the main task.

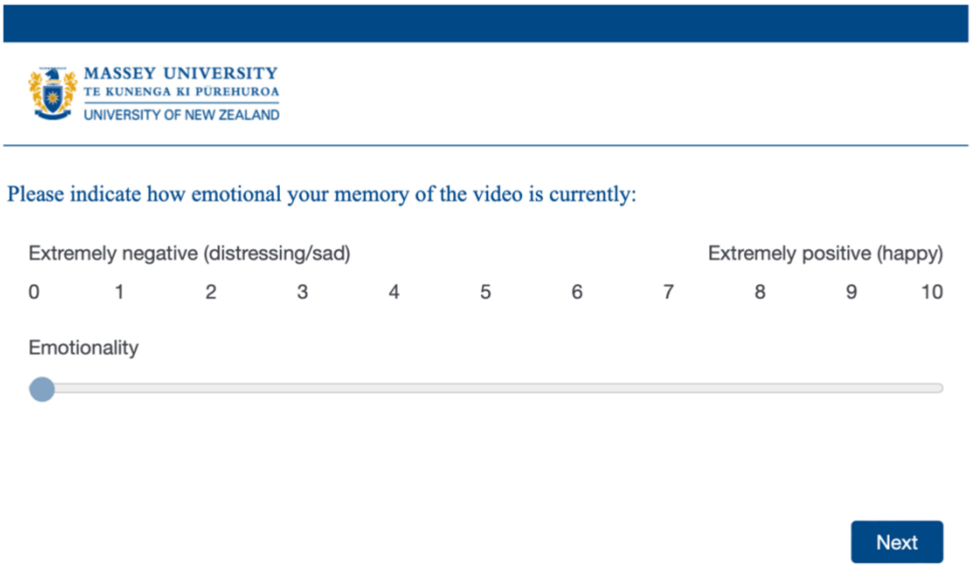
After a block of sentences, you will be asked a yes/no question about one of the sentences to see if you have understood it.

Click “next” to begin the Sentence Completion Task.

Appendix C

Rating Scales for Memory Vividness and Emotionality





Appendix D

Misinformation Narrative

Eyewitness:

"When the video started, I saw two teenage boys standing outside of a school talking to a video camera. There was a blonde boy called Cal and a dark-haired boy wearing a hat named Andre. The boys were talking about their plan to attack their school, which they referred to as Zero Day. Then the video switched to show Andre driving a car and talking about killing people as if they were spiders.

Next, the video showed the boys driving to their school and parking outside. Andre was in the driver's seat of the car and he was wearing a blue t-shirt. He got out of the car and retrieved the first gun from the backseat of the car. The video then showed the boys sitting in the car and loading the guns. The two boys were discussing who was going to take which gun, and I heard Andre agree to take the glock. Then the video camera was placed on the car dashboard and the boys were shown running towards the school with their guns.

The video then switched to the view from the security cameras inside the school. I saw a group of high school students standing around talking to each other. Then I heard a phone call to an emergency service dispatch person and a bunch of students started running around and screaming. The two shooters came into view and they started shooting at the students and chased after them as they ran away. One of the shooters took the phone from the victim who had called emergency services and he started yelling at the lady on the phone.

The shooters then walked into a room with lots of desks and chairs and people were screaming and trying to hide. The emergency dispatch lady started calling out for Andre to pick up the phone. I saw one of the shooters tip some chairs over, and then Andre started shooting at some people under the desks.

Cal stood up on top of the desks and started tormenting a female victim, who was screaming. He yelled out "why are you still alive?" The perpetrators then found a boy named Thomas hiding behind a couch and they shot him multiple times. Another male victim managed to crawl out from under a desk, quickly grabbing his cellphone before escaping the room.

Next, the perpetrators looked out a window as they heard police car sirens approaching. The two shooters started talking about shooting themselves, but Andre was nervous. The emergency dispatch lady was pleading with Andre to pick up the phone. Eventually, Cal convinced Andre. The video ended with the two boys standing on the desks and shooting themselves."

Appendix E

Recognition Test

<u>Instructions:</u>

In the following section you will be presented with some statements relating to the ***video of the school shooting*** that you watched in Part 1 of this study. For each statement, you will be asked to rate how confident you are that the statement is true or false on a scale from 1 (certain this is false) to 6 (certain this is true). Please respond as accurately as possible ***based on what you remember from the video***.

Click "next" to begin answering the questions.

<u>Test Questions (presented in random order):</u>

1. Andre was wearing a blue t-shirt on the day of the shootings
2. The blonde perpetrator's name was Cal
3. A male victim managed to successfully escape from under a desk and quickly grabbed his cellphone before escaping
4. A male victim, who was hiding under a desk, tried to reason with the shooters
5. During the shooting Andre tipped over a chair
6. Andre agreed to take the glock
7. Andre was wearing a hat on his head when he was first shown in the video
8. The dark-haired perpetrator's name was Chris
9. The perpetrators heard the police car sirens

10. The first boy who was found hiding behind the couch was named Thomas
11. During the shooting Cal yelled at a victim "Why are you still alive?"
12. The perpetrators placed the video camera in the glovebox before getting out of the car
13. The first gun was retrieved from the backseat of the car
14. Andre compared his victims to spiders
15. Andre was nervous about shooting himself
16. Three victims were found hiding behind the couch
17. During the shooting Cal stood on the desks
18. The emergency dispatch lady attempted to talk to Andre on the phone
19. Andre was driving the car to the school before the shooting began
20. The perpetrators named the day of the shootings 'Zero Day'
21. The perpetrators loaded the guns while sitting in the car
22. Immediately before shooting themselves, the perpetrators were talking about how much they would miss their families
23. When the perpetrators shot themselves they were standing on the desks
24. Andre was wearing a blue t-shirt on the day of the shootings

Appendix F

Confirmatory Analysis Data

Figure F1

Boxplots Showing the Distribution of Cognitive Bias Index Scores for the CBM-App Training Group and Control Group.

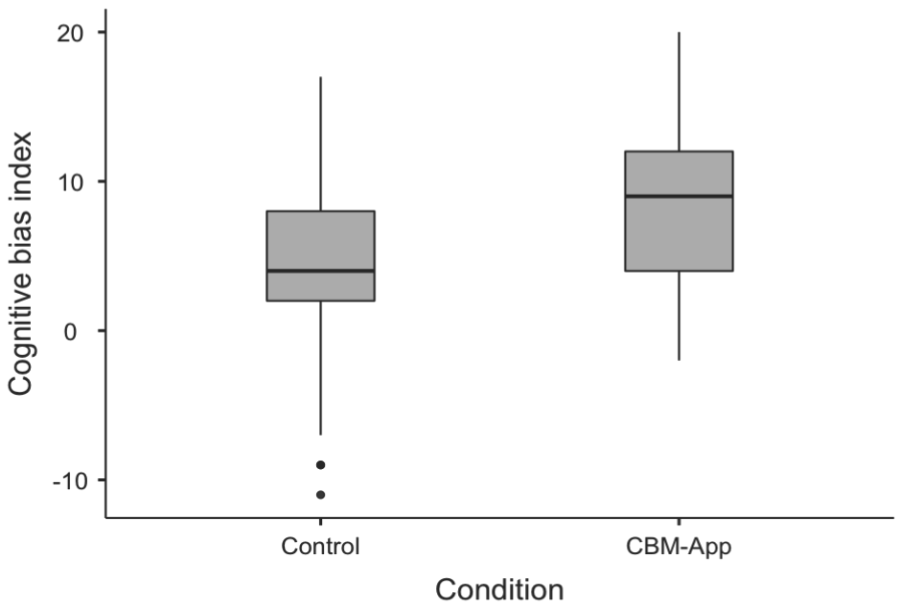


Figure F2

Boxplots Showing the Distribution of d-prime (d') for the CBM-App Training Group and Control Group.

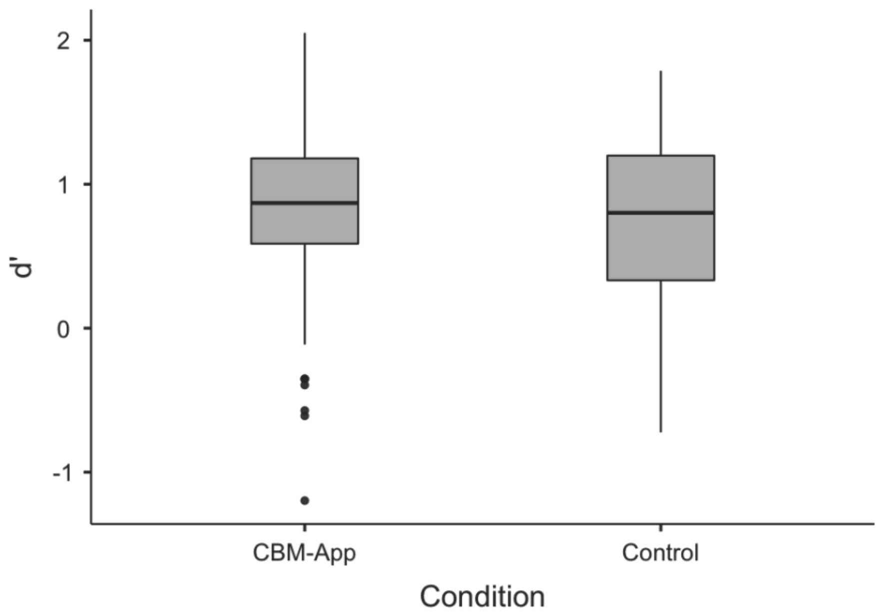


Figure F3

Boxplots Showing the Distribution of Response Bias (c) for the CBM-App Training Group and Control Group.

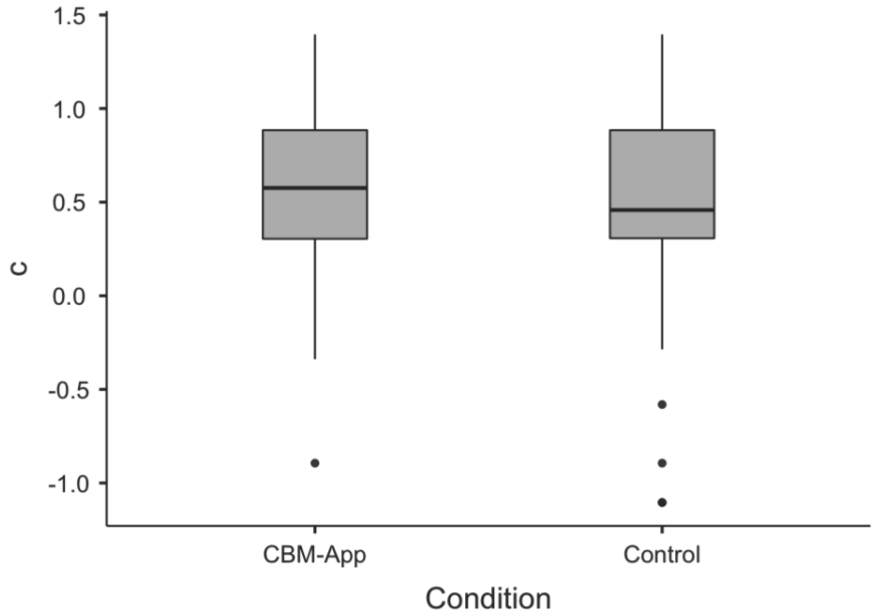


Figure F4

Boxplots Showing the Distribution of Area Under the Curve (AUC) for the CBM-App Training Group and Control Group.

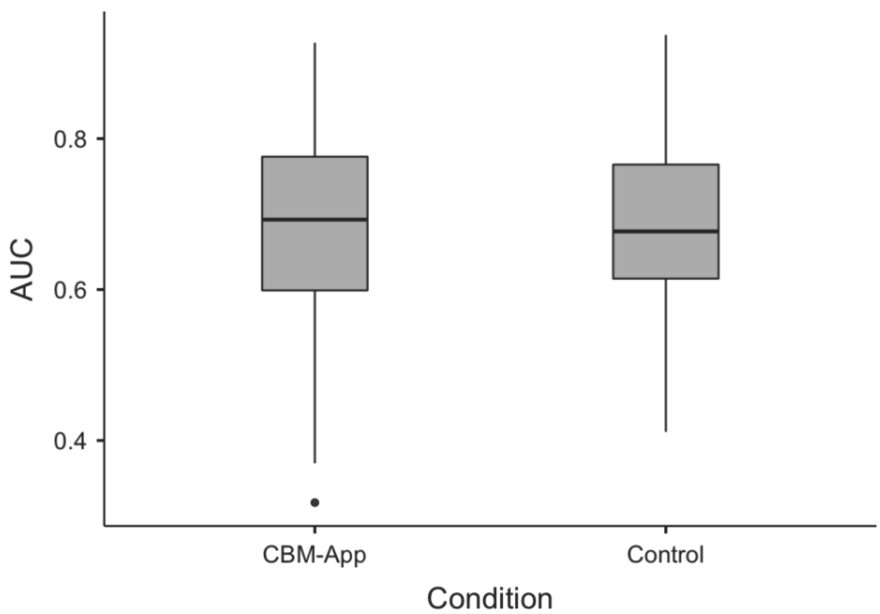


Figure F5

Boxplots Showing the Distribution of Hit Rates for the CBM-App Training Group and Control Group.

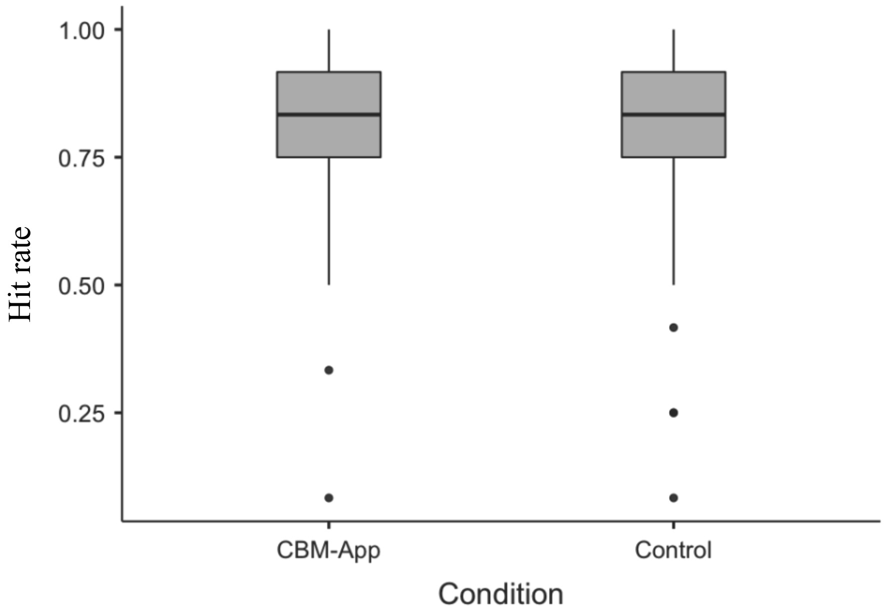


Figure F6

Boxplots Showing the Distribution of False Alarm Rates for the CBM-App Training Group and Control Group.

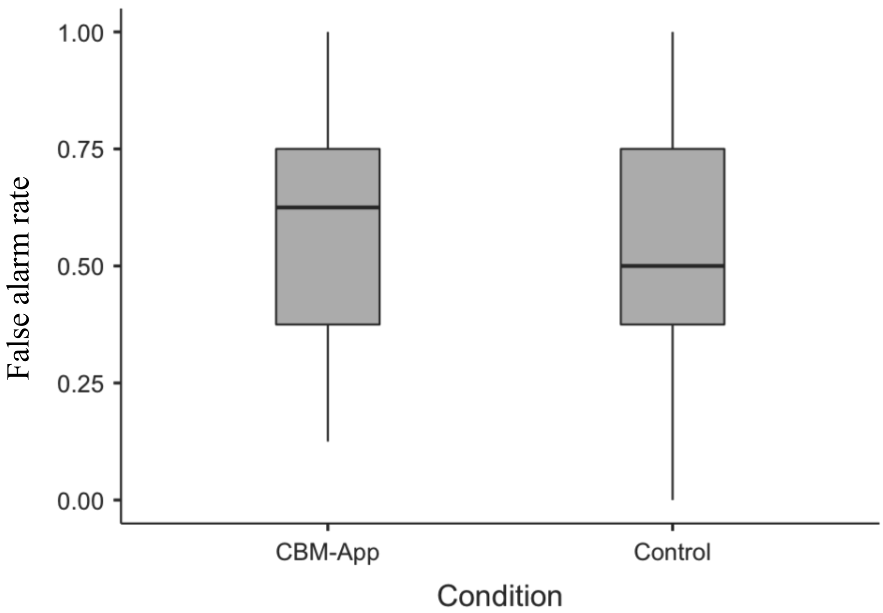


Figure F7

Boxplots Showing the Distribution of Memory Emotionality Ratings at T1 (Pre-Intervention) for the CBM-App Training Group and Control Group.

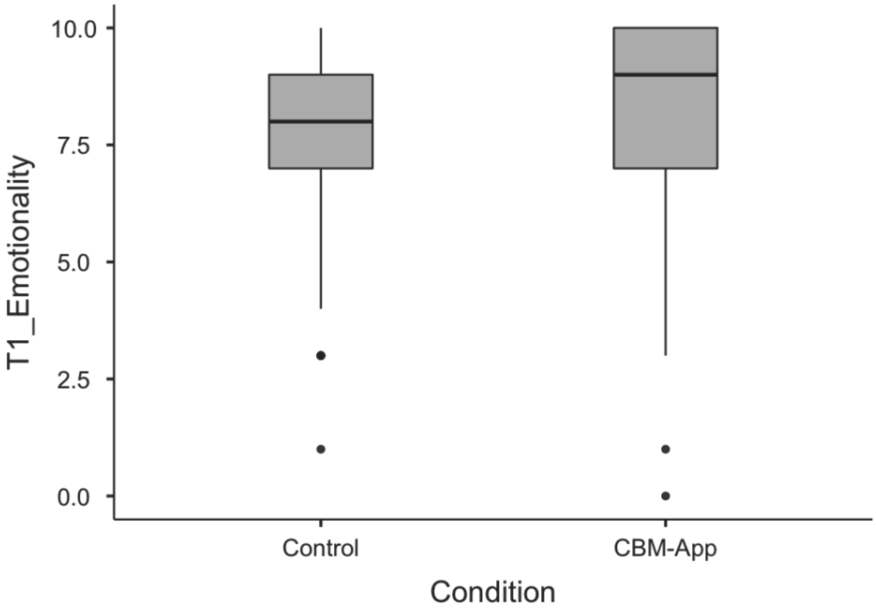


Figure F8

Boxplots Showing the Distribution of Memory Emotionality Ratings at T2 (Immediately Post-Intervention) for the CBM-App Training Group and Control Group.

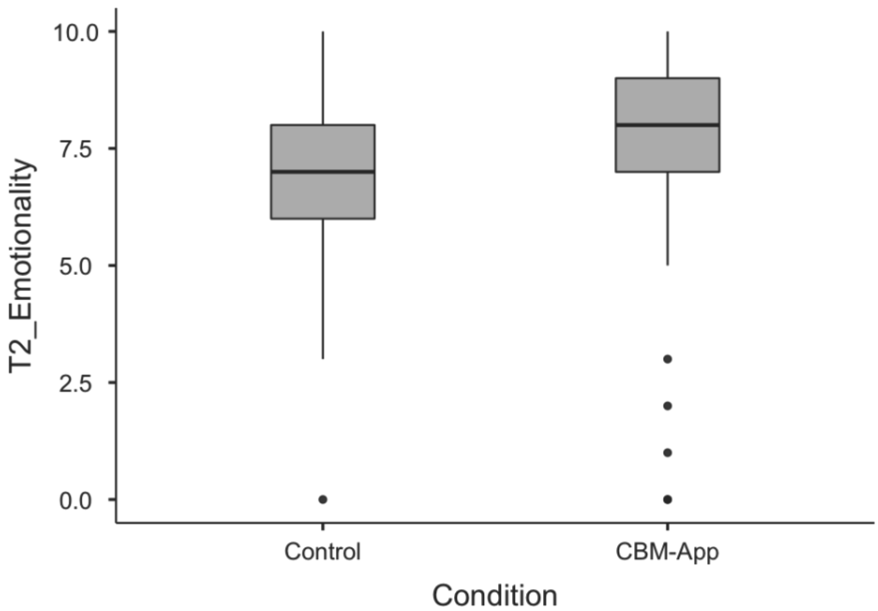


Figure F9

Boxplots Showing the Distribution of Memory Emotionality Ratings at T3 (One-Week Post-Intervention) for the CBM-App Training Group and Control Group.

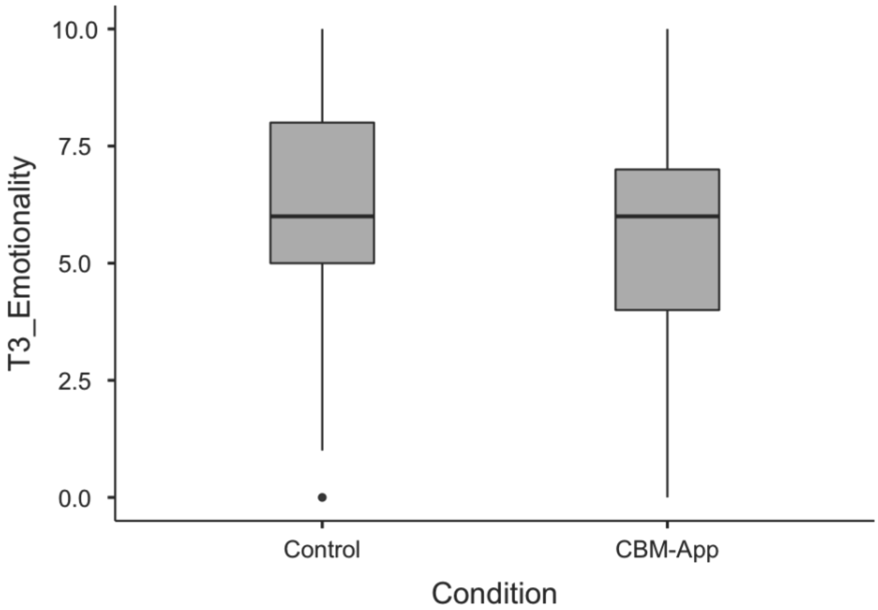


Figure F10

Boxplots Showing the Distribution of Memory Vividness Ratings at T1 (Pre-Intervention) for the CBM-App Training Group and Control Group.

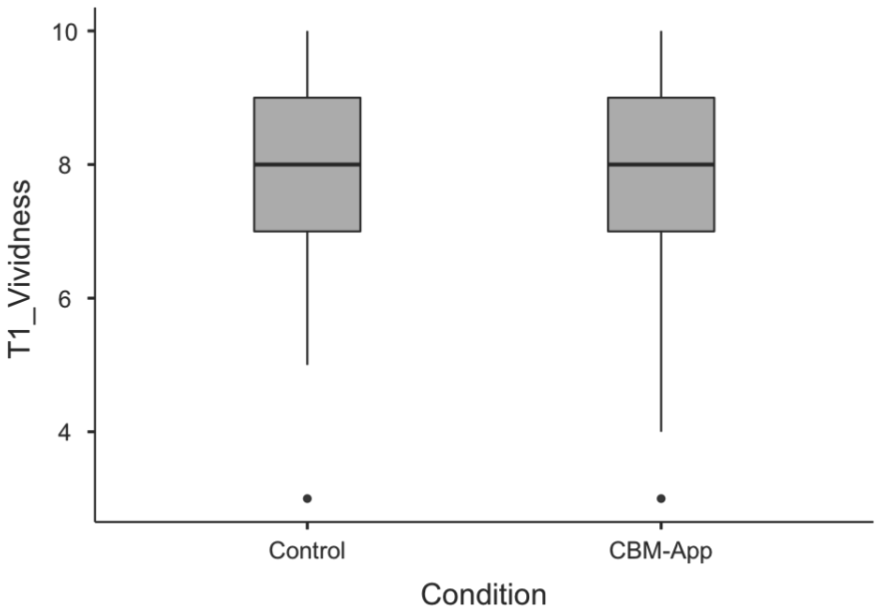


Figure F11

Boxplots Showing the Distribution of Memory Vividness Ratings at T2 (Immediately Post-Intervention) for the CBM-App Training Group and Control Group.

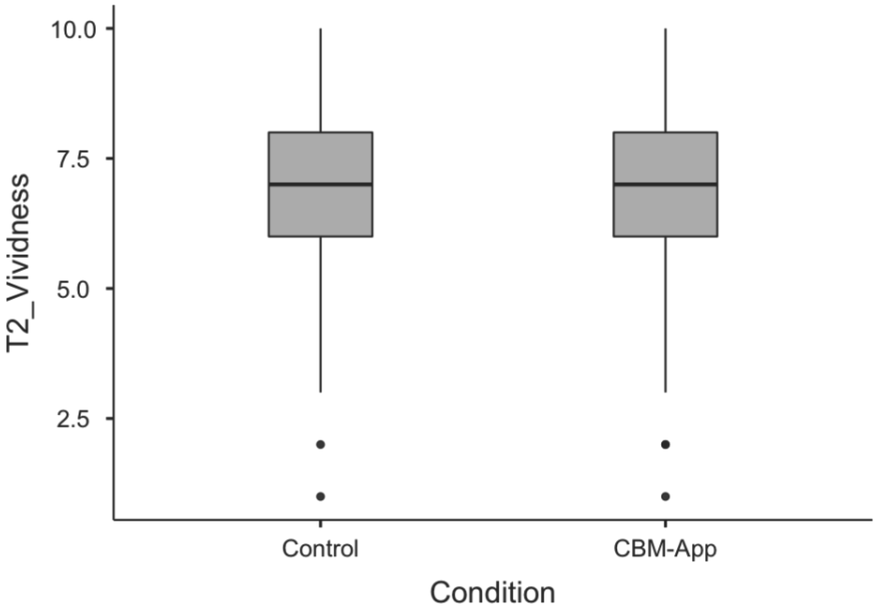
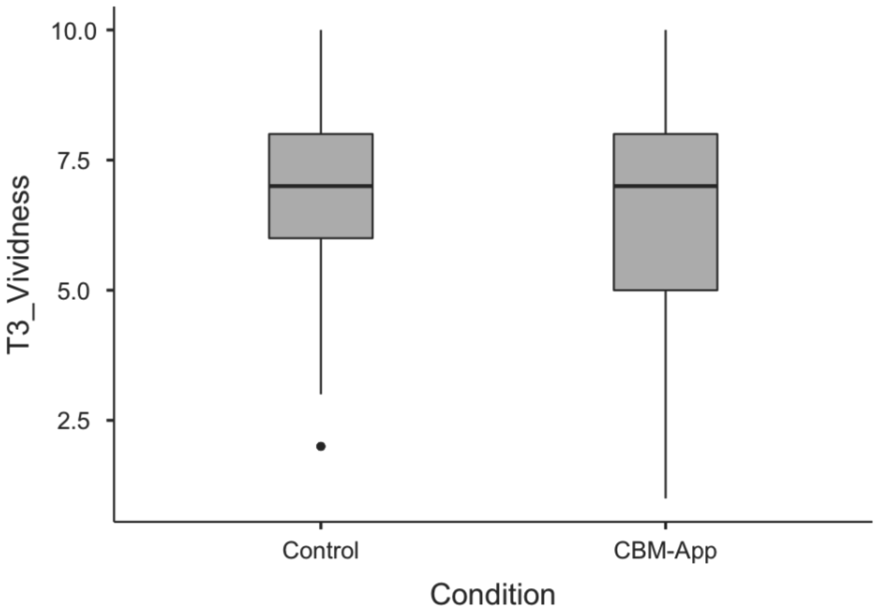


Figure F12

Boxplots Showing the Distribution of Memory Vividness Ratings at T3 (One-Week Post-Intervention) for the CBM-App Training Group and Control Group.



Appendix G

Research Case Study: The Impact of my Doctoral Thesis on my Clinical Practice

This case study was completed during the period of an internship as part of a Doctor of Clinical Psychology

In accordance with the Code of Ethics for Psychologists Working in Aotearoa/New Zealand the privacy of this client is maintained by utilising pseudonyms and adapting identifying information.

Abstract

This case study considers the impact of my doctoral research on my practice as an intern psychologist this year at Ara Poutama Aotearoa. My doctoral research focused on memory for traumatic material viewed online and investigated the effects of a computerised trauma intervention on susceptibility to misinformation. The case study begins with a brief overview of the relevant literature, research aims and hypotheses, and method for my doctoral research. This provides the context in which I gained skills and knowledge that I was able to apply during my internship. The case study concludes with reflections on the contributions of my research to my clinical practice. Reflections focus on the development of my knowledge regarding theories of trauma, differences between single-event and complex trauma, the impact of vicarious trauma in my work, and learnings from my experiences through COVID-19.

Doctoral Thesis Overview

Rationale for Research

The proliferation of social media use has meant we are at increased risk of being exposed to potentially traumatic events and erroneous information about real-world events (Peterson & Densley, 2017; Redmond et al., 2019). Exposure to misleading information can infiltrate and distort our memory of an event; this is known as the misinformation effect (Loftus, 2005). Misinformation effects are problematic because they reduce the reliability of our memory in forensic eyewitness contexts (Loftus, 2018), and can exacerbate distress. Moreover, once exposed to misinformation, it is extremely difficult to correct or negate the information (Chan et al., 2017; Swire et al., 2017).

Some researchers argue that memory for traumatic or distressing events are more vulnerable to misinformation effects (Knott et al., 2018; Porter et al., 2008; Strange & Takarangi, 2015). In addition, there is evidence to suggest certain therapeutic techniques used to reduce trauma-related distress may further increase susceptibility to misinformation (Houben et al., 2018). The premise of these concerns focuses on the notion that many trauma therapies aim to reduce the vividness and emotional intensity of trauma memories – and this reduction may leave memory vulnerable to misinformation effects. However, research in this field is conflicted, with subsequent replications failing to find any misinformation effects (Calvillo & Emami, 2019; Sievwright, 2018; van Schie & Leer, 2019).

One explanation for the failed replications is that existing methods and materials for instilling misinformation effects in experimental research are not well-suited to investigating misinformation effects in the context of trauma and therapy. The present research aimed to address this issue through the development and testing of novel misinformation stimuli and procedures. The novel experimental method was then used to investigate whether a computer-administered trauma intervention can reduce the vividness and emotional intensity of traumatic memory without increasing susceptibility to misinformation.

Research Aims and Hypotheses

The present research investigated misinformation effects in two online experiments. Experiment one explored the effectiveness of a novel paradigm for inducing misinformation effects in an online trauma context. Experiment two primarily addressed the question "can a computerised trauma intervention reduce the vividness and emotional intensity of memories for a traumatic video without increasing susceptibility to misinformation?" The hypotheses for each experiment are outlined below. Additional preliminary and exploratory analyses were also conducted during the present research which are not discussed in this case study.

Experiment One

The first experiment of the present research explored the effect of exposure to post-event misinformation about a witnessed traumatic video on later endorsement of misinformation using novel stimuli and procedures. In attempt to improve upon existing misinformation paradigms, this experiment was conducted with an ostensibly realistic online viral-style video for the traumatic event. A longer retention interval of five days was used between witnessing the traumatic event and exposure to misinformation to mimic realistic exposure and memory retrieval timeframes and to promote more misremembering. The experiment also used more sensitive estimates of misinformation effects by measuring participants' confidence in their memory reports and using this to determine discriminability (d-prime) and points on a receiver operating characteristic (ROC) curve. Consistent with decades of research demonstrating the effect of post-event misinformation on memory, it was hypothesised that:

1. Participants who received misinformation about a traumatic video would have a lower d-prime value (indicating a poorer ability to discriminate between post-event misinformation and original event details at test) than participants who did not receive any misinformation.
2. Participants who received misinformation about a traumatic video would have a smaller area under the ROC curve (indicating poorer accuracy in discriminating between misinformation and original event details) compared to participants who did not receive any misinformation.

Experiment Two

The second experiment of the present research used the novel experimental paradigm developed in experiment one to test the effects of a computerised trauma intervention on memory vividness, emotional intensity of memory, and susceptibility to misinformation. The computerised trauma intervention used in this study was Cognitive Bias Modification-Appraisal (CBM-App) training (Woud et al., 2012). CBM-App training is an intervention based on components of cognitive-behavioural therapy. It is designed as a single-use early intervention to reduce post-trauma reactions at a sub-clinical level (Woud et al., 2012, 2018). At its core, CBM-App training focuses on reframing maladaptive cognitive appraisals regarding a traumatic event and one's ability to cope in the aftermath of that event. Although research suggests CBM-App training is effective in reducing trauma-related distress (e.g., de Kleine et al., 2019; Vermeulen et al., 2019; Woud et al., 2013), there is also theoretical reason to believe this intervention may increase susceptibility to post-event misinformation. This is because shifts in the way people appraise events can alter the way these autobiographical events are remembered (Patihis et al., 2019). In light of this, it was hypothesised that:

1. Participants who received CBM-App training would have a lower d-prime value (indicating poorer ability to discriminate between misinformation and original event details at test) compared to participants who received a control intervention.
2. Participants who received CBM-App training would have a smaller area under the ROC curve (indicating poorer discrimination accuracy) compared to participants who received a control intervention.

Since experiment two was also concerned with the effects of CBM-App training as a computerised intervention for reducing the emotional intensity and vividness of trauma memories, the following hypotheses were also developed:

3. Participants who received CBM-App training would experience greater decreases in ratings of memory emotionality from pre- to post- intervention than participants who received a control intervention.
4. Participants who received CBM-App training would experience greater decreases in ratings of memory vividness from pre- to post-intervention than participants who received a control intervention.

Method

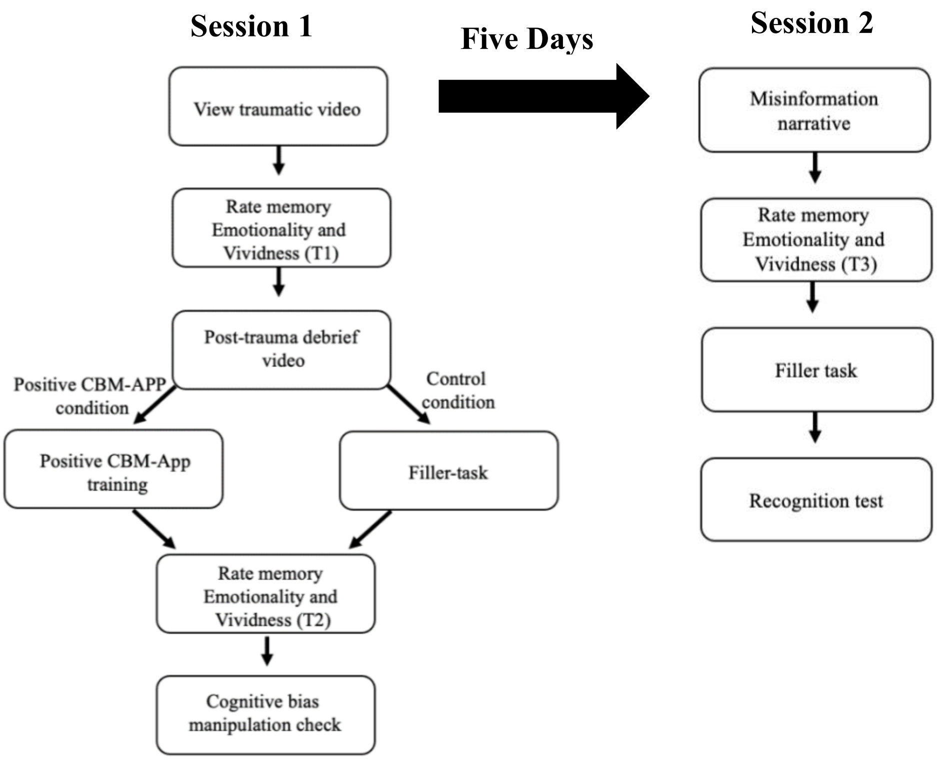
Since Experiment One largely focused on the development of the novel misinformation paradigm stimuli for use in Experiment Two, this section will focus on the methods used in Experiment Two of my thesis research.

Design

A mixed design was used with two experimental sessions spaced five days apart. Participants were randomly assigned to one of two conditions: CBM-App training or a post-trauma debrief control condition. Dependent variables included ratings of memory vividness and emotional intensity and recognition discrimination accuracy between misinformation details and original event details. Recognition discrimination accuracy was post-test-only design and memory vividness/emotionality ratings were both pre-test/post-test design across three time points (see Figure 18). The entire experiment was conducted online using Qualtrics and participants were recruited using Prolific Academic (an online crowdsourcing site).

Figure G 1

Schematic Illustration of the Experiment Flow Showing Two Experimental Sessions Spaced Five Days Apart.



Participants

The required sample size was calculated using an a priori power analysis through G*Power. This revealed that a total of 240 participants were required to detect a medium effect size of Cohen's d = 0.42 at 0.05 alpha probability error with a power of 0.90. In light of this, a convenience sample of 257 participants (140 male; 117 female; 0 non-binary) with a mean age of 32.55 years (SD =

12.30) were recruited for session one of the experiment. To be eligible to participate in the experiment, participants were required to:

1. Be currently living in New Zealand, Australia, Canada, the United States of America, or the United Kingdom
2. Be at least 18 years old
3. Have no current diagnosed mental health condition that is uncontrolled (by medication or intervention), and which has a significant impact on their daily life/activities

Prior to being invited to take part in session 1 of the experiment, all eligible participants completed the Primary Care PTSD screen for DSM-5 (PC-PTSD-5; Prins & Ouimette, 2004). Participants who endorsed any items on this measure were not eligible to participate in the experiment. This was to minimise the potential for triggering elevated levels of distress through the trauma film that were beyond the scope of the study (Weidmann et al., 2009).

After completing session 1 of the experiment, 36 participants were excluded from participating in session 2 due to failing attention checks or failing to view the full 10-minute trauma film. A further 11 participants chose not to complete session 2 and were therefore excluded from analyses. Table 6 displays the final participant demographics and the number of days between experimental sessions for each condition.

Procedure

Session 1. Figure 18 (presented earlier) displays the sequence of events during session 1 of the experiment. Participants who consented to participate in session 1 began the study by watching the 10-minute trauma film. Following this, participants rated how emotional and vivid they found the trauma video and completed the two attention checks. All participants viewed the post-trauma debrief video (control intervention), which included information about common reactions to traumatic events, helpful coping strategies, and contacts for local mental health hotlines in Australia, Canada, New Zealand, United States of America, and United Kingdom. Participants in the debrief-only control group then completed the filler task while participants in the experimental group received CBM-App training. CBM-App training required participants to complete a series of incomplete sentences relating to positive appraisals of a person's ability to cope following a traumatic event.

Table 6

Gender, Mean Age, and Mean Retention Interval Between Experimental Sessions for Participants in the CBM-App Training Condition and the Control Group.

		Condition	
		CBM-App training	Control
Demographics			
Gender	Female *n*	40	46
	Male *n*	69	55
	Non-binary *n*	0	0

Age *M* (SD)	34.00 (12.90)	32.40 (10.90)
Retention interval (days) *M* (SD)	5.65 (1.50)	5.33 (0.69)

After completing the intervention tasks, all participants again rated the emotionality and vividness of their memory for the traumatic video and then completed a manipulation check to determine whether the CBM-App training intervention increased positive appraisals. After completing the manipulation check, participants were given the opportunity to comment on their experience of participating in the study, including any issues they encountered.

Session 2. Participants who completed session 1 and passed both attention checks were invited via Prolific to complete session 2 five days later. Session 2 remained open to participants for five days, therefore session 2 was completed 5-10 days after participants completed session 1. Figure 18 also illustrates the sequence of events for session 2. Participants first read the misinformation narrative. Next, participants rated the emotionality and vividness of their memory for the trauma film they viewed during session 1. After completing a filler task for approximately 10 minutes, participants completed the recognition test for their memory of the trauma film. They were informed they would be presented with some statements about the film and were asked to rate how confident they were that each statement is true or false on a 6-point scale. Participants were instructed to respond as accurately as possible based on what they remembered from the video. At the end of the session, participants were again given the opportunity to comment on their experience of participating in the study, including if they encountered any issues.

Internship Overview

The following section describes my reflections about the impact of my doctoral research on my development as a clinical psychologist throughout my internship at Ara Poutama Aotearoa this year. During my internship, I worked with clients at Manawatu Prison, Whanganui Prison, and Whanganui Community Probation. When required, I also completed assessment and treatment via Audio Visual Link in regions that do not have a psychology team, such as New Plymouth. Assessments I completed throughout my internship were primarily for the purpose of identifying an individual's risk of reoffending, determining an appropriate rehabilitative pathway (including offending-related treatment targets), and identifying potential responsivity barriers (e.g., cognitive difficulties, mental health issues, and personality functioning). Consistent with the Risk-Need-Responsivity (RNR) model (Bonta & Andrews, 2017), individual treatment was targeted at individuals with a high risk of reoffending (usually violent or sexual offending). Treatment generally aimed to address an individual's offending risk by targeting dynamic risk factors (i.e., criminogenic needs), such as criminal attitudes, emotion dysregulation, impulsivity, substance use, and criminal peers. Clients were also referred for individual treatment to address responsivity barriers to offence-focused treatment (e.g., low motivation, cognitive difficulties, trauma, anxiety, paranoia, or personality issues that can limit an individual's ability to engage in group treatment programmes).

Internship Reflections

Trauma has a heavy presence in correctional settings. Research has demonstrated higher rates of adverse childhood experiences (ACEs) within correctional settings than in the general population (Levenson & Grady, 2016; Levenson & Willis, 2019). Although I knew about the prevalence of trauma coming into my internship with Corrections, I was still surprised at just how many of the clients I have seen this year have an extensive trauma history. The knowledge I have gained through my thesis research about trauma processes has provided me with a solid foundation for working with clients from a trauma-informed perspective. Specifically, I have been able to utilise my knowledge of the cognitive model of trauma to provide psychoeducation to clients about the symptoms they are experiencing and the role of automatic thoughts, maladaptive assumptions, core beliefs, and safety behaviours. I have found this has helped to normalise and validate the client's experiences, and this has often had an immediate positive impact on their mood and anxiety. I think providing psychoeducation is especially powerful in correctional settings since many of our clients have come from backgrounds where emotions are not talked about and are poorly understood. Providing simple information about the normality and function of their symptoms often alleviates some of the confusion and distress around what they are experiencing.

Through the lens of my thesis research, I also gained greater insight regarding the memory processes that occur in response to trauma. Understanding the fragmented nature of trauma memories and the notion of dual representation of trauma memories has influenced the way I explain intrusive symptoms to the clients I work with. I have found that explaining the idea of trauma memories being “stuck” in the brain and not processed properly has helped with socialising clients to the idea of trauma memory reprocessing. I have noticed that having this understanding and being able to communicate the rationale clearly to clients has helped to reduce some of their anxiety around engaging in trauma therapy.

My thesis research has also made me reflect on how I ask clients about possible trauma during an assessment. Being asked questions about traumatic experiences can often be triggering for clients, and, if questioning is too invasive there is a risk of breaking rapport and causing psychological/emotional harm to the client. Moreover, as my research suggests, there is a risk of inadvertently introducing misinformation and disrupting memory when asking questions about a person’s traumatic experiences. I have found this important to keep in mind throughout my internship, since I often walk into a new assessment with pre-existing knowledge about a client and their trauma history based on their file information. To avoid introducing misinformation and to help me remain objective during an assessment, I typically adopt a naïve-enquirer approach. Using this approach enables me to use open-ended questions and take the client’s lead regarding how much information they are comfortable to share about their trauma history or ACEs. I also preface my questioning about trauma/ACEs by normalising the difficulty of talking about these experiences and giving the client permission to share as much or as little as they are comfortable with.

An important tension that has been raised for me this year is the difference between single-event trauma and complex trauma. While my doctoral research focuses on single-event trauma, most of the clients I have worked with throughout my internship have extensive histories of ongoing trauma. Through this, I have recognised the way in which complex trauma can affect people on a much deeper and more personal level – often leading to issues with personality functioning and with developing a solid and positive sense of identity. Recognising these differences has enabled me to adapt my treatment plan to the client's specific needs. For example, while trauma-focused cognitive behavioural therapy (TF-CBT) may be appropriate for a client who experienced a single-event trauma, other approaches, such as elements of narrative therapy and schema therapy are often more effective for clients who have experienced complex trauma.

In many ways, my experiences throughout my doctoral research and my internship have shaped my perspective of trauma as encompassing a spectrum of responses and difficulties. It has also made me question the usefulness of the stringent criteria for PTSD in the DSM-5. Many of the clients I have seen who present with symptoms of trauma have not experienced events that would be categorised as a "traumatic event" according to the current diagnostic criteria. For example, many clients have experienced repeated rejection or abandonment during childhood yet develop similar core beliefs and schemas as a person diagnosed with PTSD (e.g., the world is unsafe/unpredictable, people cannot be trusted, I cannot cope). From this perspective, I have found it more useful to consider trauma on a spectrum and adopt a transdiagnostic approach to treatment.

Another important reflection sparked by my doctoral research is the impact of vicarious trauma in my work as an intern psychologist. My research partly demonstrates the impact of vicarious trauma (exposure to other people's trauma) on mental wellbeing. Vicarious trauma is something I have had to manage every day during my internship – both in hearing about client's trauma histories, as well as in hearing about their offending. There have been times I have felt shocked, horrified, upset, and even angry about the stories clients have shared with me. I have had to develop ways of processing this information and my emotional reactions to ensure I can remain calm and present during sessions, and to protect my own mental wellbeing. I have found supervision especially helpful in processing some of the material I come across, and I have felt comfortable being open and honest with my supervisor about my reactions to particular clients and situations. Engaging in self-care has also been vital for me in managing my own wellbeing and creating distance between my personal life and my work life.

Lastly, the challenges I faced during my doctoral research due to COVID-19 well-prepared me with the adaptability and resilience required during my internship. As a result of COVID-19 social distancing requirements, I had to make significant changes to my doctoral research to support online data collection. This was a stressful process, and required flexibility, adaptability, and resilience. Similarly, I had to make significant changes to my work as an intern this year when we went back into Level 4 lockdown. During this time, I had to work in isolation from home (after being used to working in an open-plan office with around 100 staff!) I also had to adjust to seeing clients via video link. Although this was challenging, I knew I had the ability to adapt to these changes. I understood the importance of being kind to myself and maintaining communication with my work team, as these were strategies that significantly helped me during my thesis research. In addition, the COVID-19 lockdown highlighted the issue of work-life balance, which became increasingly difficult when working from home. Since I had developed good practices during my doctoral research (e.g., setting a finish time, ending my day with exercise, turning off my computer), I was well-equipped to manage this during my internship.

DRC 16

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DOCTORATE WITH PUBLICATIONS/MANUSCRIPTS

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