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An Investigation into the Mechanisms Underlying the Memory Amplification Effect

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Table of Contents

Summary

Field research shows that trauma survivors sometimes report experiencing more trauma at follow-up compared to initial assessment. There is limited research regarding why this "memory amplification" occurs. I investigated three potential explanations for amplification.

First, because amplification is positively related to higher PTSD symptom severity, one possibility is that amplification arises from a reappraisal process: people may appraise the trauma event and past symptoms as worse than initially reported to make sense of, or justify, current symptomatology; therefore, traumatic events initially considered minor may be relabeled as significant. Consistent with this proposal, and with field data, my findings in Studies 1, 2, 3, and 4 show distress at follow-up is related to memory amplification errors. Further, in Studies 1, 2, and 3, participants with higher symptom levels made comparatively more amplification errors than participants with lower symptom severity. Interestingly, in Studies 3 and 6, I also found evidence that reappraising trauma as *less* important over time may be an adaptive adjustment mechanism that helps people regulate distress or vice versa.

A second possibility I examined is that amplification is caused by exposure to post-event information. Field amplification studies have usually investigated memory for large-scale events (e.g., war), after which people are likely to encounter new information, for example, in media reports. Numerous studies show post-event information can distort memory, likely because it shares similar details to the original event. Overlap in characteristics of memories from two sources can lead to source monitoring errors, resulting in the post-event information being inadvertently incorporated into original event memory. In Study 1, people who were exposed to media reports about their experiences and similar experiences made more amplification errors compared to those who were not. Further, in a series of analogue experiments in Study 4, participants falsely remembered more trauma

photos after post-event information exposure, suggesting that they inadvertently incorporated post-event information into their memory. Further, I found improving source monitoring with subtle warnings and source tests made participants resistant to inaccurate post-event information, suggesting that source monitoring is an underlying mechanism of amplification.

Third, trauma experiences investigated in amplification studies are often shared (e.g., soldiers in a unit). Recent research shows that sharing an experience with another person, even without communication, can enhance or amplify that experience. Therefore, a third possible mechanism for memory amplification is that people construct mental representations of others' responses (perspective-taking) and spontaneously incorporate them into their own responses. Indeed, my findings in Studies 5 and 6 suggest that aspects of shared *stressful* experiences can amplify, becoming more negative, compared to when the same experiences are not shared. I also found some evidence that this shared amplification may occur because of perspective-taking.

My thesis adds to a growing body of literature showing trauma memory can be inconsistent and inaccurate. Further, my findings support Rubin, Berntsen, and Bohni's (2008) memory-based PTSD model, which posits that trauma memory will be distorted by factors like current goals and concerns. Overall, my findings provide some support for all three explanations of memory amplification.

Declaration

I certify that this thesis:

1. does not incorporate without acknowledgment any material previously submitted or degree or diploma in any university; and
2. to the best of my knowledge and belief, does not contain any material previously published or written by another person except where due reference is made in the text.

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Chapter One: Memory Distortion of Negative Experiences

Trauma Memories and PTSD

According to current diagnostic criteria (*DSM-5*; American Psychiatric Association, 2013), a diagnosis of Posttraumatic Stress Disorder (PTSD) requires exposure to a traumatic event that leads to specific symptoms (negative cognitions and mood, arousal, avoidance and re-experiencing). There is an implied causal model in these diagnostic criteria: a traumatic event causes PTSD symptoms. Critically, however, clinicians are often unable to "measure" or corroborate clients' trauma events. Instead, they rely on the client's *memory* of the traumatic event. Therefore, theorists such as Rubin, Berntsen, and Bohni (2008) add a memory component to the DSM-5 model described above, suggesting that PTSD symptoms arise from the memory of the event rather than the event itself.

Traumatic Memories: Fixed or Malleable?

Theoretical perspectives on the effect of trauma on memory are inconsistent, and even contradictory at times. Some clinicians and theorists believe that our memories for trauma are special (see Shobe and Kihlstrom, 1997 for review), and therefore encoded and remembered differently compared to everyday memories. For example, Brown and Kulik (1977) proposed that surprising and emotional events are special because they result in "flashbulb" memories that are long-lasting and accurate. Repression theory instead essentially suggests that trauma memory is special because it can be buried in the unconscious (Freud, 1915). In a similar vein, Brewin's dual representation theory of PTSD posits that some sensory details (e.g., visuospatial information) and emotional states experienced during a traumatic event can only be accessed through exposure to relevant contextual cues (e.g., when hearing of a similar trauma on television); that is, they cannot be accessed voluntarily, and are also often difficult to edit (Brewin & Burgess, 2014; Brewin, Dalgleish, & Joseph, 1996). The diagnostic criteria for PTSD also indicate that traumatic events can lead to memory problems: people will both

avoid thoughts of the trauma and find it difficult to forget the trauma due to re-experiencing symptoms (e.g., intrusive thoughts and images of the event). Beyond clinicians and theorists, most laypeople also believe their memory for shocking events is accurate (see Talarico & Rubin, 2007).

Flashbulb memory. Early field studies seemingly provided support for this assumption that memories for emotional events are special and fixed. In Brown and Kulik (1977), for example, participants recalled the circumstances (e.g., where they were, what they felt) in which they learned of ten shocking events (e.g., JFK's assassination) years after the events occurred. The researchers found that people vividly and confidently recalled the events and concluded, therefore, that there is a special brain mechanism that gives a "now print!" order to preserve memories of greatly shocking, emotional events. In other words, these shocking "flashbulb memories" are recorded into memory with perfect accuracy and are immune to normal memory flaws like decay or distortion over time.

However, there are several limitations to Brown and Kulik's (1977) study and others like it. First, these studies did not compare memory for shocking, emotional events with memory for more ordinary, or unsurprising events. Therefore, it is not possible to conclude that a special mechanism exists solely for shocking, emotional events. Second, people rehearse emotional events internally (e.g., via rumination) and externally (e.g., by verbal discussion) more than everyday memories (Talarico & Rubin, 2007); therefore, ordinary rehearsal mechanisms may explain the enhanced vividness of memory for emotional events. Third, reported details cannot be corroborated, making it impossible to discern whether memory reports that feel vivid are actually accurate or distorted. Indeed, many later field studies have shown that while flashbulb memories are remembered vividly and confidently, they are also, in fact, remembered *inconsistently* over time (e.g., Day & Ross, 2014; Neisser & Harsch, 1992; Talarico & Rubin, 2003). For example, Schmolck, Buffalo, and Squire

(2000) asked college students three days and again 15 or 32 months after the verdict for the O.J. Simpson murder trial was announced how they heard the news. After 15 months, only 50% of the recollections were highly accurate, and 11% contained major distortions. After 32 months, only 29% of the recollects were highly accurate, and over 40% contain major distortions. Therefore, memory was inconsistent and became even more so with a longer delay, similar to the decay of memories for more ordinary events.

Repression. In 1989, George Franklin was accused by his daughter, Eileen Franklin-Lipsker, of the rape and murder of her childhood friend who had been killed in 1969. Despite having no memory of this event for twenty years, Eileen insisted that she recovered her memory of the crime 20 years later. Soon after the Franklin case, a string of other cases arose involving accurate recovered memories that had been previously "repressed." Repression theory, originally suggested by Freud (1915), is a considered a special memory device whereby the mind buries information into the unconscious. While the repressed information cannot be remembered, it still leads to various psychological problems until it is recovered. Because memory recovery often occurs via therapeutic intervention, there has been debate about the authenticity of such memories. Critics of repression have noted at least four key points against repression theory. First, critics have pointed out that most of the evidence for repressed memory derives from case studies or surveys, meaning that there is rarely evidence that the events occurred. Second, rather than repressing trauma memories, there is considerable evidence that people have difficulty *forgetting* traumatic events (Kessler Sonnega, Bromet, Hughes, & Nelson, 1995). Third, there is theory and research to suggest that people sometimes forget prior episodes of remembering (see e.g., Arnold & Lindsay, 2002; Schooler, Bendiksen, & Ambadar, 1997). In other words, it is possible for individuals to forget remembering a traumatic experience for a period of time and later remember the experience, a "forgot-it-all-along" phenomenon that is sometimes interpreted as recovering a

repressed memory. Fourth, and most important for the purpose of this thesis, critics have pointed out memories are easily distorted and implanted—therapeutic interventions could lead people to falsely believe that they were abused.

Emotion and arousal. There is evidence that some details of trauma events are enhanced in memory while others are impaired. Easterbrook (1959), for example, argued that when arousal is extreme, people will have difficulty consciously attending to all the available information during an event. In such cases, attention narrows or focuses on the arousing, central details. In other words, arousal facilitates encoding for central, but not peripheral, details. In a classic study, participants saw either an emotional slide sequence including a critical slide depicting a woman lying next to her bicycle after apparently being hit by a car, or a neutral sequence where the woman is simply riding her bicycle near the car (Christianson & Loftus, 1991). Participants who saw the emotional sequence remembered the color of the woman's coat, a central detail, better than those who saw the neutral version. However, memory for a peripheral detail (the color of the car in the distance) was impaired in the emotional condition compared to the neutral condition. Other studies have found a similar pattern of findings (see Rubin et al., 2008 for review) despite defining central details (e.g., details central to the plot of the emotional event or visually central to the event vs. details associated with material central to the person's attention regardless of whether that material is, for example, central to the plot; see e.g., Burke, Heuer, & Reisberg, 1992; Christianson & Loftus, 1991) and peripheral details differently. These findings suggest that central details for emotional events are better remembered compared to neutral events, while the opposite is true for peripheral details.

Importantly, however, memory for peripheral details of an emotional scene can be enhanced when people are instructed to pay attention to the visual details of a scene, thus, making them try to encode all elements (Kensinger, Garoff-Eaton, & Schacter, 2007;

Kensinger, Piguet, Krendl, & Corkin, 2005). In other words, a person's goals can affect which details they encode. Indeed, other studies have not found memory impairment for peripheral details (Laney, Campbell, Heuer, & Reisberg, 2004; Mather, Gorlick, & Nesmith, 2009). Mather and Sutherland's (2011) Arousal-Biased Competition (ABC) theory may explain the contradictory findings. According to this theory, arousal strengthens mental representations of high priority stimuli. Priority is determined by both bottom-up (e.g., how much stimuli "pops out") and top-down (e.g., a person's goals, knowledge, and expectations) influences. Therefore, the theory suggests that emotional narrowing findings are actually a reflection of bias to high-priority information that is often the central, emotion details. As such, if peripheral details were to be considered high-priority information, they too would be encoded. In other words, attention can be drawn away from central details if goals are manipulated.

External suggestion. Laboratory experiments from several lines of research confirm that emotional, and even traumatic memories distort. First, emotional memories have been found to be susceptible to misinformation. In a misinformation paradigm, participants witness an event before receiving additional information containing both accurate and inaccurate details of the event. A large number of studies have found that people are less accurate on memory tests for details on which they were misled versus details on which they were not (see Loftus, 2005 for review). This finding applies for shocking emotional events as well. For example, four years after a plane crashed into an Amsterdam apartment building, Crombag et al. (1996) asked participants if they had seen the moment of impact on television—subtly suggesting that such footage existed when, in fact, it did not—and, if so, whether they could provide further details (e.g., the angle of the plane when it hit the building). Sixty-six percent of participants not only stated that they had seen the film, but also gave further details about the crash (e.g., that the plane hit the building horizontally). Ost, Vrij, Costall, and Bull (2002)

later replicated this pattern of findings, with 45% of participants reporting that they had seen a non-existent film of the car crash that killed Princess Diana. These findings suggest that this "crashing memories" misinformation paradigm leads to false memories for seeing real-life shocking events (see also Granhag, Stromwall, and Billings, 2003).

Trauma analogue studies have also found that trauma memories are prone to distortion from external suggestion. In Monds, Paterson, Kemp, and Bryant (2013), some participants were exposed to misinformation via an eyewitness statement a week after watching either a neutral or trauma film. Participants exposed to a trauma film falsely remembered 29% of misinformation (e.g., "event occurred on Thursday") at free recall, suggesting the traumatic nature of the film did not protect their memory from misinformation. In a subsequent study, Monds, Paterson and Kemp (2016) found evidence that people incorrectly recognize more misinformation after a traumatic, compared to a neutral, stimulus. Porter, Bellhouse, McDougall, ten Brinke, and Wilson (2010) embedded misinformation in open-ended questions (e.g., "what was the gender of the person holding the bike?") for a highly negative and a highly positive photo. Participants were more likely to remember a major misleading detail for the negative photo rather than the positive photo. Therefore, it is possible that negative emotion *increases* susceptibility to misleading information.

Trauma memory distortion can also occur spontaneously without external suggestion. For example, in Strange and Takarangi (2012), participants watched a film, cut down into small clips, depicting a fatal car accident. Six clips were removed (Missing clips) from the original film. Twenty-four hours later, participants were given a memory test comprised of Old clips (seen in the film the previous day), Missing clips, and control clips. Participants were very successful at determining whether they had seen Old and Control clips the day before. However, they falsely recognized seeing 26% of the Missing clips in the film, most of

which were emotional and traumatic in nature (see also Segovia, Strange, & Takarangi, 2016; Strange & Takarangi, 2015).

Memory Amplification

Ultimately, the research strongly suggests that trauma memory changes over time. This research, however, has examined memory errors for public or laboratory analogue events. Yet, memory distortion also occurs for *personally experienced* traumatic events, and typically follows a specific pattern: people come to remember more thematically related trauma events (e.g., unit ambushed) that occur within a larger context (e.g., military deployment) that they initially failed to remember or report, a phenomenon called memory amplification (see Giosan, Malta, Jayasinghe, Spielman, & Difede, 2009; Southwick, Morgan, Nicolaou, & Charney, 1997). Field studies with war veterans first demonstrated memory amplification. Southwick et al. (1997) asked Desert Storm veterans 1 month (Time 1) and 2 years (Time 2) after their return from war whether certain traumatic events (e.g., seeing others killed or wounded) occurred during their service. Surprisingly, most participants were inconsistent, with 70% of participants reporting events at Time 2 that they never previously reported. Similar results have been observed with peacekeepers (Roemer, Litz, Orsillo, Ehlich, & Friedman, 1998; Bolton, Gray, & Litz, 2006; Bramsen, Dirkzwager, van Esch, & van der Ploeg, 2001), Gulf War veterans (Brewer, Hallman, & Kipen, 2008; King et al., 2000; Wessely et al., 2003), soldiers deployed to Iraq (Alosco et al., 2015; Engelhard, van den Hout, & McNally, 2008) and former prisoners of war (Dekel, Solomon, & Ein-Dor, 2016) despite using different exposure scales and delay periods.

Amplification has also been found after non-combat related events including the September 11 terrorist attacks (Dekel & Bananno, 2013; Giosan et al., 2009), natural disasters (Fivush, Sales, Goldberg, Bahrick, & Parker, 2004; Heir, Piatigorsky, &Weisaeth, 2009; Weems et al., 2014), and a fatal school shooting (Schwarz, Kowalski, & McNally,

1993). For example, participants in Giosan et al. (2009) were asked at two assessment points if they had experienced eleven events (e.g., "seeing people jump") when they were deployed as disaster restoration workers at the World Trade Centre during or after the 9/11 attack. The researchers found that 62.9% of participants endorsed events at the second session that they had not endorsed in the first session. In another example, 33.1% of a community-based cohort reported experiencing potentially traumatic events (e.g., assaultive violence, sudden unexpected death (or threat of death) of close friend or relative of close friend or relative) at the second assessment point that they had not reported initially (Hepp et al., 2006).

Why does memory distortion occur?: the reconstructive nature of memory

To date, there has been little research systematically investigating why this "memory amplification" occurs. However, we *do* know that autobiographical memory is a reconstructive process; it involves "piecing together activated memorial information while at the same time making inferences based on other information available to us" (Newman & Lindsay, 2009, p. 373). A sense of current self—which includes a person's current goals, motivations and concerns—influences how people reconstruct memories (see Conway, 2005; Conway & Pleydell-Pearce, 2000). In other words, the current self may distort retrospective reports of trauma events and emotions. For example, people experiencing mood and anxiety disorders also experience memory exhibiting the same dysfunction: depressed people tend to remember more negative than positive memories (Bradley, Mogg, & Williams, 1995) and more past symptoms than initially reported (e.g., Schrader, Davis, Stefanovic, & Christie, 1990), people experiencing PTSD reported experiencing more trauma events than initially reported (e.g., Southwick et al., 1997); previously depressed people who are functioning well tend to *forget* past suicidal ideation and behaviors (Goldney, Winefield, Winefield, & Saebel, 2009). Therefore, the underlying mechanisms for amplification that this thesis will explore are all related to the current goals, motivations and concerns of the person. Specifically, I will

investigate whether current distress, source monitoring ability during post-event information exposure, and the presence of other people are all factors related to memory amplification.

Reasons for amplification. Southwick et al., (1997) identified four potential explanations for memory amplification. First, they suggest that some memories are initially repressed and then recovered over time (Explanation 1). However, this explanation is unlikely given the lack of laboratory evidence for repressed memory and the considerable evidence that: 1) people instead have difficulty forgetting traumatic events, 2) people may forget they remembered a traumatic experience for a period of time and later remember the experience, and 3) memories are easily distorted and implanted. Therefore, this explanation will not be investigated in this thesis.

Second, a few studies have found that re-experiencing symptoms (e.g., spontaneous, intrusive thoughts and memories) are the only PTSD symptom cluster related to trauma memory amplification (Giosan et al., 2009; Koenen et al., 2007; Roemer et al., 1998). It is possible that these intrusions and flashbacks may contain traces of events that are similar to their trauma but never experienced. Over time, these details may be incorporated into the person's existing memory of the trauma, resulting in memory amplification (Explanation 2). One group of researchers have already researched this possibility and therefore I will not investigate it further. In two studies, Oulton, Takarangi, and Strange (2016) exposed participants to negative photographs and assessed their memory for the photographs at two time-points. Although they did not find a memory amplification effect overall, they did find evidence that, in people who did amplify, amplification was related to re-experiencing symptoms.

The third explanation for memory amplification is related to PTSD symptomatology generally rather than to one specific cluster. Individuals experiencing PTSD symptoms at the second assessment may exaggerate or amplify their memory for trauma to understand or

explain their current distress (Explanation 3). Fourth, trauma memories may amplify after exposure to post-event information such as media reports and co-witness/victim discussions, via source monitoring errors (Explanation 4). This thesis will also investigate a fifth potential mechanism for amplification. Based on recent research, there are indications that simply sharing an event with another person could amplify the experience (Explanation 5). These three explanations (i.e., Explanations 3-5) will be further discussed below.

Amplification and current PTSD symptomatology. Some field amplification studies have found that reporting more events at follow-up is related to experiencing higher PTSD symptoms (see van Giezen, Arensman, Spinhoven, and Wolters, 2005 for review). In Engelhard et al., (2008), for example, Dutch Army soldiers were asked at 5 months and 15 months after their deployment whether certain traumatic events (e.g., having injured civilians due to their own action) had occurred during their service (yes or no). At the second assessment, 70% reported experiencing at least one event they had not reported experiencing at the first assessment; 45% remembered two or more. Critically, higher PTSD symptom severity was related to retrospectively increased reporting of traumatic stressors, that is, memory amplification. The researchers suggested that people use their current distress and concerns to appraise what they experienced in the past; therefore, distressed people may appraise the trauma event as worse than initially reported. In other words, events that were initially seen as irrelevant are relabeled as significant and endorsed in order to make sense of their psychopathology (e.g., a person describing the sight of blood as seeing human remains).

Amplification and post-event information. Field amplification studies, thus far, have usually investigated memory for large-scale events (e.g., war). After such events, people are likely to encounter copious amounts of additional information or perspectives that they were not aware of during the event, for example, in media reports, police interviews and therapy. Numerous studies have shown that memory for an event can be distorted by such post-event

misinformation. Therefore, Strange and Takarangi (2012) suggested that memory amplification may reflect a source monitoring error (Strange & Takarangi, 2012, 2015). According to the source monitoring framework (SMF; Johnson, Hashtroudi, & Lindsay, 1993; Lindsay, 2008), memory errors occur because we do not store memory with labels specifying their origins. Instead, we employ quick mental shortcuts, such as how familiar event details feel, to determine whether we truly experienced the memory. It is likely that as post-event information becomes more familiar and similar to original event memory details, it is inadvertently incorporated into the original event memory, potentially explaining the consistent finding that people amplify trauma memories.

At least two lines of research suggest that source monitoring ability plays a part in memory errors caused by inaccurate post-event information. The first is that giving a source monitoring test decreases source monitoring errors. Participants in Intraub and Hoffman (1992), for example, read paragraphs describing Old (seen in initial presentation) and New slide scenes. At test, they often claimed to have seen the corresponding slides of New paragraphs they only read about. When Dodson and Johnson (1993) replicated the study using a source monitoring test (where participants are asked to choose the source of the test item e.g., the paragraphs or the slides), they found that participants made fewer source errors, likely because source judgments cannot be made based on quick mental shortcuts. Instead, people are motivated to evaluate their memory in terms of specific sources for each item (see Lindsay & Johnson, 1989). In other words, the source monitoring test makes people more deliberate, thus increasing their source monitoring ability and reducing source monitoring errors. This finding has been replicated in multiple studies (e.g., Henkel, 2004; Lindsay & Johnson, 1989; Zaragoza & Koshmider, 1989; Zaragoza & Lane, 1994). These results suggest that source monitoring plays an important part in memory distortion caused by post-event information.

The second line of research that suggests that source monitoring ability is related to memory errors investigates the effect of giving participants an implicit or explicit warning against misinformation. Findings have been mixed and inconclusive, however. In Strange and Takarangi (2015), participants who were warned that some clips were removed before watching the film, thereby encouraging more deliberate source monitoring at test, exhibited reduced memory distortion compared to those who were not warned. By contrast, encouraging participants to adopt more heuristic (quick, unintentional) source monitoring increased distortion compared to the warning condition (Strange & Takarangi, 2015). However, the timing of the warning may have an impact on memory. In Greene, Flynn, and Loftus (1982), for example, participants were warned either before (pre-warning) or after (post-warning) misinformation that they may have been exposed to misinformation. The pre-warning, but not the post-warning, reduced misinformation suggestibility. Other studies have found that post-warnings reduce (e.g., Chambers & Zaragoza, 2001) or even eliminate (Zaragoza & Koshmider, 1989) misinformation suggestibility. Therefore, warnings may motivate subjects to engage in more deliberate source monitoring at test, thus reducing memory errors. However, given the inconsistent findings in past literature, further research is needed to determine the usefulness of warnings in reducing memory distortion.

Amplification and sharing experiences. The trauma experiences investigated in amplification studies are often shared (e.g., soldiers in a unit, people who collectively endured a school shooting, hurricane, 9/11 etc.). Recent research shows that sharing an experience with another person, even without communication, can enhance or amplify that experience (e.g., Boothby, Clark & Bargh, 2014; Boothby, Smith, Clark & Bargh, 2016; Martin et al., 2015). In Boothby et al. (2014), for example, after building rapport, participants liked a chocolate more and found it more flavorful when eating it at the same time as a confederate, compared to when eating it while the confederate completed another task. They

also liked an unpleasant-tasting chocolate less, felt more absorbed in the experience, and more on the same wavelength as the confederate and tended to think more about what the confederate was thinking and feeling when the confederate ate the chocolate with them (vs when they did not). Martin et al. (2015) investigated whether shared stressful experiences also amplify. After half their participants did a bonding exercise in pairs, all participants performed the Cold Pressor Task (CPT) twice, once in pairs and once in isolation (alone rather than next to someone who performed another task). Participants who bonded first found the CPT more intense when sharing the experience with another (vs alone). Therefore, shared stressful experiences also appear to amplify; here in terms of pain intensity.

There are two possible explanations for shared amplification. Shteynberg (2015) suggests that a shared attention state does not require a person to think about others' inner states. Instead, when a person realizes that they are simultaneously co-attending to an experience with another person with whom they will act collectively with in future, the shared attention state selectively 'switches on.' Cognitive resources are then accurately channeled to the shared stimuli, leading to enhancement of the experience. On the other hand, Smith and Mackie (2015) suggest that social influence occurs because people construct mental representations of others' responses or experiences that are spontaneously incorporated into their own responses. Indeed, this model may be supported by Boothby et al.'s (2014) finding that people tended to mentalize more when sharing the chocolate-eating experience: participants likely believed the confederate was thinking about the shared stimulus (e.g., that it tasted good or bad), and incorporated this response into their own response, thus amplifying the experience.

Summary

A large number of studies suggest that trauma memories distort over time naturally and as a result of misinformation. Specifically, people tend to remember or report

experiencing more trauma events at follow-up compared to initial assessment. In other words, their memory for personally experienced trauma amplifies over time. The mechanisms underlying this phenomenon, however, have not been systematically investigated. This thesis will, therefore, investigate four potential mechanisms of amplification: a re-appraisal process by distressed people, post-event information, and sharing the event.

Chapter Two: Overview of Thesis Studies

Study 1

My first study was modelled after previous field amplification research. In a longitudinal design, participants reported at two time-points with a six-month delay, whether they had experienced certain sexual assault events in the last five years (i.e., prior to the first time-point). The study addressed the three possible explanations for amplification discussed in Chapter One. First, to explore whether amplification is related to distress and change in distress over time, participants rated PTSD, stress, anxiety and depression symptoms at both time-points. Second, to address the possibility that amplification occurs because the large-scale events investigated in previous research were shared, my study investigated memory for sexual assault events, a usually more private type of trauma. In a survey of female victims by the National Institute of Justice (2006), 84.5% experienced rape in a private setting such as their home, with the perpetrator likely an intimate partner in most cases. Sexual assault is also rarely reported. Third, to determine whether amplification is related to potential post-event information exposure, I asked participants if they had discussed or reported their sexual assault experiences with others or saw media reports about their actual or similar experiences.

Study 2

My second study was a conceptual replication of Study 1. The methodology remained exactly the same except for one change: instead of responding yes/no to items on the exposure scale, participants rated how likely it was the event occurred. The use of a yes/no scale assumes that participants are completely sure that they either did or did not experience an event, a counterintuitive assumption given that I am investigating memory inconsistency.

Study 3

Interestingly, people's memory for *symptoms* can also change. For example, in Harvey and Bryant (2000), motor vehicle accident victims were asked to recollect acute stress

disorder (considered a precursor to PTSD) symptoms two years after initial assessment. Those who remembered experiencing more acute stress disorder symptoms than initially reported (i.e., there memory amplified) experienced high levels of PTSD severity at two years; people who forgot past ASD symptoms were experiencing low levels of PTSD severity. Data from research comparing daily versus retrospective reports of PTSD suggest that people without current PTSD are likely to remember PTSD symptoms consistently with initial reports (Naragon-Gainey et al., 2012). On the other hand, people with current PTSD may remember more symptoms retrospectively than in daily reports (Campbell et al., 2017).

To investigate the possibility that current PTSD symptom severity influences recollection of past symptoms, using the same sample as Studies 1 and 2, I asked participants to recollect, at a second assessment, their reported symptom severity at initial assessment six months earlier. I determined whether people who are PTSD-negative underestimated past PTSD symptom severity while people who are PTSD-positive overestimate past symptoms. In other words, I examined whether people take into account current concerns and attitudes to reconstruct memory of past symptoms as suggested by Rubin et al.'s (2008) memory-based model of PTSD.

Study 4

I argued in Studies 1 and 2 that people are likely to be exposed to copious amounts of PEI regarding their trauma experience when it is a large-scale event. A limited number of studies have found that repeated exposure to the same misinformation leads to more memory distortion than single exposure (Foster, Huthwaite, Yesberg, Garry, & Loftus, 2012; Mitchell & Zaragoza, 1996; Zaragoza & Mitchell, 1996). Importantly, I found no empirical studies investigating the effect of repeated misinformation for *traumatic* events. Therefore, before investigating the relationship between PEI and amplification more thoroughly in Study 5, I

ran a series of brief experiments to determine whether being exposed to repeated misinformation about negative experiences in itself leads to more memory errors.

Participants were shown a trauma film with some scenes removed. After 24 hours, participants read three "eyewitness" reports describing the film's events with some containing descriptions of removed scenes. I included both traumatic and non-traumatic misinformation items because rehearsal typically focuses on emotional elements. Therefore, people may be more likely to rehearse and incorporate emotional misinformation into their original event memory. Given that I did not find that repeated misinformation led to more memory errors than single misinformation exposure, I did not incorporate repeated PEI into my next paradigm.

Study 5

I next specifically investigated whether amplification is caused by (single) post-event information exposure, this time using a traumatic photo paradigm, across four experiments. Due to the correlational nature of Study 1, I was unable to determine whether PEI exposure *causes* amplification. It was also impossible to corroborate what participants had actually experienced, making it possible that they, in fact, became more accurate over time. Therefore, controlled laboratory experiments are necessary.

In the first experiment, I adapted Oulton, Strange and Takarangi's (2016) trauma photo paradigm, showing participants traumatic IAPS photos and testing their memory at two time-points with a delay in between. Before the second test, however, I exposed participants to PEI through photo descriptions. I examined whether participants inadvertently incorporate post-event information into their memory and, thus, amplify their trauma memory (that is, remember more photos over time if those photos are described compared to not described).

In follow-up experiments I subtly warned participants to increase their discrepancy detection by forcing them to decide whether each description was Old (i.e., they had seen a

photo of that description in the previous session) or New in the post-event information phase. I did so to provide further evidence that: a) memory amplification is directly caused by source-monitoring ability and, b) memory is reconstructed to take into account the person's current motivations, in this case improving discrepancy detection to make fewer errors. My last experiment investigated whether evaluating the source of photos increases source monitoring, making people resistant to inaccurate post-event information.

Study 6

Next, I investigated whether memory amplification is caused by shared effects. Because Study 1 found that memory for sexual assault does not amplify, possibly due to the private nature of such events, I investigated whether amplification may occur because the events investigated in field research are usually shared in nature. I conducted an analogue study requiring participants to experience a *shared* or unshared stressful experience for two reasons: a) to be consistent with previous shared amplification paradigms, and b) to maintain experimental control of trauma analogue exposure while also using a more ecologically valid stimulus where participants *experienced* the negative event, similar to field research. Participants completed the Cold Pressor Task (CPT; where participants immerse their hand in cold water, causing mild to moderate pain and stress) at the same time as a confederate (shared condition) or while the confederate completed another task (unshared condition). They then rated their emotional reactions to completing the task immediately after and again 24-hours later. I examined whether shared stressful experiences amplify. I also examined whether the mechanism for shared amplification may be as Smith and Mackie's (2015) model suggests: people may construct mental representations of others' responses or experiences (e.g., "this task is painful") that are then spontaneously incorporated into their own responses, thus increasing the stimuli's impact.

Study 7

My previous study confirmed that sharing a stressful experience can amplify that experience. Furthermore, my results suggest that the underlying mechanism of this shared amplification is that people construct and incorporate others' responses into their own responses (a process I will call perspective-taking here): the shared condition reported perspective-taking more than the shared condition and second and perspective-taking was positively correlated with many of my measures (e.g., pain and stress). Once again, these results suggest that current concerns (in this case, wondering how others are responding to a situation) seem to be the mechanism underlying amplification. Therefore, for my final study, I decided to manipulate perspective-taking to determine whether it leads to amplification. I conducted a study using the same paradigm as in Study 5 with three alterations: a) I added an extra condition where participants shared the CPT with the confederate, similar to the control shared condition, but were explicitly instructed to take the confederates perspective before the task, and b) I removed the rapport phase in order to determine if perspective-taking alone leads to amplification.

Chapter Three: Memory consistency for sexual assault events

Author Contributions: Sasha Quayum developed the study concept and design (90% contribution), collected data, performed the data analysis and interpretation (100% contribution) and drafted the manuscript (80% contribution to the writing and editing). Melanie Takarangi and Reginald Nixon provided guidance during the design phase and made critical revisions to the manuscript. All authors approved the final version of the manuscript for submission.

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Chapter Four: Is memory for sexual assault consistent over time?: a conceptual replication of Study 1[1]

Author Contributions: Sasha Quayum developed the study concept and design (85% contribution), collected data, performed the data analysis and interpretation (100% contribution) and drafted the manuscript (80% contribution to the writing and editing). Melanie Takarangi and Reginald Nixon provided guidance during the design phase and made critical revisions to the manuscript. All authors approved the final version of the manuscript for submission.

Abstract

Memory amplification studies have typically found that people remember or report experiencing more trauma events over time. Study 1 suggested source monitoring errors as a possible mechanism underlying amplification: post-event information (PEI) exposure for survivors (e.g., via media reports, police reporting, etc.) may become familiar and similar to original event memory details over time and be inadvertently incorporated into the original memory. Indeed, Study 1 did not find amplification with a sample of sexual assault survivors who were exposed to minimal post-event information due to the nature of the trauma. Importantly, participants who *were* exposed to media reports about their, or similar, experiences made more no-to-yes (amplification) errors. Amplification errors were also related to symptomatology. This follow-up study sought to replicate Study 1 with a different scale given that yes/no scales may not best reflect participants' true responses (e.g., some may always say "yes" if they're mostly sure they experienced an event). Accordingly, we asked participants, at two time-points with a 6-month delay, whether they had experienced certain sexual assault events in the last five years on a likelihood scale (1 = definitely did not

[1] Nahleen, S., Nixon, R. D. V., & Takarangi, M. K. T. (2018). *Is memory for sexual assault consistent over time: A conceptual replication of Study 1.* Unpublished manuscript.

happen; 8 = definitely did happen). Consistent with Study 1, participants reported experiencing fewer events over time. People who were not exposed to media reports about similar sexual assault experiences rated the likelihood that they experienced events lower over time, whereas people who *were* exposed to such reports remembered consistently with initial assessment. We also found that distress at follow-up and worsening symptomatology over time was related to amplification errors.

Introduction

Memory amplification studies have typically found that people remember or report experiencing more trauma events at follow-up compared to initial assessment. For example, Southwick, Morgan, Nicolaou, and Charney (1997) asked Desert Storm veterans, 1 month and 2 years after the war, whether they experienced 19 traumatic events during their service (e.g., seeing others killed or wounded). Seventy percent of participants made amplification or no-to-yes (NY) errors, saying 'no' to experiencing events at 1 month, to 'yes' at 2 years. This effect has been observed in many (mostly soldier and veteran) populations (see van Giezen et al., 2005). The mechanisms underlying the effect are still unclear, although a few studies have found a positive relationship between amplification and posttraumatic stress disorder (PTSD) symptoms. Engelhard, van den Hout, & McNally (2008) suggested a reappraisal mechanism: people who are experiencing more distress are likely to try and understand or justify it by attributing it to their trauma; that is, traumatic events initially considered minor (and, therefore, not reported) may be relabelled as significant and reported.

Study 1 suggested yet another possible mechanism underlying amplification: source monitoring errors (see also Oulton, Takarangi & Strange, 2016; Takarangi, Oulton, Green & Strange, 2015). Field amplification studies have usually investigated memory reports for large-scale events that were discussed and reported widely, potentially resulting in high post-event information (PEI) exposure for survivors (e.g., via media reports, police reporting,

etc.). PEI may become familiar and similar to original event memory details over time and be inadvertently incorporated into the original memory because people employ mental shortcuts such as familiarity to determine whether they experienced an event (Johnson, Hashtroudi, & Lindsay, 1993; Lindsay, 2008).

Therefore, our primary aim in Study 1 was to replicate field amplification research with a sample of sexual assault survivors who were likely exposed to minimal post-event information due to the nature of the trauma (e.g., 84.5% of survivors experience rape in private settings and only 23% of sexual assaults are reported; Bureau of Justice Statistics, 2016; National Institute of Justice, 2006). This method allowed us to determine whether amplification still occurs when there is little PEI exposure, or if the memory phenomenon is eliminated in those situations. We asked participants from the general population, at two time-points with a 6-month delay, whether they had experienced certain sexual assault events in the last five years. Importantly, participants reported experiencing fewer events at follow-up than initial assessment. In other words, their overall memory for sexual assault victimization did not amplify over time. Further, supporting our proposition that sexual assault survivors may be exposed to minimal post-event information, participants infrequently discussed, or were exposed to media reports about their trauma, possibly resulting in fewer source monitoring errors. Importantly, participants who *were* exposed to media reports about their, or similar, experiences made more no-to-yes (amplification) errors. Thus, these results overall suggest that amplification may be explained by source monitoring errors caused by possible PEI exposure.

However, one critical point about that study, and indeed many other field amplification studies, needs to be addressed. The use of a yes/no scale assumes that participants confidently recollect experiencing or not experiencing an event, perhaps a counterintuitive assumption given that the study specifically examined memory

inconsistency. A longstanding issue in the memory literature is whether recollection is a dichotomous or continuous detection process. Single-process models assume that recognition decisions reflect a singular, continuous process like familiarity (Egan, 1958), which refers to the feeling that the item was previously experienced without retrieval of contextual details. Many dual-process theories of recognition memory, which suggest that recognition decisions are based on recollection (the retrieval of contextual details associated with the item) *and* familiarity, assume that recollection is a dichotomous process, while familiarity is continuous. For example, Yonelinas' (1994) model suggests that when someone recollects successfully, they make an "old" decision confidently and accurately. When recollection fails, recognition decisions are based on a continuous familiarity process. An alternate model posits that both recollection and familiarity are continuous processes associated with varying degrees of confidence and accuracy (Wixted, 2007). Regardless of which model more accurately reflects the recognition process, both models suggest that some decisions on a recognition test may be based on continuous processes, and participants may need to total the evidence that they did or did not encounter an item before making a yes/no decision. It is possible that a yes/no response does not best reflect participants' true responses (e.g., some may always say "yes" if they're mostly sure they experienced an event, or "no" if they are remotely unsure, etc.). Indeed, there is evidence that yes/no reports are not always accurate; for example, studies show that people have a bias to say "yes" regardless of what the question is (e.g., Messick, & Jackson, 1961; Moum, 1988; Hinz, Michalski, Schwarz, & Herzberg, 2007). This phenomenon, called acquiescence, occurs for a number of reasons, including an inclination to satisfice (pursue the minimum requirements) when answering questionnaires (Krosnick, 1991).

In terms of how we should measure trauma recollection, Ost (2003; 2016), drawing on Bartlett's work, proposed that *belief*, rather than direct recollection, was important.

Bartlett (1932) argued that people make inferences about current circumstances based on what occurred in the past, that is, they believe something specific must have occurred to explain their current state. These beliefs guide reconstruction of the past, largely to justify current circumstances. This proposition is consistent with the reappraisal theory of amplification that suggests that people appraise trauma as worse at follow-up than at initial assessment to make sense of, or justify, current symptomatology (Engelhard et al., 2008).

Therefore, in this paper, we replicated Study 1 with an exposure scale that assesses people's beliefs that they likely experienced certain sexual assault events. Accordingly, we asked participants, at two time-points with a 6-month delay, whether they had experienced certain sexual assault events in the last five years on a likelihood, rather than yes/no, scale (1 = definitely did not happen; 8 = definitely did happen). All other measures remained the same (see Table 1).

Method

Participants

To be consistent with past research (e.g., Southwick et al., 1997 and Study 1) we decided to examine memory consistency for a traumatized sample. Therefore, we only gave follow-up questionnaires to participants who said "yes" to at least one item at initial assessment. Two hundred sixty female participants from Amazon Mechanical Turk—restricted to US workers with an approval rating of at least 95%—completed both parts of this study. We excluded 60 participants: 26 failed the instructional manipulation (i.e., attention) checks (Oppenheimer, Meyvis, & Davidenko, 2009), 22 claimed that their most recent unwanted sexual experience occurred more than five years and seven months (six month delay plus the one month we allowed to complete follow-up questionnaires), six restarted the surveys, five completed the follow-up assessment beyond the one-month deadline, and one was accidentally given the follow-up questionnaires a month early. Our

analyses were conducted for the remaining 200 participants (M_{age} = 34.23, *SD* = 9.78). Participants received US$3.00 for their time.

Measures

Trauma exposure scale. We used the same 19-item trauma exposure scale[2] (e.g., "A man has put his penis into my vagina without my consent"; see Table 2) as Nahleen et al. to assess participants' unwanted sexual experiences in the past 5 years (initial assessment) or 5.5 years (follow-up after the six-month delay; we asked when the most recent event occurred), including an "other" option, allowing participants to add unlisted events. However, instead of yes/no responses, participants were required to rate each item on the likelihood the event occurred (1 = definitely did not happen; 8 = definitely did happen; Garry, Manning, Loftus, Elizabeth, & Sherman, 1996). Two independent coders coded whether the "other" event: clearly occurred before the participant was 18, did not mention a specific event or only mentioned the context in which the event likely occurred ("gang rape"), was not considered a Criterion A stressor for sexual assault ("pictures of penises in text messages") , or repeated an event that they had already said "yes" to on the exposure scale list. These responses were removed (changed from "yes" to "no") if both coders agreed.[3] We removed 17 "other" responses at initial assessment and 9 at follow-up. At the end of the follow-up session, participants were asked whether each event they rated over 1 on the scale for "actually occurred in the six-month delay (yes/no). The corresponding responses on the trauma exposure scale were then changed to 1."[4]

[2] We measured mood to see if there were mood differences between the two time-points. There was no difference in positive (p = .065) or negative mood (p = .764) from T1 to T2.
[3] If people reported that Item 19 ("other") bothered them the most on the PCL-5, we removed their PCL-5 and PTCI responses (seven at T1 and three at T2).
[4] Participants were also asked if each unwanted sexual experience had occurred in specific contexts (e.g., if they were drinking). At the end of T2, they were also given the Reactions to Research Participation Questionnaire-Revised questionnaire. These data are not reported here.

Post-event information questions. We asked participants if, and how often (free recall response), they had (a) reported or discussed any of the sexual experiences (e.g., in court, with friends/family, etc.), (b) seen media reports specifically relating to their unwanted sexual experiences, and (c) seen media reports about events similar to their unwanted sexual experiences.[5] At T2, they were asked the same questions in regard to the last six months.

Depression Anxiety Stress Scales (DASS-21). The 21-item DASS (Lovibond & Lovibond, 1995) measured depression (DASS-D), anxiety (DASS-A), and stress (DASS-S) symptoms at the start of both T1 and T2. Participants rated each item (e.g., "I felt I was close to panic") on a scale of frequency or severity of the symptom in the last week (e.g., 0 = did not apply to me at all; 3 = applied to me very much, or most of the time; range: 0-63). The DASS-21 has high internal consistency (α = .87-.94 across scales). The DASS-21 subscales correlate strongly with other measures (e.g., Beck Depression Inventory (Beck, Rush, Shaw, & Emery), Beck Anxiety Inventory (Beck & Steer, 1990), the State-Trait Anxiety Inventory-Trait Scale (Spielberger, 1983; Antony, Bieling, Cox, Enns, & Swinson, 1998), etc.).

PTSD Checklist (PCL-5). At both T1 and T2, we used the 20-item PCL-5 to assess participants' trauma symptoms (e.g., "feeling jumpy or easily startled?") experienced in the last month in relation to the unwanted sexual experience that bothered them the most (1 = not at all; 5 = extremely; Weathers, Litz, Keane, Palmieri, Marx, & Schnurr, 2013). Higher scores indicate more trauma symptoms (range: 0-80). PCL-5 scores exhibited strong internal consistency (α = .94), test-retest reliability (r = .82), and convergent validity with other PTSD measures (e.g., Posttraumatic Distress Scale: r = .85; Blevins, Weathers, Davis, Witte, & Domino, 2015).

[5] We also asked which items they had reported/discussed and who they reported/discussed the item with (police, friends, partner, counsellor/therapist, parents/direct family, online, co-victims, other). These data are not reported here.

Posttraumatic Cognitions Inventory (PTCI). We used the 33-item PTCI to assess participants' Self-Blame, Negative Cognitions About Self, and Negative Cognitions About the World after their unwanted sexual experiences (Foa, Ehlers, Clark, Tolin, & Orsillo, 1999). Participants rated how much they agree with each item (e.g., "I am a weak person";1 = totally disagree; 7 = totally agree). The PTCI has strong internal consistency (α = .86-.97), test-retest reliability (r_s = .74-.85), and convergent validity with the Personal Beliefs and Reactions Scale (Resick et al., 1991) scales (r_s = .72-.74; Foa et al., 1999).[6]

Procedure

All procedures were approved by the Social and Behavioural Research Ethics Committee at Flinders University. Following informed consent procedures, participants completed the PANAS, DASS-21, sexual assault exposure scale, and the post-event information, and distress (PTSD and cognitions regarding trauma) questions. After a 6-month delay, participants responded to the same questionnaires again. Dataset is available at https://osf.io/5db4n/.

Results

Memory inconsistency

The primary question in this study is whether people report experiencing more sexual assault events over time. We examined memory amplification in three ways: no-yes errors and belief change scores.

No-to-Yes errors. In order to compare our findings to Study 1, we first converted exposure scale ratings to No-Yes (NY; not endorsed at initial assessment but endorsed at follow-up), YN (endorsed at initial assessment but not at follow-up), YY (endorsed at both times), and NN (never endorsed) responses. To do so, we coded participants' 1-4 ratings of

[6] PCL-5 and PTCI responses were also removed for two people at T2 because they later admitted that the event that bothered them the most only occurred within the six-month delay.

whether events had occurred on the trauma exposure scale (1 = definitely did not happen), at both times, as 'no' and ratings from 5-8 (8 = definitely did happen) as 'yes' responses. This coding allowed us to examine whether people moved broadly from no-to-yes on the scale (see Table 2). Interestingly, participants appeared to have made fewer YN errors than in Study 1, suggesting that their memory may have been more consistent over time: 69.5% of participants (vs. 84.1% in Study) 1 reported at least one YN change, and 46.5% (vs. 58.8%) reported two or more. However, they seemed to have made a similar number of amplification or NY errors: 49.0% (vs. 56.2%) reported at least one NY change, and 31.5% (vs. 34.5%) reported two or more. A within-subjects t-test revealed that at T2, participants reported experiencing fewer events (M = 3.81; SD = 3.90) compared to what they reported at T1, M = 4.66, SD = 3.47; $t(199) = 3.37$, $p = .001$. This overall pattern is consistent with Study 1.

Table 1

Mean DASS, PCL-5, PTCI scores across participants at both assessment times (standard deviations in brackets)

	Time 1	Time 2
DASS		
Depression subscale	12.19 (10.47)	12.68 (10.22)
Anxiety subscale	9.88 (8.89)	9.70 (8.80)
Stress subscale	14.75 (8.93)	15.28 (8.65)
PCL-5		
Total	25.34 (19.00)	25.26 (17.95)
Intrusion subscale	5.49 (4.89)	5.41 (4.57)
Avoidance subscale	3.69 (2.63)	3.29 (2.51)
Negative alterations in cognitions and mood subscale	8.66 (7.19)	9.03 (7.07)
Alterations in arousal and reactivity	7.50 (6.46)	7.53 (6.01)
PTCI		
Total (summed score)	100.63 (42.36)	106.61 (42.71)
Negative cognitions about self subscale (average score)	2.86 (1.30)	3.03 (1.32)
Negative cognitions about world subscale (average score)	3.90 (1.45)	4.14 (1.44)
Self-blame subscale (average score)	2.75 (1.33)	2.94 (1.36)

Table 2

Change in endorsement of exposure variables (% of participants) and mean change scores (SD's in brackets)

Sexual assault experience	No-No	Yes-No	Yes-Yes	No-Yes	Change Score
A man put his penis into my vagina without my consent	65.0	11.0	16.5	7.5	-.13 (2.64)
Someone put their mouth or tongue on/in my vagina without consent	74.5	7.5	7.5	10.5	.13 (2.70)
Someone put their mouth or tongue on/in my anus without consent	90.0	3.5	0.5	6.0	.23 (2.03)
Someone made me put my mouth or tongue on/in their vagina without consent	91.0	4.0	1.5	3.5	-.05 (1.77)
Someone made me put my mouth or tongue on/in their anus without consent	95.0	1.0	0.5	3.5	.13 (1.43)
A man put his penis into my anus without consent	83.5	5.5	6.0	5.0	-.11 (2.18)
Someone inserted a foreign object into my vagina without consent	91.5	2.0	3.0	3.5	.07 (1.44)
Someone inserted a foreign object into my anus without consent	95.5	1.0	0.5	3.0	.08 (1.24)
Someone put their fingers into my vagina without consent	56.0	14.5	19.5	10.0	-.29 (3.18)
Someone put their fingers into my anus without consent	82.0	4.5	6.5	7.0	.06 (2.21)
Someone forced kisses on me without consent	37.0	25.0	29.5	8.5	-1.16 (3.40)
Someone touched my private parts without consent	27.0	26.0	35.5	11.5	-.96 (3.69)
Someone removed some or all of my clothing without consent	56.5	14.0	18.0	11.5	-.19 (3.16)
Someone grabbed or fondled me in a sexual way without consent	25.5	31.5	35.5	7.5	-1.58 (3.61)
Someone rubbed against me in a sexual way without consent	30.0	27.0	30.5	12.5	-1.03 (3.77)
Someone has TRIED to make me take part in any of the sexual acts above	50.0	23.0	17.5	9.5	-.86 (3.40)
I have woken with a sore vagina/anus, and had little/no memory of what happened	80.5	6.5	8.5	4.5	-.13 (2.00)
Someone masturbated in front of me without consent	72.5	9.0	9.0	9.5	-.08 (2.70)
Any other types of unwanted or uninvited sexual contact	96.0	3.5	0.0	0.5	-.17 (1.22)

Note. No-No = not endorsed at either time-point, Yes-No = endorsed at initial assessment but not follow-up (minimizing or forgetting), Yes-Yes = endorsed at both time-points, No-Yes = not endorsed at initial assessment but endorsed at follow-up (amplification error).

Belief change scores. To examine belief ratings more precisely (as opposed to the broader no-yes categorization), we examined point change in participants' ratings that the event likely did or did not occur. First, we averaged participants' ratings from 1-8 across events separately at both T1 and T2. Only 6.0% had no change in memory over time, that is they had a change score (average ratings at T2 – average ratings at T1) of zero. The majority of participants (62.0%) had a negative change score, rating the likelihood of experiencing events lower over time, suggesting that most people minimized or forgot events. The remainder—32.0% of participants—had a positive change score, that is, they amplified over time. Further, a paired samples t-test comparing total ratings at both times found that the majority of participants minimized or forgot events over time, T1: $M = 2.71$, $SD = 1.20$; T2: $M = 2.40$; $SD = 1.31$; $t(199) = 3.66$, $p < .001$. This result suggests that participants believed that they were less likely to have experienced the sexual assault events at follow-up compared to initial assessment.

Amplification errors and PEI

We then examined the relationship between amplification errors and PEI exposure. As expected, participants rarely discussed their sexual assault (47.0% at T1 and 26.1% at T2 (of those who reported experiencing sexual assault) reported discussing their experiences), and were rarely exposed to media reports regarding to their own sexual assault (17.1% at T1, 21.7% at T2 reported being exposed). They were sometimes exposed to media reports about similar sexual assault experiences (67.0% at T1, 61.4% at T2 reported being). Because rates were so low overall, we dichotomized PEI responses into yes/no responses.

In Study 1, we found that people who did *not* report the event during the six-month delay (measured at follow-up) unexpectedly experienced comparatively more NY errors. Here there was no difference on NY errors or change scores between those claimed to have reported/discussed their experiences and those who did not, at either time-point, *p*s = .069-

.670. Compared to Study 1 where we found that participants who were exposed (vs. not exposed) to media reports about their experiences made more NY errors, here we found no difference at either initial assessment or follow-up, *p*s = .164-.885. Unlike in Study 1 where participants who reported being exposed (vs. not exposed) to media reports about *similar* experiences at initial assessment made more NY errors, here there was no difference on NY errors at either time point, or a difference in change score at initial assessment (*p*s = .112-.994). However, there was a difference in change score at for participants who were exposed to media reports on similar experiences (vs. not) at follow-up ($p = .027$). One-sample t-tests showed that people who were not exposed had a change score significantly lower than zero, rating the likelihood of experiencing events lower over time ($p = .010$), whereas people who were exposed did not ($p = .978$).

Amplification errors and symptoms

We then examined the relationship between amplification errors and symptomatology. First, we divided participants into PTSD-positive and PTSD-negative groups using a cut-off score of 31 (Ashbaugh et al., 2016; Bovin et al., 2016). There were no differences between PTSD-positive and negative groups at initial assessment for NY errors or average belief change scores, $p = .113$-$.184$. At follow-up, however, consistent with Study 1, the PTSD-positive group ($N = 59$; $M = 2.37$, $SD = 2.39$) made significantly more NY errors than the PTSD-negative group, $N = 100$; $M = 1.30$, $SD = 1.87$; $t(99.64) = -2.96$, $p = .004$. The PTSD-positive (vs. negative) group also had higher change scores across events, PTSD-positive: $M = .14$, $SD = 1.17$; PTSD-negative: $M = -.24$, $SD = 1.17$; $t(157) = -2.01$, $p = .047$. One sample t-tests showed that the mean change score for PTSD-negative participants was significantly different from zero ($p = .041$), suggesting they minimized or forgot events over time.

However, the mean change score for PTSD-positive participants was not different from zero (p = .353), suggesting their exposure reports were consistent over time.[7]

Table 3

Correlations (ps in brackets) between memory errors and T1 and T2 distress measures

	Number of YN errors	Number of NY errors	Average Change Score
T1 DASS (depression, anxiety, stress)			
Depression	.05 (.467)	.07 (.341)	.01 (.878)
Anxiety	.08 (.277)	.08 (.298)	.01 (.932)
Stress	.09 (.227)	.01 (.914)	-.06 (.417)
T1 PTCI (trauma-related cognitions)			
Total scores	.20 (.004)**	.12 (.088)	-.07 (.340)
Negative cognitions about the self	.20 (.005)**	.10 (.148)	-.08 (.257)
Negative cognitions about the world	.19 (.009)**	.14 (.050)	-.03 (.699)
Self-blame	.19 (.008)**	.12 (.085)	-.06 (.384)
T2 DASS (depression, anxiety, stress)			
Depression	-.01 (.883)	.16 (.024)*	.09 (.192)
Anxiety	.06 (.436)	.10 (.151)	.02 (.750)
Stress	.07 (.361)	.12 (.103)	.01 (.921)
T2 PTCI (trauma-related cognitions)			
Total scores	.08 (.330)	.19 (.016)*	.04 (.598)
Negative cognitions about the self	.08 (.340)	.17 (.038)*	.02 (.782)
Negative cognitions about the world	.01 (.862)	.21 (.008)**	.10 (.202)
Self-blame	.12 (.145)	.23 (.004)**	.04 (.610)

To explore whether other psychological and trauma-related symptoms might also be related to amplification, we correlated errors with negative appraisals regarding the trauma (PTCI), anxiety, depression and stress (DASS-21) at each assessment. Initial assessment

[7] We also classified participants into probable PTSD-positive and PTSD-negative status by treating each item rated as 2 or above on the PCL-5 as a symptom endorsed, and then following the DSM-5 diagnostic criteria for PTSD which requires at least: 1 Criterion B item, 1 Criterion C item, 2 Criterion D items, and 2 Criterion E items. We found the same pattern of results except that people who were PTSD-positive at initial assessment (N = 73; M = -.59, SD = 1.30) had a significantly lower change score compared to those who were PTSD-negative, N = 123; M = -.15, SD = 1.16; $t(194) = 2.46$, p = .015. One samples t-tests show that the PTSD-negative group's change score was not significantly different from zero (p = .146), while the PTSD-positive's score was ($p < .001$).

measures did not correlate with any error types (see Table 3), unlike in Study 1 where NY errors were correlated with stress and anxiety at initial assessment. NY errors were related to more follow-up trauma-related cognitions (unlike in Study 1), and depression, but not anxiety and stress as well like in Study 1. Other correlations were non-significant (see Table 3). NY errors and total change scores were also positively related to *worsening* PTSD symptoms, and worsening trauma-related thoughts; in Study 1 NY errors only related to worsening stress. Other correlations were non-significant (see Table 4). Overall, our results suggest that memory amplification is indeed related to current (follow-up) distress.

Table 4

Correlations (ps in brackets) between memory errors and worsening distress over time

	Number of YN errors	Number of NY errors	Average Change Score
DASS (depression, anxiety, stress)			
Depression	-.08 (.261)	.11 (.107)	.10 (.149)
Anxiety	-.03 (.658)	.04 (.594)	.02 (.746)
Stress	-.03 (.683)	.13 (.061)	.08 (.248)
PCL (PTSD)			
Total scores	-.31 (<.001)***	.28 (<.001)***	.34 (<.001)***
Intrusions subscale	-.23 (.004)**	.16 (.050)	.26 (.001)**
Avoidance	-.18 (.022)*	.27 (.001)**	.25 (.002)**
Negative cognitions and mood	-.28 (<.001)***	.28 (<.001)***	.32 (<.001)***
Arousal and reactivity	-.26 (.001)**	.18 (.023)*	.24 (.002)**
PTCI (trauma-related cognitions)			
Total scores	-.21 (.008)**	.19 (.020)*	.24 (.002)**
Negative cognitions about the self	-.22 (.006)**	.18 (.025)*	.25 (.002)**
Negative cognitions about the world	-.22 (.006)**	.18 (.029)*	.22 (.005)**
Self-blame	-.14 (.089)	.19 (.019)*	.20 (.013)*

Forgetting experiences over time

We analyzed whether not being exposed to PEI is related to forgetting or underreporting events over time (YN errors). There was no difference in YN errors between those who discussed their experiences versus those who did not (p = .076-.436), and those

who were exposed to media reports about their experiences (p = .223-.726), or similar experiences (p = .102-.202) versus those who were not.

We next looked at whether forgetting events over time would be related to adjustment or less distress over time rather than current distress. Indeed, similar to Study 1, those who were PTSD-positive at T1 made more YN errors (N = 76, M = 2.95, SD = 2.93) than those who were PTSD-negative, N = 124, M = 1.74, SD = 2.06; t(120.73) = -3.14, p = .002. However, unlike in Study 1 where we found no difference, those who were PTSD-positive at T2 made *fewer* YN errors (N = 59, M = 1.73, SD = 1.89) than those who were PTSD-negative, N = 141, M = 2.40, SD = 2.69; t(152.19) = 2.00, p = .047. Further, YN errors were positively correlated with higher PTCI (but not DASS-21) scores at T1 but not at T2 unlike in Study 1 where YN errors correlated at both times (Table 3). Also consistent with our hypothesis, number of YN errors was correlated with *less* distress over time (Table 4).

Discussion

Consistent with Study 1, we did not find an overall memory *amplification* effect: participants rated the likelihood of experiencing events lower at follow-up compared to initial assessment. The "de-amplification" finding across both studies suggest that, unlike some other trauma events, it is possible that sexual assault memories do not amplify over time. One explanation for this discrepancy is that victims in other amplification research may have been exposed to new information about their experiences through (specifically, based on our findings) media reports due to the large-scale nature of the event. This PEI can be incorporated into memory, resulting in source monitoring errors. Sexual assault survivors may be less likely to be exposed to PEI because of the often private nature of the trauma, resulting in few amplification errors. Interestingly, we found that people who had not been exposed to media reports of similar sexual assault experiences underreported over time, whereas those who had been exposed did not, further suggesting that the lack of any exposure

to PEI is related to underreporting of experiences. It is plausible that discussion and exposure to media reports on their own experiences were not related to amplification errors because those reports did not contain new or inaccurate information.

There is another potential explanation for this de-amplification. As discussed above, participants' true responses may not be best captured with yes/no scales because of acquiescence biases and the forced nature of the scale which may lead some people to always say "yes" or "no" if they believed they likely did or did not experience an event respectively. Further, in Study 1, we wondered whether amplification has been quite consistently found in past studies because the possibility of experiencing additional thematically related trauma events (e.g., sniper fire) *within* a larger trauma context (e.g., military deployment) may have seemed plausible or believable to participants, increasing their belief that certain events occurred (Mazzoni, Loftus, & Kirsch, 2001). On the other hand, additional sexual assault events, which often do not occur in a larger context, may seem less plausible, making participants less likely to believe they experienced those events and therefore amplify their experience over time. Our findings are consistent with this possibility.

However, despite finding *overall* de-amplification, we also found that people still made amplification errors. Consistent with past amplification research, including Study 1, we found that distress at follow-up and worsening symptomatology over time was related to these amplification errors. Therefore, amplification may be the result of a reappraisal process: people who are more distressed by their sexual assault experiences may try to understand their current symptoms by relabelling those experiences as significant over time (see Engelhard et al., 2008). Importantly, forgetting or underreporting of traumatic experiences over time was related to *less* distress at follow-up, further suggesting that people may unintentionally draw on current appraisals of, and reactions to, their trauma to help reconstruct what they experienced. It is possible that forgetting or appraising experiences as

less important over time may be an adaptive adjustment mechanism that helps people regulate negative emotion and distress or vice versa.

Our study has several limitations. First, it is impossible to determine causality with our design, making future experimental (e.g., analogue) research necessary. However, analogue trauma is not personally experienced or as highly arousing as real trauma. Second, items on our exposure scale may have been ambiguous and subjective and, as such, vulnerable to reinterpretation (see Engelhard et al., 2015; Takarangi, Oulton, & Strange, 2017). Therefore, report inconsistency may reflect this reinterpretation rather than memory errors. Third, we did not find completely consistent findings between this study and Study 1. Future research should systematically investigate these differences. Last, researchers could ask participants to provide detailed descriptions of their trauma to determine the types of events and aspects of memory that change over time. Despite these limitations, our findings have important implications, suggesting that some trauma reports may not always be a reliable indicator of all aspects of actual trauma exposure.

Supplementary Data

At Time 1, participants were asked the following questions:

1. Who did you report to or discuss the experience(s) with?

    - 35.5% discussed their sexual assault experience(s) with friends
    - 29.5% discussed with their partner
    - 11.0% discussed with their counsellor/therapist
    - 13.5% discussed with their parents/direct family
    - 6.5% discussed with/reported to police
    - 6.0% discussed online
    - 2.0% discussed with other victims who had been present during the experience
    - 1.5% said "other"

2. If you have seen or heard any media reports specifically relating to your sexual experience(s) please say where by clicking on the relevant response(s) below. Please only click "N/A" if you have not seen/heard any media reports about events specifically relating to your experience(s).

    - 84.0% said N/A
    - 10.5% on the internet/online
    - 10.5% on television
    - 3.5% in the newspaper
    - 1.5% said "other"

3. If you have seen or heard any media reports about events similar to your experience(s) please say where by clicking on the relevant response(s) below. Please only click "N/A" if you have not seen/heard any media reports about events similar to your experience(s).

    - 38.0% said N/A
    - 49.5% on the internet/online
    - 41.0% on television
    - 11.0% in the newspaper
    - 2.5% said "other"

At Time 2, participants were asked the following questions:

1. Who did you report to or discuss the experience(s) with in the last 6 months?

    - 14.0% discussed their sexual assault experience(s) with friends
    - 9.0% discussed with their partner
    - 3.5% discussed with their counsellor/therapist
    - 2.5% discussed with their parents/direct family
    - 1.0% discussed with/reported to police
    - 1.5% discussed online
    - 0.0% discussed with other victims who had been present during the experience
    - 0.0% said "other"

2. If you have seen or heard any media reports specifically relating to your sexual experience(s) in the last 6 months, please say where by clicking on the relevant response(s) below. Please only click "N/A" if you have not seen/heard any media reports about events specifically relating to your experience(s).

    - 66.5% said N/A
    - 10.5% on the internet/online
    - 10.5% on television
    - 1.5% in the newspaper
    - 0.5% said "other"

3. If you have seen or heard any media reports about events similar to your experience(s) in the last 6 months, please say where by clicking on the relevant response(s) below. Please only click "N/A" if you have not seen/heard any media reports about events similar to your experience(s).

    - 38.5% said N/A
    - 35.5% on the internet/online
    - 28.5% on television
    - 5.5% in the newspaper
    - 1.0% said "other"

Chapter Five: Current Posttraumatic Stress Disorder symptomatology distorts memory for past symptoms[8]

Author Contributions: Sasha Quayum developed the study concept and design (85% contribution), collected data, performed the data analysis and interpretation (100% contribution) and drafted the manuscript (80% contribution to the writing and editing). Melanie Takarangi and Reginald Nixon provided guidance during the design phase and made critical revisions to the manuscript. All authors approved the final version of the manuscript for submission.

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[8] Nahleen, S., Nixon, R. D. V., & Takarangi, M. K. T. (2018). *Current Posttraumatic Stress Disorder symptomatology distorts memory for past symptoms.* Manuscript submitted for publication.

Chapter Six: Exposure to repeated misinformation does not increase memory distortion after a traumatic event[9]

Author Contributions: Sasha Quayum developed the study concept and design (85% contribution), collected data, performed the data analysis and interpretation (100% contribution) and drafted the manuscript (70% contribution to the writing and editing). Melanie Takarangi and Deryn Strange provided guidance during the design phase and made critical revisions to the manuscript. All authors approved the final version of the manuscript for submission.

Abstract

Misinformation can distort trauma memory. Importantly, however, no empirical studies have investigated the effect of *repeated* misinformation on trauma memory. Addressing this gap is important because witnesses and trauma victims are often repeatedly exposed to the same misinformation. In three experiments, participants viewed a traumatic film with some scenes removed. Twenty-four hours later, they were given three "eyewitness" reports describing the film's events. To manipulate misinformation repetition, either zero, one, or all three of the reports described removed scenes. Participants exposed to misinformation falsely remembered more removed scenes compared to participants who were not exposed to misinformation. Interestingly, however, repeated misinformation exposure did *not* lead to higher error rates compared to single exposure. There may be a simple explanation for this null finding: participants who were exposed to the misinformation once made more errors than those who were not exposed at all, but fewer than those who were exposed repeatedly. Therefore, the repeated vs. single misinformation effect may exist but be very small as suggested by our effect sizes.

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[9] Nahleen, S., Strange, D., & Takarangi, M. K. T. (2018). *Exposure to repeated misinformation does not increase memory distortion after a traumatic event.* Manuscript submitted for publication.

Introduction

A wealth of research demonstrates that misinformation exposure can distort eyewitness memory (Loftus, 2005). The source monitoring framework (SMF; Johnson, Hashtroudi, & Lindsay, 1993) explains that when people are asked about misinformation, they likely imagine that misinformation while retrieving the original event. As the misinformation images become more detailed and sensory, they become more familiar and similar to memories of the original event. The SMF posits that because we employ heuristics (e.g., familiarity) to determine whether we experienced an event, we inadvertently incorporate misinformation into our original memory.

In reality, people are likely exposed to the same misinformation repeatedly. For example, after witnessing a car accident involving a red car, Witness A may wrongly inform Witness B that the car was pink. The police, working from this inaccurate information, may also suggest to Witness B that the car was pink. Witness B may also read media reports reporting the car as pink. Only a few studies have systematically examined the effect of such repeated misinformation exposure on memory (Foster, Huthwaite, Yesberg, Garry, & Loftus, 2012; Mitchell & Zaragoza, 1996; Zaragoza & Mitchell, 1996). In Zaragoza and Mitchell (1996), participants watched a home burglary video and answered questions, some embedded with misleading suggestions (e.g., the thief wore gloves). These suggestions were given zero, one, or three times in different questions. Repeated suggestion increased false memories for seeing suggested details in the video. The researchers argued that with repetition, images of suggested details become more detailed and similar to the original event memory. Therefore, repetition decreases people's ability to monitor the misinformation source and, thus, increases the likelihood that people will incorporate the misinformation into their original event memory.

Importantly, we found no empirical studies investigating the effect of repeated misinformation for *traumatic* events. Addressing this gap is important because there are additional opportunities for repeated exposure to the same piece of misinformation among trauma victims—for example during medical treatment and therapy—as well as exposure to more 'typical' sources via co-witness discussion, police interviews, when reading media reports, and so on. If Zaragoza and Mitchell's (1996) findings extend to trauma memories, repeated misinformation exposure could have deleterious consequences on the accuracy of trauma victims' testimony and perhaps mental health.

Trauma memories, like other memories, can be distorted. Specifically, people tend to remember *more* trauma than they actually witnessed or experienced (e.g., Southwick, Morgan, Nicolaou, & Charney, 1997; Strange & Takarangi, 2012). For example, Southwick et al. (1997) asked Desert Storm veterans 1 month and 2 years after the war whether certain traumatic events (e.g., seeing others killed or wounded) occurred during their service. Most subjects were inconsistent, with 70% of participants reporting 'yes' to events at 2 years that they had not reported at 1 month. This amplification of trauma memories has been observed with a variety of populations (see van Giezen, Arensman, Spinhoven, & Wolters, 2005). However, when we examine the magnitude of memory distortion for trauma after misinformation exposure, the literature presents a somewhat murky picture. For example, Monds, Paterson, Kemp and Bryant (2013) found that participants exposed to a trauma film falsely remembered 29% of misinformation (e.g., that the event occurred on Thursday when it actually occurred on Tuesday) at free recall, suggesting the traumatic nature of the film did not protect their memory from misinformation. However, participants who saw the trauma film recalled *less* misinformation than participants exposed to a neutral film (52%), contradictory to the paradoxical negative emotion (PNE) hypothesis that the researchers aimed to test. The PNE hypothesis suggests memory for negative events should be accurate

to help people avoid such events in the future, but also flexible to include information on others' negative experiences, thus increasing accuracy and memory distortion simultaneously (Porter, Taylor, & ten Brinke, 2008). In a subsequent study, Monds, Paterson and Kemp (2016) gave participants a free recall *and* recognition test. No differences emerged on the free recall test, but participants in the trauma film condition *recognized* significantly more misinformation items (M = 5.34, SD = 1.68) compared to the neutral film condition (M = 4.48, SD = 1.58). Additionally, Porter, Bellhouse, McDougall, ten Brinke, and Wilson (2010) embedded misinformation in open-ended questions (e.g., "what was the gender of the person holding the bike?") for a highly negative photo and a highly positive photo. Participants falsely remembered a major misleading detail for the negative photo (i.e., they were less accurate; M = .82, SD = .31) more often than the positive photo (M = .92, SD = .20), suggesting negative emotion increases suggestion susceptibility. Therefore, paradigm differences (e.g., test type and comparison stimuli) may account for some of the inconsistent findings.

It is important to note that, regardless of whether the original event for which memory was tested was traumatic or neutral, both Monds et al.'s (2013, 2016) and Porter et al.'s (2010) misinformation and test *items* were relatively non-traumatic. Although trauma events may comprise both emotional and unemotional elements, when people rehearse trauma events—which we know they typically do extensively, both unintentionally via intrusions and intentionally (Schacter, 2001; Ehlers & Clark, 2000)—that rehearsal tends to focus on the emotional elements. For example, a car accident victim may frequently experience intrusive images of headlights approaching and voluntarily discuss those intrusions with his or her therapist (see Ehlers & Clark, 2000). Further, it is possible that contextual details (e.g., spatial location or thoughts and feelings closely linked to the emotional meaning of the event)) of a negative emotional event are enhanced while memory for other, more extrinsic, details may

not (e.g., background details or more tangential thoughts and feelings; see Kensinger, 2007 for review). Therefore, people may be more likely to attend to, rehearse, and incorporate *emotional misinformation* (vs. unemotional misinformation) into their original event memory. To test this hypothesis, we included both traumatic and non-traumatic misinformation items in our study.

To summarize, trauma memories amplify and are susceptible to misinformation, but it is unclear whether certain aspects of trauma memories are more susceptible than others. Indeed, research on how *repeated* misinformation affects trauma memory is lacking. To address this research gap, we adapted Strange and Takarangi's (2012) trauma film paradigm. We chose this paradigm for three reasons: a) it allows us to investigate whether misinformation can lead people to remember *more* trauma—indeed, entire parts of an event, rather than small details—than they actually saw, similar to findings from field research (e.g., Southwick et al., 1997); b) film content depicting actual or perceived threat and serious injury—events listed as traumatic in the *DSM-5* (American Psychiatric Association, 2013)—has been found to elicit responses analogous to symptoms experienced after actual trauma e.g., intrusions, physiological arousal, negative cognitions and mood (see James, Lau-Zhu, Clark, Visser, Hagenaars & Holmes, 2016) and, c) the trauma film paradigm is a common and accepted method of investigating trauma: as of 2016, 74 peer-reviewed articles (with a total of 87 experiments) have used traumatic or negative film stimuli within an experimental or prospective study design (James et al., 2016). Participants watched a trauma (car accident) film with some scenes removed. After 24 hours, participants read three "eyewitness" reports describing the film's events with some containing descriptions of removed scenes (see Foster et al., 2012). Participants then received a memory test containing Old (i.e., previously seen), Missing (removed), and Control clips. To examine whether people more frequently rehearse and incorporate emotional (vs. unemotional) misinformation into their original event

memory, removed Missing clips were divided into crux clips (scenes crucial to the film's meaning e.g., cars colliding) and non-crux clips (less crucial, more peripheral scenes e.g., a rescue helicopter arriving). Importantly, pilot testing in Strange and Takarangi (2012) found that participants' ratings of cruciality were highly correlated with how traumatic they found the clip (r=.95, p<.01).

Experiment 1

Method

Participants. Participants were recruited from Amazon Mechanical Turk. Strange and Takarangi (2015), using the same film stimuli, found that participants exposed to Missing clip descriptions—similar to our Single Misinformation condition—during encoding, exhibited increased memory distortion compared to those not exposed to the descriptions (d = .45). Given that our misinformation would be repeated and delivered in a separate phase (allowing memory to fade), we expected to find a larger difference in memory distortion between the Repeated Misinformation and No Misinformation condition. Our a priori power analysis for a two-tailed, two-group t-test (using G*Power) with an alpha of .05, power of .95, and effect size of .80 revealed that the desired sample size for our study was at least 42 per group.

Of the 149 participants who met our inclusion criteria,[10] 82 were female and aged 20 to 72 (M = 40.52, SD = 12.59). There were no significant differences in age and gender

[10] Four hundred and twelve participants completed the study. We excluded 263 participants: 15 said that they had seen the trauma film before, 13 repeated Time 2 and, therefore, potentially read reports more than once, 9 failed to accurately complete instructional manipulation checks (Oppenheimer, Meyvis, & Davidenko, 2009), 34 admitted to watching the test clips more than once, 7 admitted to leaving the study session, 7 completed Session 2 beyond the 36 hour deadline (we kept one person who completed it 50 minutes early), and 178 read at least one report faster than 318 words per minute. In a pilot study, participants read each report in full. However, reading time data revealed that many participants read the reports unrealistically fast, indicating lack of exposure to the manipulation. We found no misinformation effects in our pilot until we excluded those who only skimmed the misinformation, at which point we found that participants who were exposed to misinformation repeatedly falsely remembered more trauma stimuli compared to participants who were not exposed to misinformation at all. According to Trauzettel-Klosinski and Dietz (2012), average reading time for online English text is 228 words per minute, plus or minus 30 words. We therefore chose 318 words per minute (i.e., 228 words plus three standard deviations) as our cut-off because it would account for 99.7% of the reading

between groups (*p*s = .904-930). Participants completed Time 2 *M* = 29.37 hours (*SD* = 6.71) after Part 1.

Materials. The following materials were used in Experiment 1.

Positive Affect Negative Affect Schedule (PANAS). Subjects rated how they felt before and after the trauma stimuli (1=very slightly or not at all; 5=extremely) on 10 positive (e.g., excited, enthusiastic) and 10 negative (e.g., distressed, upset) mood adjectives. The scales have excellent convergent correlations with other mood measures (.76 to .92; Watson & Tellegen, 1988). The Negative Affect (NA) and Positive Affect (PA) subscales correlate with other measures of distress and psychopathology, including the Hopkins Symptom Checklist (NA: *r* = .56; PA: *r* = −.29) and Beck Depression Inventory (NA: *r* = .74; PA: *r* = −.36).

Trauma analogue. The traumatic stimulus was the same film used in Strange and Takarangi's (2012, 2015; see also Segovia, Strange & Takarangi, 2016) trauma analogue paradigm. The film was a United Kingdom public service announcement warning against texting while driving. Briefly, a teenage driver, while looking at her phone, collides with another vehicle head-on. Another car then crashes into them. Emergency services deal with the situation while the driver screams in distress at her injuries and upon noticing the dead passengers. The injuries and fatalities are graphically depicted (e.g., passenger's neck snaps, dead baby). Participants in Strange and Takarangi (2012) rated crux clips as not pleasant and moderately traumatic, indicating it is an appropriate negative, traumatic analogue. The film was cut into clips (separated by 2 seconds of blank screen) with 6 clips removed before encoding.

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speed data in a normal distribution. Such large numbers of attention check failures appear to be standard based on other studies we have conducted. Further, Amazon Mechanical Turk workers are more attentive to attention instructions than undergraduate subjects, suggesting that the failure rate could have been even higher if the expe\riments had been conducted in the laboratory rather than online (Hauser & Schwarz, 2016).

Post-event information. Three research assistants acting as mock witnesses wrote a report describing the events depicted in the film. Each mock witness wrote two versions of the report, one accurate and one containing misinformation (see Appendix F). We edited these reports to remove repetition (e.g., "…the driver looked at her friend in the passenger seat. The two girls in the front were still conscious, they looked at each other") and shortened where possible without removing key information (e.g., "An ambulance approached the scene of the crash" was changed to "An ambulance arrived"). The accurate version (word count range: 420-435) only described what participants had seen in the film. The misinformation version (word count range: 530-544) described what participants had seen in the film but also the removed scenes they had not seen. Therefore, all reports contained the same misinformation.

We counterbalanced and randomized reports so that participants read one report from each mock witness. Each report was also 'chunked', that is, presented 2-5 sentences at a time. Participants clicked 'Next' to move between sections. We chose this method to make reading easier and skimming the text (and thus not encoding the material) harder.

Recognition test. The memory test consisted of six Old (previously seen), six Control (never seen), and six Missing (removed) clips. Three of the Missing clips were cruxes i.e., crucial to the film's meaning and three were non-cruxes. Participants were asked if each clip was Old (appeared in the trauma film) or New and rated their confidence in their decision (1=not at all, 5=extremely confident). The Control clips were from online sources and also depicted car accidents and their aftermath. All clips were approximately equal in length (Old: M = 8.65s, SD = 2.15; Missing: M = 7.27s, SD = 2.74s; New: M = 8.15s, SD = 2.16s).

Note that clips were not counterbalanced (as Old or Missing). When constructing the test, Strange and Takarangi (2012, 2015) ensured that Old and Missing clips were equally memorable (pilot data available on request), not consecutive or the first or last clips in the

film (to avoid any primacy and recency effects). They also imposed a rule whereby the clips also could not have received a memorability rating in pilot testing at the anchor points of the scale. The short length of the film prevented the researchers from creating two sets of Old and Missing clips that satisfied these rules. We randomized test items to control for order effects.

Analogue trauma symptoms. The 15-item Impact of Event Scale (IES; Horowitz Wilner, & Alvarez, 1979) has two subscales: Intrusions and Avoidance. Participants rated items (e.g., "I thought about it when I didn't mean to") on 4-point scales (1 = not at all, 4 = often) in relation to the film (see Appendix G). The scale is internally consistent (Intrusions: *M* α = 0.86, range = 0.72-0.92; Avoidance: *M* α = 0.82, range: 0.65-0.90) and has strong validity (see Sundin & Horowitz, 2002).

Design and Procedure. Participants first provided informed consent—we warned them that participation would involve viewing a potentially graphic and unpleasant film, and that they could withdraw their participation any time without penalty. We told them we were working with a government agency to develop new materials for a Drivers Education campaign. There were three phases across two separate sessions. Session 1 included the encoding phase during which participants completed the PANAS, watched the film, and then completed the PANAS again. Session 2, emailed out 24-hours later with a 24-hour completion deadline, included the post-event information phase, and the memory test phase. Participants read the three eyewitness reports. To manipulate misinformation repetition, the No Misinformation condition read three accurate reports, the Single Misinformation condition read two accurate reports and one misinformation report, and the Repeated Misinformation condition read three misinformation reports. Participants were allowed to spend as much time as they needed to read the reports because this allowed us to collect timing data and exclude participants who skimmed. All participants completed a 5-minute

filler task (mazes), the PANAS, the memory test and the IES, respectively. Participants were fully debriefed and paid for their participation.

Results

Emotional Impact of Trauma Film. We first conducted analyses to ensure our film acted as a trauma analogue (Table 1). After viewing the film at Session 1, participants reported a decrease in positive mood ($t(148) = 11.37$, $p < .001$, $d = -0.74$ [-0.83, -0.64]) and an increase in negative mood, $t(148) = -13.43$, $p < .001$, $d = 1.42$ [1.25, 1.59]. Before taking the memory test at Session 2, there were no significant differences in mood between groups (Bonferroni adjusted pairwise comparison *p*s = .278-1.000). Viewing the film produced some analogue symptoms; however, there were no significant differences for total or subscale IES scores between conditions, *p*s = .213-1.000.

Table 1

Mean (and standard deviations) PANAS scores before and after viewing the film at Session 1 and after reading the reports at Session 2; IES scores at the end of Session 1 and 2 for all experiments

	Experiment 1	Experiment 2	Experiment 3
Session 1 PANAS (mood) before film			
Positive affect	31.50 (8.49)	29.07 (8.35)	29.99 (8.25)
Negative affect	11.10 (2.48)	11.89 (5.59)	11.88 (4.20)
Session 1 PANAS after film			
Positive affect	25.62 (7.40)	22.62 (5.95)	24.36 (7.18)
Negative affect	19.15 (7.63)	19.79 (8.08)	20.29 (7.70)
Session 2 PANAS after reports			
Positive affect	27.66 (8.43)	25.90 (8.09)	26.96 (8.79)
Negative affect	14.23 (6.17)	13.71 (5.90)	13.70 (5.03)
Session 1 IES (trauma symptoms) total		17.23 (9.73)	17.43 (10.25)
Intrusions subscale		9.81 (5.83)	10.01 (6.37)
Avoidance		8.56 (6.17)	9.05 (6.70)
Session 2 IES total	11.00 (9.46)	11.90 (11.66)	14.03 (12.05)
Intrusions	4.69 (5.79)	4.75 (5.68)	5.25 (6.27)
Avoidance	6.89 (6.11)	7.76 (8.22)	9.35 (8.13)

Memory distortion. Across conditions, participants were good at rejecting Control clips as Old, and recognizing Old clips as Old. However, they also wrongly claimed to have seen 34-55% of Missing clips (Table 2).

Table 2

Proportion of "Old" Responses for each clip type in each condition

	No Misinformation *M (SD)*			Single misinformation *M (SD)*			Repeated misinformation *M (SD)*		
	Exp. 1	Exp. 2	Exp. 3	Exp. 1	Exp. 2	Exp. 3	Exp. 1	Exp. 2	Exp. 3
Control clips	.02	.01	.03	.02	.004	.01	.01	.02	.01
	(.06)	(.04)	(.09)	(.05)	(.03)	(.04)	(.05)	(.07)	(.04)
Old clips	.84	.82	.78	.90	.88	.87	.90	.90	.88
	(.21)	(.22)	(.20)	(.14)	(.12)	(.15)	(.13)	(.11)	(.14)
Old *crux*	.91	.88	.84	.93	.94	.93	.95	.95	.93
clips	(.21)	(.23)	(.23)	(.15)	(.13)	(.13)	(.13)	(.12)	(.14)
Old *non-*	.77	.76	.73	.86	.83	.81	.84	.85	.84
crux clips	(.27)	(.29)	(.29)	(.22)	(.20)	(.24)	(.22)	(.21)	(.21)
Missing clips	.34	.34	.35	.52	.39	.42	.55	.48	.52
	(.26)	(.26)	(.28)	(.26)	(.33)	(.28)	(.31)	(.29)	(.30)
Missing	.35	.42	.38	.59	.45	.48	.59	.56	.56
crux clips	(.30)	(.29)	(.33)	(.31)	(.37)	(.34)	(.34)	(.33)	(.34)
Missing	.32	.25	.33	.45	.33	.36	.51	.40	.47
non-crux clips	(.30)	(.30)	(.31)	(.35)	(.35)	(.34)	(.37)	(.34)	(.35)

To determine whether our conditions differed, we conducted a 3 (Clip Type: Control, Old, Missing) × 3 (Condition: No Misinformation, Single Misinformation, Repeated Misinformation) repeated measures analysis of variance (ANOVA) on the proportion of "Old" responses. As expected, there were main effects for Clip Type ($F(1.63, 237.29)$[11] = 916.34, $p < .001$, $\eta_p^2 = .863$) and Condition, $F(2, 146) = 7.93$, $p = .001$, $\eta_p^2 = .098$), and a Clip Type x Condition interaction, $F(3.25, 2.37.29) = 6.00$, $p < .001$, $\eta_p^2 = .076$. Pairwise comparisons revealed that condition did not affect memory performance on Control (*p*s = 1.000) or Old clips, *p*s = .224-1.000).

[11] Degrees of freedom adjusted (Greenhouse-Geisser) when Mauchly's test of significance was significant.

To answer our primary question—whether repeated misinformation would lead to more memory distortion for trauma—we then focused on memory accuracy for Missing clips. Our measure of memory distortion was the proportion of Missing Clips falsely identified as "Old". We found that the Single (p = .004, d =.70 [.29, 1.11]) and Repeated Misinformation (p < .001, d = .76 [.35, 1.17]) conditions made significantly more errors for Missing Clips compared to the No Misinformation condition. Interestingly, however, memory distortion for Missing clips did not significantly differ between the Single and Repeated Misinformation conditions, p = 1.000, d = .12 [-.27, .52]. Post Hoc tests in a Bayesian ANOVA (Rouder, Speckman, Sun, Morey, & Iverson, 2009) revealed a BF_{01} = 4.00, suggesting the hypothesis that the two means are similar is four times more likely than the hypothesis that the two means are different. Put differently, a Bayes factor of 4.00 indicates substantial evidence of a negligible difference (Jeffreys, 1961; see Wetzels et al., 2011). Therefore, our findings suggest that repeated misinformation exposure does *not* lead to more memory distortion compared to single exposure.

We next analyzed memory distortion for crux vs non-crux clips. We found an effect of Clip Type: across crux and non-crux clips, participants said "Old" to more Old clips than Missing clips, F(1, 146) = 337.28, p < .001, ηp^2 = .698. We also found an effect of Crux type (crux vs. non-crux): across Old and Missing clips, participants said "Old" to more crux (vs. non-crux) clips, F(1, 146) = 28.71, p < .001, ηp^2 = .164. We also found a Clip Type x Condition interaction (F(2, 146) = 4.54, p = .012, ηp^2 = .059): compared to participants in the No Misinformation condition, participants in the Single (p = .004, d = -0.69 [-1.10, -0.28]) and Repeated Misinformation (p < .001, d = -0.73 [-1.14, -0.33]) conditions made significantly more errors for Missing Clips. There was no significant difference between the Single and Repeated Misinformation conditions (p = 1.000, d = -0.11 [-0.50, 0.29]). There were no other significant interactions ps > .163.

Confidence. To assess participants' confidence for Old/New responses, we conducted a 3 (Clip Type: Control, Old, Missing) × 3 (Condition: No Misinformation, Single Misinformation, Repeated Misinformation) repeated measures ANOVA on confidence ratings. There was a main effect for Clip Type ($F(2, 292) = 187.67$, $p < .001$, $\eta_p^2 = .562$): participants were more confident in their responses for Control ($M = 4.73$, $SD = .38$) compared to Old ($M = 4.24$, $SD = .60$; $p < .001$, $d = -0.98$ [-1.66, -0.28]) and Missing clips, $M = 3.71$, $SD = .70$; $p < .001$, $d = -1.81$ [-2.45, -1.17]. They were also more confident for Old compared to Missing clips, $p < .001$, $d = -0.81$ [-1.34, -0.28], There was a Clip Type x Condition interaction, $F(4, 292) = 2.61$, $p = .036$, $\eta_p^2 = .035$. However, pairwise comparisons revealed no differences between conditions. There was no main effect for Condition, $F(2, 146) = .35$, $p = .709$, $\eta_p^2 = .005$.

We then analyzed confidence for crux vs. non-crux clips. We found a Clip Type x Crux Type interaction, $F(1, 146) = 12.07$, $p = .001$, $\eta_p^2 = .076$. For both Old and Missing clips, participants were more confident for crux (vs. non-crux) clips, ps $< .001$. For both crux and non-crux clips, participants were more confident for Old (vs. Missing) clips. We found main effects of Clip Type ($F(1, 146) = 100.01$, $p < .001$, $\eta_p^2 = .407$) and Crux Type, $F(1, 146) = 120.00$, $p < .001$, $\eta_p^2 = .451$. We found no other interactions, ps >198.

In summary, misinformation in general increased memory errors, but repeated misinformation exposure did not increase memory errors compared to single exposure. It is possible that participants in the Single and Repeated Misinformation conditions realized that they did not remember the Missing clips but assumed the reports must be accurate, thus choosing to report those clips themselves. Therefore, in Experiment 2, we measured the authenticity of participants' memory errors in two ways. First, after completing the recognition test, participants selected the reason they said Old for each applicable clip: 1) It appeared in the film I watched yesterday, 2) I read it in the eyewitness report(s) and that was

only memory I had, 3) I read it in the eyewitness report(s) and I trust the report(s), 4) I read it in the eyewitness report(s) and I didn't want to contradict the report(s), 5) It appeared in the film I watched yesterday and in the eyewitness report(s), and 6) I guessed. Second, at the end of the survey, we asked participants whether they believed the eyewitness accounts were accurate.

Also, to further prevent participants from skimming the reports, we replaced our Session 2 attention check with a more difficult one. We told participants that each report would contain up to three code words embedded in the text (e.g., "Police cars arrived. *Write down the word england.* An officer and a middle-aged man approached the car with the girls.") and they would be asked what those code words were after reading all reports. Participants were excluded if they did not catch all six code words.

Experiment 2

Method

Participants. Of the 126 participants (42 per condition) who met our inclusion criteria,[12] 75 were female and age ranged from 18 to 69, $M = 39.23$, $SD = 12.22$. There were no significant differences in age and gender between groups, ps = .427-.673). Participants completed Time 2 $M = 30.02$ hours ($SD = 8.06$) after Part 1.

Design and Procedure. Our procedure remained the same except that we asked participants why they said "Old" for each applicable clip after the recognition test and if they believed the reports were accurate at the end of the survey. We also made our attention check

[12] Three hundred and eighty-two participants completed the study. We excluded 256 participants: 22 said that they had seen the trauma film before, 41 repeated Time 2 and, therefore, potentially read reports more than once, 113 failed to accurately complete instructional manipulation checks (Oppenheimer, Meyvis, & Davidenko, 2009), 5 admitted to watching the test clips more than once, 8 admitted to leaving the study session, 7 completed Session 2 beyond the 36 hour deadline (we kept one person who completed it 50 minutes early), 59 were excluded because they read at least one report faster than 318 words per minute, and one experienced technical issues (filler task did not load).

more robust. Both Experiments 2 and 3 were pre-registered and data for all three experiments can be accessed at https://osf.io/6y9mt/.[13]

Results

Emotional Impact of Trauma Film. We first conducted analyses to ensure our film acted as a trauma analogue.[14] After viewing the film at Session 1, participants reported a decrease in positive mood ($t(125) = 10.85$, $p < .001$, $d = -0.89$ [-1.01, -0.77]) and an increase in negative mood, $t(125) = -12.86$, $p < .001$, $d = 1.14$ [0.99, 1.29]. Viewing the film produced some analogue symptoms. There were no significant differences for IES scores between conditions, ps > .207. After reading all three reports (before taking the memory test) at Session 2, the Single Misinformation condition reported feeling in a more negative mood compared to the Repeated Misinformation condition ($p = .006$). There were no other significant differences between groups for negative or positive mood (ps = .094-1.000). Therefore, we added negative mood at T2 as a covariate in our analyses.

Memory distortion. Across conditions, participants were good at rejecting Control clips as Old, and recognizing Old clips as Old. However, similar to Experiment 1, they also wrongly claimed to have seen 34-48% of Missing clips (Table 2).

As expected, there was a main effect for Clip Type ($F(1.47, 179.20) = 104.74$, $p < .001$, $\eta_p^2 = .462$): participants responded "Old" to Old clips more than Control ($p < .001$, $d = -7.26$) and Missing clips ($p < .001$, $d = -1.96$ [-3.52, -0.39]); they responded "Old" to Missing clips more than Control clips, $p < .001$, $d = -1.81$ [-3.55, -0.07. There was also a main effect for Condition, $F(2, 122) = 3.92$, $p = .022$, $\eta_p^2 = .060$ (but not for the covariate, negative mood, $p = .572$). Pairwise comparisons showed that participants in the Repeated Misinformation

[13] All analyses were pre-registered except for the chi-square analyses examining the Accuracy Rating of reports.
[14] We found a most likely spurious baseline difference for negative mood (p = .010): the Single Misinformation condition reported higher negative mood than the other two conditions before watching the film (ps = .016-.040). However, there was no difference between conditions after the film (p=.568).

condition responded "Old" more than the No Misinformation condition (p = .018, d = -0.47 [-0.90, -0.04]); there were no other differences, ps = .405-.674. However, unlike in Experiment 1, we found no Clip Type x Condition interaction, $F(2.94, 179.20) = 1.68$, p = .174, η_p^2 =.027. We ran a Bayesian ANOVA to further examine any differences between the Single and Repeated Misinformation conditions on Missing clip responses. We found a BF_{01}=2.27, indicating anecdotal evidence of a negligible difference (Jeffreys, 1961; see Wetzels et al., 2011).

We next analyzed memory distortion for crux vs non-crux clips. We found an effect of Clip Type: participants responded "Old" to Old clips more than Missing clips, $F(1, 122) = 56.26$, $p < .001$, $\eta_p^2 = .316$. Participants also responded "Old" more to crux vs. non-crux clips, a main effect of crux, $F(1, 122) = 7.34$, p = .008, $\eta_p^2 = .057$. We also found a main effect of Condition ($F(1, 122) = 3.78$, p = .026, $\eta_p^2 = .058$): participants in the Repeated Misinformation condition responded "Old" more than those in the No Misinformation condition, p = .021. There was no difference with the Single Misinformation condition (p = .533-.563). We found no interactions, ps > .180.

Confidence. There was a main effect for Clip Type ($F(1.87, 228.01) = 23.07$, $p <$ .001, $\eta_p^2 = .159$): participants were more confident in their responses for Control (M = 4.73, SD = .49) compared to Old (M = 4.23, SD = .62; $p < .001$, d = -0.90 [-1.57, -0.22]) and Missing clips, M = 3.75, SD = .76, $p < .001$, d = -1.53 [-2.14, -0.92]. They were also more confident for Old compared to Missing clips, $p < .001$, d = -0.69 [-1.23, -0.15]. There was no main effect for Condition ($F(2, 122) = .44$, p = .643, $\eta_p^2 = .007$) or a Clip Type x Condition interaction, $F(3.74, 228.01) = 2.37$, p = .057, η_p^2 =.037.

There was a Clip Type x Crux Type interaction for crux and non-crux clips, $F(1, 122) = 4.05$, p = .046, $\eta_p^2 = .032$. For both Old and Missing clips, participants were more confident for crux (vs. non-crux) clips, ps < .001-.001. For both crux and non-crux clips, participants

were more confident for Old (vs. Missing) clips. We found main effects of Clip Type (F(1, 122) = 15.02, p < .001, η_p^2 = .110) and Crux Type, F(1, 122) = 5.55, p = .020, η_p^2 = .043. We found no other interactions, ps > .069.

Authentic Memory Distortion. We next analyzed our memory distortion data using only those Old responses classified as "authentic", that is, when participants clicked "it appeared in the film I watched yesterday" when asked why they responded "Old" to clips. We found a main effect of Clip Type, F(1.64, 199.78) = 52.81, p < .001, η_p^2 = .302: participants responded "Old" to Old clips (M = .54, SD = .33) more than Control (M = .01, SD = .04, p < .001, d = -2.26 [-3.86, -0.64]) and Missing clips (M = .21, SD = .22, p < .001, d = -1.18 [-2.51, 0.16]); they responded "Old" to Missing clips more than Control clips, p < .001, d = -1.27 [-3.62, 1.10]. We also found an interaction with Clip Type and T2 negative mood, F(1.64, 199.78) = 4.27, p = .022, η_p^2 = .034. For our crux/non-crux analyses we again found a main effect of Clip Type, (with participants saying "Old" to Old clips more than Missing clips), F(1, 122) = 38.02, p < .001, η_p^2 = .238, and an interaction with Clip Type and T2 negative mood, F(1, 122) = 4.70, p = .032, η_p^2 = .037.[15]

Accuracy rating. The majority (88.1%) of participants believed that the reports accurately described the trauma film. A chi-square analysis showed no difference between conditions (p = .130).

The lack of Clip Type x Condition interaction and the lower Missing Clip error rate for the Repeated Misinformation group compared to Experiment 1 (39% vs. 52%

[15] Across participants and all six Missing clips, there were 159, out of a potential 756 (six Missing clips x 126 participants) instances (or 21.0%) of participants reporting that they clicked "Old" because they saw it in the film only. The other responses were rarely chosen and are not analyzed here. There were 27 instances (3.6%) of reporting, "I read it in the eyewitness report(s) and that was only memory I had, 12 instances (1.6%) of "I read it in the eyewitness report(s) and I trust the report(s)", five instances (0.7%) of "I read it in the eyewitness report(s) and I didn't want to contradict the report(s) (5 instances)", 81 instances (10.7%) of "It appeared in the film I watched yesterday and in the eyewitness report(s)" and 20 instances (2.6%) of guessing. We acknowledge that selecting "It appeared in the film I watched yesterday and in the eyewitness report(s)" may qualify as a false memory. However, we did not find differences in conditions for this response either, and do not report results here.

respectively) suggested to us that our codeword attention check may have been *too* robust. That is, embedding the attention check in the reports may have increased participants' discrepancy detection while reading, ultimately decreasing errors. Indeed, full (compared to divided) attention decreases false memories for misinformation items (Zaragoza & Lane, 1998). Therefore, in Experiment 3, we removed this attention check (replacing it with the easier Experiment 1 check). To still try and prevent skimming, we gave participants a minimum period of time before they could move past each eyewitness report. Specifically, for each report, the 'Next' button at the end of each report did not appear until participants were given enough time to read the reports at no faster than 318 words per minute. We also tweaked our programming to make it impossible for participants to watch the film or the clips more than once.

Experiment 3

Method

Participants. In our first experiment, we found a basic misinformation effect, that is, a difference between the No Misinformation and Single Misinformation groups on errors for Missing clips, d = .70. To ensure we had enough power to find this misinformation effect again, we also ran an a priori power analysis for a two-tailed, two-group t-test with alpha = .05, power = .95, and d = .70. The analysis revealed that the desired total sample size for our study was at least 165 (55 per group, higher than our previous two experiments).

Of the 165 participants (55 per condition) who met our inclusion criteria, 94 were female and aged 21 to 76 (M= 38.09, SD=10.72). There were no significant differences in age and gender between groups (ps=.298-.679). Participants completed Time 2 M = 32.08 hours (SD=9.49) after Part 1.[16]

[16] Two hundred and two participants completed the study. We excluded and replaced 37 participants: 10 said that they had seen the trauma film before, 7 repeated Time 2 and, therefore, potentially read reports more than once, 11 failed to accurately complete instructional manipulation checks, 5 admitted to leaving the study

Design and Procedure. Our procedure remained the same except that we reversed our attention check to the one we used in Experiment 1 and participants had a minimum period of time before they could move past each eyewitness report.

Results

Emotional Impact of Trauma Film. We first conducted analyses to ensure our film acted as a trauma analogue. After viewing the film at Session 1, participants reported a decrease in positive mood ($t(164) = 10.59$, $p < .001$, $d = -0.73$ [-0.82, -0.64]) and an increase in negative mood, $t(164) = -15.83$, $p < .001$, $d = 1.36$ [1.20, 1.51]. After reading all three reports and before taking the memory test at Session 2, there was no difference in mood between groups ($p > .250$). Viewing the film produced some analogue symptoms, however, there was no difference between groups for total or subscale score *p*s = .057-1.000.

Memory distortion. Across conditions, participants were good at rejecting Control clips as Old, and recognizing Old clips as Old. However, they also wrongly claimed to have seen 35-52% of Missing clips (Table 2).

As expected, there was a main effect for Clip Type ($F(1.62, 262.84) = 852.08$, $p < .001$, $\eta_p^2 = .840$): participants responded "Old" to Old clips more than Control ($p < .001$, $d = -6.51$) and Missing clips ($p < .001$, $d = -1.77$ [-3.15, -0.38]); they responded "Old" to Missing clips more than Control clips, $p < .001$, $d = -1.94$ [-3.50, -0.39]. There was also a main effect for Condition, $F(2, 162) = 5.67$, $p = .004$, $\eta_p^2 = .065$: participants in the Repeated Misinformation condition responded "Old" more than the No Misinformation condition ($p = .003$, $d = -0.44$ [-0.82, -0.06]); there were no other differences, *p*s = .182-.431. Similar to Experiment 1, we found a Clip Type x Condition interaction, $F(3.25, 262.84) = 3.98$, $p = .007$, $\eta_p^2 = .047$. Pairwise comparisons confirmed that condition did not affect memory

session, and 4 completed Session 2 beyond the 36-hour deadline. Notably, our changes to the procedure reduced the number of participants who did not meet our inclusion criteria.

performance on Control, *p*s = .230-1.000. However, participants in the Repeated (p = .008, d = -0.58 [-0.96, -0.20]) and Single Misinformation (p = .019, d = -0.51 [-0.89, -0.13]) conditions recalled more Old clips than the No Misinformation condition. This finding may suggest that participants in the Repeated and Single Misinformation (vs. No Misinformation) conditions attended to the reports more, thus improving their memory for Old clips. However, there was no difference in reading time between groups, making further research necessary to understand this difference. There was no difference between Single and Repeated conditions, p = 1.000. To answer our primary question—whether repeated misinformation would lead to more memory distortion for trauma—we examined memory accuracy for Missing clips. Participants in the Repeated Misinformation condition also falsely remembered more Missing clips than the No Misinformation condition, p = .010, d = -0.59 [-0.97, -0.20]. There were no other differences, p = .265-.616. A Bayesian ANOVA found a BF_{01}=2.27, indicating anecdotal evidence of a negligible difference between Single and Repeated Misinformation conditions on Missing clip responses (Jeffreys, 1961; see Wetzels et al., 2011).

We next analyzed memory distortion for crux vs non-crux clips. We found an effect of Clip Type: participants responded "Old" to Old clips more than Missing clips, $F(1, 162) = 353.80$, $p < .001$, $\eta_p^2 = .686$. Participants also responded "Old" more to crux vs non-crux clips, a main effect of crux, $F(1, 162) = 29.93$, $p < .001$, $\eta_p^2 = .156$. We also found a main effect of Condition, $F(2, 162) = 6.78$, $p = .001$, $\eta_p^2 = .077$: participants in the Repeated Misinformation condition responded "Old" more than those in the No Misinformation condition (p = .001); there were no other differences, *p*s = .085-.451.

Confidence. There was a main effect for Clip Type ($F(1.89, 305.67) = 207.12$, $p < .001$, $\eta_p^2 = .561$): participants were more confident in their responses for Control (M = 4.64, SD = .49) compared to Old (M = 4.15, SD = .53, $p < .001$, d = -0.96 [-1.60, -0.31]) and Missing clips, M = 3.70, SD = .67, $p < .001$, d = -1.60 [-2.18, -1.02]. They were also more

confident for Old compared to Missing clips, $p < .001$, $d = -0.75$ [-1.29, -0.20]. There was no main effect for Condition or a Clip Type x Condition interaction, *p*s = .202-.582.

For crux and non-crux clips, there was a Clip Type x Crux Type interaction ($F(1, 162) = 15.56$, $p < .001$, $\eta_p^2 = .088$): across Old and Missing clips, participants were more confident for crux (vs. non-crux) clips; across crux and non-crux clips, participants were confident for Old (vs. Missing clips), *p*s < .001. We found main effects of Clip Type ($F(1, 162) = 90.42$, $p < .001$, $\eta_p^2 = .358$) and Crux Type, $F(1, 162) = 127.34$, $p < .001$, $\eta_p^2 = .440$.

Authentic memory distortion. We then analyzed our memory distortion data for only "authentic" Old responses for Missing clips (21%-25% across conditions).[17] We found a main effect of Clip Type, $F(1.88, 305.28) = 296.35$, $p < .001$, $\eta_p^2 = .647$. There was no effect of Condition or a Clip Type x Condition interaction, *p*s = .270-.344. For our crux/non-crux analyses, we found a main effect of Clip Type ($F(1, 162) = 173.29$, $p < .001$, $\eta_p^2 = .517$) and Crux Type, $F(1, 162) = 7.56$, $p = .007$, $\eta_p^2 = .045$.

Accuracy rating. The majority (88.5%) of participants believed that the reports accurately described the trauma film. A chi-square analysis showed no difference between conditions, $p = .386$.

Discussion

Overall, our results suggest exposure to trauma misinformation leads to increased false memories for trauma stimuli. However, this misinformation effect disappears after increasing people's discrepancy detection (Experiment 2) or excluding inauthentic memory errors (where participants later reported clips as Old because it was described in the reports or

[17] Across participants and all six Missing clips, there were 230, out of a potential 990 (six Missing clips x 165 participants) instances (or 23.2%) of participants reporting that they clicked "Old" because they saw it in the film only. The other responses were rarely chosen and are not analyzed here. There were 38 instances (3.8%) of reporting, "I read it in the eyewitness report(s) and that was only memory I had, 23 instances (2.3%) of "I read it in the eyewitness report(s) and I trust the report(s)", seven instances (0.7%) of "I read it in the eyewitness report(s) and I didn't want to contradict the report(s) (5 instances)", 100 instances (10.1%) of "It appeared in the film I watched yesterday and in the eyewitness report(s)" and 27 instances (2.7%) of guessing.

as a guess rather than seeing it in the film alone Experiment 2 and 3), perhaps suggesting that trauma memory malleability with this paradigm can mostly be explained by natural memory decay over time. Further, exposure to *repeated* misinformation did not lead to more memory errors compared to single exposure across experiments.

Consistent with previous research (e.g., Monds et al., 2013), participants in Experiments 1 and 3 falsely remembered misinformation as being included in the trauma analogue. At test, participants may have used simple heuristics such as the familiarity of the clip's content to determine whether the clips were shown at encoding. The misinformation, provided in the mock witness reports during the post-event information phase, likely felt familiar to them and thus was indistinguishable from what they originally encoded, leading to their inaccurate Old responses. Put differently, they failed to monitor the source of the clip. However, *repeated* misinformation exposure did not lead to more false memories for Missing clips compared to single misinformation exposure. How, then, do we reconcile our data with previous findings showing repeated misinformation exposure enhances memory distortion? There may be a simple explanation for our findings based on raw means: the number of errors for Missing clips made by the Single Misinformation condition was in between the number made by the No Misinformation and the Repeated Misinformation conditions. That is, the Single Misinformation condition made more errors than the No Misinformation condition but fewer than the Repeated Misinformation condition. Therefore, the repeated vs. single misinformation effect may exist but be very small as suggested by our effect sizes.

It is important to note, however, that our results from Experiment 2 and 3 (when only looking at authentic errors) suggest that misinformation had no effect on memory for Missing clips. Across the two experiments and conditions, participants had an error rate of 19-25% for Missing clips. This rate is similar to the one found by Strange and Takarangi (26%; 2012), whose paradigm we adapted. Therefore, our data may simply reflect a bias to respond "Old"

for emotional stimuli. For example, all participants may have been biased to say that any clip that fit the gist or emotional tone of the film was "Old". Indeed, this may explain why we consistently found that participants, across conditions, were more like to falsely remember crux (traumatic) clips compared non-crux clips. Unfortunately, we could not run signal detection analyses to investigate this explanation due to the small number of test clips, especially once those clips were separated into crux and non-crux clips.

Importantly, other features of crux clips besides emotionality—e.g., how well they fit the movie, how crucial they were to the narrative, and how much they stood out in memory (see Strange and Takarangi, 2012)—may have contributed to our finding. However, given that the film depicts a highly traumatic event, it is unsurprising and likely unavoidable that traumatic clips consisted of these other features along with emotionality. Regardless, our finding suggests that emotional misinformation items should be included when investigating misinformation effects following trauma.

It is worrying that participants were more confident for crux (vs. non-crux) Missing clips responses despite making more errors for such clips. Furthermore, although higher confidence for non-crux Missing clips was associated with decreased memory errors for all Missing clips (rs = -.29--.26, ps = .001), the same did not apply for crux Missing clips, ps > .532. Given that previous research suggests that highly confident people seem more credible and persuasive (see Cramer, Brodsky, & DeCoster, 2009), this finding could have significant real-world consequences, such as leading investigations astray.

We found no significant differences for IES scores between conditions, suggesting that repeated misinformation exposure does not increase PTSD symptomology. This finding is perhaps unsurprising: *all* participants read three eyewitness reports describing traumatic scenes. Thus, the few additional sentences (half of which were non-traumatic) the misinformation conditions read were unlikely to have significantly increased symptomology.

Of course, our study has limitations. The film likely did not replicate the stress and emotionality experienced during trauma, thus our results may not generalize to real-world scenarios. However, trauma film paradigms do elicit analogue PTSD symptoms (James et al., 2016). Furthermore, the *DSM-5* considers professional indirect exposure through electronic media, television, movies or pictures as a Criterion A stressor, indicating that it is important to investigate the effect of film content itself on trauma memory and symptomology.

In summary, our findings have important theoretical implications. Our data suggest that for trauma events, repeated misinformation exposure does *not* result in more memory errors compared to single misinformation exposure. Across conditions, the traumatic elements of the stimulus produced more memory distortion compared to the non-traumatic elements. But misinformation did not lead to more PTSD symptomology. Although these results seem encouraging, it is critical to note that any degree of misinformation exposure led to a 33-59% memory error rate. Thus, our findings have implications for victim/eyewitness accuracy. If people are exposed to misinformation, it appears likely that their memory will be distorted.

Chapter Seven: Can attention to source reduce source memory failures when people remember traumatic stimuli?[18]

Author Contributions: Sasha Quayum developed the study concept and design (75% contribution), collected data, performed the data analysis and interpretation (100% contribution) and drafted the manuscript (80% contribution to the writing and editing). Melanie Takarangi and Deryn Strange provided guidance during the design phase. All co-authors made critical revisions to the manuscript. All authors approved the final version of the manuscript for submission.

Abstract

Trauma victims often come to remember experiencing more trauma than they initially reported. We propose this "memory amplification" reflects a source-monitoring error: people incorporate additional post-event information (PEI) into their event memory. We examined whether PEI increases traumatic memory distortion, and the extent to which it can be manipulated. In Experiment 1a, participants viewed traumatic photographs, and completed memory tests before and 24-hours after PEI exposure. As predicted, PEI distorted traumatic memory. Experiments 1b and 1c found that improving source monitoring ability with a subtle warning eliminated PEI-related errors with a short (24 hours) but not long (1 week) delay. We combined our delay (24 hours, one week) and warning (warning, no warning) conditions in Experiment 2, and also asked participants to determine the source of each photo. Our results suggest that evaluating the source of traumatic photos increases source monitoring, making people resistant to inaccurate PEI.

Introduction

[18] Nahleen, S., Strange, D., Nixon, R. D. V., & Takarangi, M. K. T. (2018). *Can attention to source reduce source memory failures when people remember traumatic stimuli?* Unpublished manuscript.

People can come to remember thematically related trauma events (e.g., sniper fire) that might have occurred within a larger context (e.g., military deployment) that they initially failed to report (e.g., Giosan, Malta, Jayasinghe, Spielman, & Difede, 2009). However, researchers have not systematically investigated why this "memory amplification" occurs. Of course, people may sometimes accurately remember additional events that they failed to remember earlier. Alternatively, people may be exposed to post-event information (PEI) about traumatic experiences—via suggestive questioning (e.g., Crombag, Wagenaar, & van Koppen, 1996) for example—which can be incorporated into their trauma memory and mistaken for their actual experience. That is, memory amplification may reflect, in part at least, source monitoring errors (Johnson, Hashtroudi, & Lindsay, 1993).

Numerous studies have investigated whether trauma memories distort like other memories (see Byrne, Hyman, & Scott, 2001; Porter & Birt, 2001). Some studies have found that memories for negative, potentially traumatic stimuli may be less susceptible to distortion than positive (Peace & Porter, 2004) or neutral (Monds, Paterson, Kemp, & Bryant, 2013) memories. Others have found that peripheral or contextual details (e.g., location of an object) may be forgotten or confused, while central, emotionally arousing details are enhanced (Mather et al., 2006). Generally, trauma memories appear to be malleable (e.g., Wessel, van der Kooy, & Merckelbach, 2000). For example, memories for shocking, emotional public events can be inconsistent (e.g., Neisser & Harsch, 1992) and susceptible to misinformation (Crombag et al., 1996).

Importantly, memory distortion also occurs for *personally experienced* traumatic events: people report additional trauma events that they initially failed to report. For example, Southwick, Morgan, Nicolaou, and Charney (1997) asked Desert Storm veterans at two time-points whether certain traumatic events (e.g., seeing others killed or wounded) occurred during their service. Seventy percent of participants reported events at follow-up that they did

not report initially. This amplification was positively associated with PTSD symptoms. Similar results have been observed with other soldier and veteran populations (Bolton, Gray, & Litz, 2006; Engelhard, van den Hout, & McNally, 2008; King et al., 2000; Roemer, Litz, Orsillo, Ehlich, & Friedman, 1998), and in post 9/11 disaster restoration workers (Giosan et al., 2009).

Memory amplification may reflect a source monitoring error (Strange & Takarangi, 2012, 2015). According to the source monitoring framework (SMF; Johnson et al., 1993; Lindsay, 2008), rather than storing and retrieving labels specifying the origins of event details, we often make judgments about whether we truly experienced events, or event details, based on subjective attributes of our mental experience, such as how familiar or perceptually/emotionally vivid event details feel. Sometimes such heuristics lead to errors. Following trauma, people often later encounter additional details and perspectives about the event, for example, in media reports, unit debriefings, and therapy. People likely imagine this PEI, making it more familiar and/or vivid, like details encoded from the original event. Thus, they may inadvertently incorporate the familiar PEI images into their original memory.

Unfortunately, field studies cannot corroborate what participants actually experienced. Therefore, it is unclear if participants became more accurate or inaccurate over time. It is also unclear whether some participants faced an increasing, rather than decreasing, trauma load (e.g., if they were deployed again), which may have distorted their memory of earlier events. Therefore, controlled laboratory studies are needed to investigate memory amplification. To examine the role of source monitoring on trauma memory distortion, Strange and Takarangi (2015) had participants watch a film about a fatal car accident that was cut into several clips with six clips removed. Twenty-four hours later, participants judged whether old, removed, and control clips were old (i.e., previously seen) or new. Participants who were warned that some clips were removed before watching the

film, thereby encouraging more deliberate source monitoring at test, exhibited reduced memory distortion by 11% versus those who were not warned. By contrast, providing descriptions of the missing clips over visual static for the duration of the missing clips during the encoding phase increased distortion by 12% compared to the warning condition. These results fit with research showing that people sometimes misattribute information from verbal descriptions to visual sources (e.g., Intraub & Hoffman, 1992), and that inducing more stringent evaluations of experiences can reduce false recognitions (Dodson & Johnson, 1993). However, the delay period between encoding and test was short (24-hours). Therefore, it is unclear if attempts at increasing discrepancy detection still reduce memory errors after longer delays, that is, after the original event memory has weakened, making detecting discrepancies less likely (Loftus, 2005).

We extended the analogue trauma memory paradigm. First, we added a baseline memory test because field studies have defined memory amplification as the change in response *over time*. Our analogue design allowed us to investigate accuracy across time. Using photo stimuli allowed us to test participants on numerous items, giving more opportunity for errors, and to generalize our findings beyond one specific set of stimuli.

Experiments 1a-1c

Method

Participants. For Experiment 1a, we aimed to recruit a similar sample size as Oulton, Takarangi, and Strange (2016), whose paradigm we adapted (N = 96). We powered for Experiments 1b and 1c using the difference found in Experiment 1a between memory errors for described vs. undescribed New photos (i.e., the effect of PEI on previously unseen stimuli). A power analysis using G*power (Faul, Erdfelder, Lang, & Buchner, 2007) suggested a total sample of 88 participants to detect an effect size of d = .39, with 95%

power, using a matched pairs *t* test with alpha at .05. We recruited more than 88 participants in each experiment in case of attrition.

Four hundred and forty participants from Amazon Mechanical Turk—restricted to US workers who had an approval rating of at least 95%—completed the three successive experiments.[19] We excluded 160 participants: 64 failed to accurately complete instructional manipulation checks[20] (Oppenheimer, Meyvis, & Davidenko, 2009), 77 admitted to looking away from the photos during the tests, two appeared to have restarted one of the surveys, 7 had technical issues (e.g., images not loading) and 10 completed Time 2 beyond the 24 hour deadline.[21] Of the remaining 280 participants (total *N*s in each experiment were: 93 in Experiment 1a, 96 in Experiment 1b, 91 in Experiment 1c), 145 were female and their age ranged from 18 to 78, M = 36.51, SD = 12.08. The mean delay period between T1 and T2 for Experiment 1a and 1b was 29.94 hours, SD = 7.25. The mean delay period between T1 and T2 for Experiment 1c was 7.39 days, SD = .42. Participants received US$5.00 for their time. Data for all four experiments are available online (https://osf.io/mnkqz/).

Materials. These materials were used in Experiments 1a-1c.

Traumatic stimuli. We selected 96 IAPS photos (Lang, Bradley, & Cuthbert, 2008) depicting traumatic scenes (e.g., burn victims, death, animal cruelty). We chose photographs based on negative valence ratings; all photos were rated <3 on a scale of 1 (low pleasure) to 9 (high pleasure). We divided the photographs into six sets of 16—matched on valence and arousal—that were counterbalanced into three sets of "Old-Negative" items, and three sets of

[19] Workers in all experiments were blocked from participating in more than one of the experiments.
[20] An IMC is an attention check question inserted among other items that asks participants to ignore the standard response format and instead confirm that they have read the instruction(s). Discarding responses by participants who fail to read the instructions increases the signal-to-noise ratio of a data set and, thus, increases the power of an experiment.
[21] We allowed participants 24 hours to complete Time 2. We included six people who completed Time 2 marginally past that 24-hour deadline (ranging from approximately 15 minutes to 3 hours).

"New-Negative" items. We also used two sets (one at each test) of 16 neutrally valenced IAPS photos as "New-Neutral" fillers.

Mood ratings. We used the 20-item Positive Affect Negative Affect Schedule (PANAS) to assess mood. Subjects rated how they felt right then on 10 positive (e.g., excited, enthusiastic) and 10 negative (e.g., distressed, upset) mood adjectives on a 5-point scale (1 = very slightly or not at all; 5 = extremely). The scales have excellent convergent correlations with more extensive measures of mood (.76 to .92; Watson, Clark, & Tellegen, 1988). Both subscales correlate with other distress and psychopathology measures, including the Hopkins Symptom Checklist (NA: r = .65-.74; PA: r = −.29- -.19; Derogatis, Lipman, Rickels, Uhlenhuth, & Covi, 1974), and Beck Depression Inventory, NA: r = .56-.58; PA: r = -.35- −.36; Beck, Ward, Mendelson, Mock, & Erbaugh, 1961).

Photo ratings. Participants rated how unpleasant, distressing, and disgusting they found the photos on a 7-point scale (1 = not at all, 7 = extremely). They also rated how much attention they paid to the photos on the same scale.

Emotional arousal ratings. Participants in the no warning conditions rated, on a 7-point scale (1 = not at all, 7 = highly), how emotionally arousing each photo was (Porter, ten Brinke, Riley, & Baker, 2014). All participants rated how emotionally arousing each description was on the same scale.

Post-event information. We provided PEI via photo descriptions (e.g., "a young boy points a gun at the viewer"). Participants read 16 descriptions of previously seen photos (correct PEI), and 16 descriptions of previously never seen New-Negative photos (misinformation). To create the photo descriptions, we ran a pilot study where participants (N = 212) each viewed a subset of 24 photos and were asked to "describe the event depicted in the photograph in one sentence." Based on the consistent details mentioned by participants,

three research assistants created descriptions for each photo, consisting of accurate, informative, and exact details of the photo, which we used as the PEI.

Recognition test. There were several versions of the two memory tests based on counterbalancing of the photo sets. Test 1 consisted of 16 Old-Negative photos (presented at initial photo presentation), 16 New-Negative photos, and 16 New-Neutral fillers (neither presented at initial photo presentation). Participants judged whether each photo was Old or New, and rated how confident they were in their decision on an 11-point scale (0 = 0%, 10 = 100%). These photos were never shown again. Test 2 consisted of 16 described and 16 undescribed Old-Negative photos, 16 described and 16 undescribed New-Negative photos, and 16 New-Neutral fillers. Participants identified each photo as Old or New and its source (saw the photo at T1, read the description at T2, both, neither, or unsure).

Trauma symptoms. We assessed participants' trauma analogue symptoms using the 17-item PTSD Checklist (PCL-S; Weathers, Litz, Herman, Huska, & Keane, 1993; see Appendix H). Participants rated how much they had been bothered by 17 symptoms (e.g., feeling jumpy or easily startled) on a 5-point scale (1 = not at all; 5 = extremely). Possible scores range from 17 to 85 with higher scores indicating more symptoms. The PCL correlates highly with a gold standard PTSD diagnostic interview, the Clinician Administered PTSD Scale (r = .93; Blanchard, Jones-Alexander, Buckley, & Forneris, 1996).

Design and Procedure. All procedures were approved by the Flinders University Ethics Committee. Before participation, participants provided informed consent—we warned participants that participation would involve viewing potentially very negative, graphic photos, and that they could withdraw participation at any time without penalty. We provided participants a cover story, namely that we were interested in how different modes of presentating stressful stimuli (visual vs. verbal) affect emotional responses. We administered the study using Qualtrics™ software (2015). The study was entirely within-subjects and

comprised of four phases across two separate sessions. Time 1 included the initial photo presentation phase and the first recognition or baseline memory test phase (Test 1). Time 2, which was emailed out after a delay, included the PEI phase and the final memory test phase (Test 2). Participants had 24-hours to complete Time 2 once they received the email. During the initial photo presentation phase, participants in all three experiments completed the PANAS and saw 48 IAPS photos presented for 2.5 seconds each. They rated how emotionally arousing the photos were after each photo. After the photo presentation, participants completed the PANAS again and the photo ratings (how unpleasant, distressing and disgusting the photos were overall). After a 20-minute Sudoku filler task, participants completed Test 1. After a 24-hour (Experiment 1a and Experiment 1b) or 1-week (Experiment 1c) delay, participants completed the PANAS before reading descriptions of some Old-Negative and New-Negative photos. In Experiment 1a, they rated how emotionally arousing the descriptions were after each description. In Experiments 1b and 1c, they were instead asked to judge if each description was Old (i.e., they had seen a photo of the description in the previous session) or New, to increase their discrepancy detection. After completing the PANAS again and the description ratings (how unpleasant, distressing and disgusting the descriptions were overall), there was a brief 5-minute Sudoku filler task. We then administered the Test 2 memory task and the PCL. Finally, we debriefed and paid participants for their participation.

Results

Emotional impact of photos and descriptions. We first investigated whether our stimuli acted as an analogue for trauma and trauma-related PEI. Results are similar for all three experiments (see Table 1). Participants reported a decrease in positive mood and an increase in negative mood after viewing the photos and reading the descriptions. They rated the photos as very unpleasant, distressing, disgusting, and moderately emotionally arousing.

They rated the descriptions as somewhat unpleasant, distressing, disgusting and emotionally arousing. Viewing IAPS photos produced some analogue symptoms among participants across all symptom clusters (re-experiencing, avoidance, and hyperarousal). These data suggest that our stimuli were an effective analogue for unpleasant events.

Table 1

Affect before and after viewing the photos at T1 and reading the descriptions; Photo (T1), description (T2), and emotional arousal ratings (T1 and T2); PCL scores (T2) for all experiments

	Experiment 1a		Experiment 1b		Experiment 1c		Experiment 2	
	T1 *M* (*SD*)	T2 *M* (*SD*)	T1 *M* (*SD*)	T2 *M* (*SD*)	T1 *M* (*SD*)	T2 *M* (*SD*)	T1 *M* (*SD*)	T2 *M* (*SD*)
PANAS positive scale	Before: 29.74 (8.96) After: 25.35 (8.14)	Before: 27.87 (10.11) After: 25.35 (9.88)	Before: 28.94 (8.09) After: 24.25 (7.46)	Before: 26.97 (8.83) After: 24.70 (8.37)	Before: 29.53 (8.35) After: 23.30 (6.83)	Before: 26.69 (9.19) After: 23.58 (7.65)	Before: 30.91 (8.66) After: 25.20 (8.06)	Before: 28.82 (9.70) After: 25.62 (9.27)
PANAS negative scale	Before: 12.13 (4.30) After: 18.09 (8.06)	Before: 11.84 (3.87) After: 14.25 (6.35)	Before: 12.22 (3.86) After: 17.79 (6.69)	Before: 12.25 (3.21) After: 13.59 (3.51)	Before: 11.86 (3.49) After: 18.71 (8.13)	Before: 12.19 (4.26) After: 14.18 (6.08)	Before: 12.62 (4.17) After: 19.21 (7.67)	Before: 12.57 (4.54) After: 15.00 (6.77)
Unpleasant[a]	6.09 (1.27)	4.27 (1.73)	6.28 (1.22)	5.06 (1.82)	6.25 (1.12)	4.98 (1.73)	6.35 (1.10)	5.15 (1.63)
Distressing	5.47 (1.61)	3.73 (1.95)	5.81 (1.63)	4.35 (1.89)	5.56 (1.68)	4.27 (1.85)	6.00 (1.40)	4.53 (1.84)
Disgusting	5.60 (1.47)	3.82 (1.84)	6.01 (1.32)	4.59 (1.83)	5.92 (1.33)	4.29 (1.82)	6.13 (1.27)	4.66 (1.85)
Emotionally arousing	4.18 (1.33)	3.88 (1.43)	4.37 (1.36)		4.55 (1.38)		4.75 (1.27)	4.65 (1.42)
Attention[22]	6.70 (.62)	6.65 (.58)	6.79 (.46)	6.79 (.48)	6.75 (.46)	6.76 (.48)	6.76 (.54)	6.76 (.57)
PCL total		23.82 (9.07)		22.85 (7.22)		25.52 (12.81)		25.70 (11.88)
Re-experiencing subscale		6.88 (2.76)		6.74 (2.63)		7.70 (4.57)		7.61 (4.32)
Avoidance subscale		9.98 (4.03)		9.44 (3.02)		10.40 (4.74)		10.57 (4.72)
Hyperarousal subscale		7.09 (3.43)		6.65 (2.93)		7.42 (4.29)		7.52 (3.95)

[22] The fact that participants said Old for Old-Negative photos far more often than for New-Negative or New-Neutral photos also suggests that they paid attention to the photos.

Table 2

Proportion of "Old" responses for photos at test for Experiments 1a-1c

	Experiment 1a			Experiment 1b			Experiment 1c		
	T1 *M* (*SD*)	T2 Described *M* (*SD*)	T2 Undescribed *M* (*SD*)	T1 *M* (*SD*)	T2 Described *M* (*SD*)	T2 Undescribed *M* (*SD*)	T1 *M* (*SD*)	T2 Described *M* (*SD*)	T2 Undescribed *M* (*SD*)
New-Neutral filler	.02 (.07)		.04 (.09)	.02 (.05)		.05 (.08)	.01 (.03)		.07 (.11)
Old-Negative	.93 (.12)	.82 (.15)	.83 (.16)	.96 (.07)	.82 (.15)	.83 (.15)	.97 (.05)	.74 (.18)	.74 (.15)
New-Negative	.08 (.09)	.20 (.23)	.12 (.15)	.08 (.09)	.11 (.12)	.10 (.12)	.08 (.12)	.12 (.21)	.10 (.11)

Table 3

Proportion of "Old" responses for photos at test for each condition in Experiment 2

	New-Neutral filler		Old-Negative			New-Negative		
	T1 *M* (*SD*)	T2 *M* (*SD*)	T1 *M* (*SD*)	T2 Described *M* (*SD*)	T2 Undescribed *M* (*SD*)	T1 *M* (*SD*)	T2 Described *M* (*SD*)	T2 Undescribed *M* (*SD*)
24-hour delay/No Warning	.01 (.08)	.04 (.09)	.95 (.08)	.79 (.18)	.83 (.17)	.10 (.15)	.15 (.20)	.12 (.16)
24-hour delay/Warning	.01 (.02)	.04 (.08)	.95 (.08)	.82 (.14)	.83 (.16)	.07 (.09)	.12 (.13)	.11 (.10)
1-week delay/No Warning	.01 (.02)	.05 (.07)	.95 (.08)	.62 (.23)	.65 (.21)	.12 (.18)	.12 (.15)	.13 (.14)
1-week/Warning	.01 (.09)	.03 (.06)	.96 (.08)	.61 (.22)	.66 (.22)	.08 (.16)	.11 (.11)	.09 (.10)

Memory for photos. We examined participants' performance on New-Neutral fillers, Old-Negative and New-Negative items. As shown in Table 2 (for Experiments 1a-1c), although participants were significantly better at rejecting New-Neutral fillers as Old at T1 compared to T2 (*p*s < .001-.033, *d* = .21-72), means were at floor in all three experiments. This finding is unsurprising given that, to reject the New-Neutral filler photos, participants only had to remember that photos in the initial photo presentation stage were all negative.

Next, we analyzed our data for Old-Negative and New-Negative photos. Because the two memory tests comprised different items, all of which were undescribed at T1, whereas half were described and half undescribed at T2, it was impossible to run a single full factorial analysis with all three independent variables (Time, Photo Type, PEI) included. Therefore, for each experiment, we ran two separate 2 (Time: T1, T2) × 2 (Photo Type: Old-Negative, New-Negative) repeated measures ANOVAs, one where photos were described (PEI) at T2, and the other where photos were undescribed (no PEI) at T2. For both undescribed and described photos, participants were more likely to say "Old" for Old-Negative photos than New-Negative photos across experiments, an effect for Photo Type, *F*s = 1300.85-4588.11, *p*s < .001, ηp^2s = .934-.980. Participants recalled fewer photos at T2 than T1 for both described and undescribed photos across experiments (*F*s = 5.39-91.70, *p*s < .001-.022, ηp^2s = .055-.505), except for described photos in Experiment 1a, $F(1, 92) = .04$, *p*s = .853, $\eta p^2 = .000$.

We next turn to our amplification data. In Experiment 1a, we hypothesized that participants would inaccurately monitor the source of PEI and therefore be significantly more likely to falsely remember described New-Negative photos as Old versus undescribed New-Negative photos. In both the undescribed and described photo analyses, we found a Photo Type x Time interaction, $F(1, 92) = 63.21\text{-}101.40$, *p*s < .001, $\eta p^2 = .407\text{-}.524$: critically, participants said "Old" for fewer Old-Negative photos at T2 than T1 (*p*s < .001)—likely

because they forgot some photos after a delay—whereas they said "Old" for more New-Negative photos at T2 than T1 (*p*s < .001-.009). Therefore, without any subtle warnings to increase discrepancy detection, participants made more errors at T2 for New-Negative photos compared to T1. To specifically compare memory accuracy on described vs. undescribed photos at T2, we then focused on memory accuracy at T2 using a 2 (Photo Type: Old-Negative, New-Negative) × 2 (PEI: described, undescribed) repeated measured ANOVA. We found main effects of Photo Type (people said "Old" for more Old vs. New photos; ($F(1, 92) = 745.49$, $p < .001$, $\eta p^2 = .890$) and PEI (errors were higher for described vs. undescribed photos; ($F(1, 92) = 6.09$, $p = .015$, $\eta p^2 = .062$)). Critically, we found a Photo Type x PEI interaction ($F(1, 92) = 22.11$, $p < .001$, $\eta p^2 = .194$). There was no difference in accuracy between described and undescribed Old-Negative, $p = .665$. Importantly, however, participants were twice as likely to falsely remember *described* New-Negative photos—wrongly identifying 20% of them as Old—compared to undescribed New-Negative photos (8%; $p < .001$). This finding suggests that participants may have made source monitoring errors, mistaking PEI for memories of the trauma event, and supports our hypothesis that external PEI can produce amplified memories of trauma.

In Experiment 1b, our primary hypothesis was that asking participants whether they had seen photos of the descriptions would increase their discrepancy detection, leading to no difference in errors for described and undescribed New-Negative photos. For both the undescribed and described photo analyses, we found a Photo Type x Time interaction, $F(1, 95) = 53.15\text{-}72.23$, *p*s < .001, ηp^2s = .359-.432: participants said "Old" for fewer Old-Negative photos at T2 than T1 (*p*s < .001), but there was no difference over time for New-Negative photos (*p*s = .067-.142). In other words, regardless of whether the photos were described or not, participant's recognition of New-Negative photos remained similar to baseline. Indeed, when we compared memory accuracy on described vs. undescribed photos

at T2, we only found a main effect of Photo Type (participants said "Old" for more Old vs. New photos; ($F(1, 95) = 2020.91$, $p < .001$, $\eta p^2 = .955$). There was no main effect of PEI ($F(1, 95) = .11$, $p = .747$, $\eta p^2 = .001$), or a Photo Type x PEI interaction, $F(1, 95) = 1.03$, $p = .314$, $\eta p^2 = .011$, indicating that participants were now similarly accurate in recognizing described and undescribed New-Negative photos as New. A paired samples Bayesian t-test with default Cauchy prior (Rouder, Speckman, Sun, Morey, & Iverson, 2009) revealed a BF_{01}=7.65, indicating substantial evidence of a negligible difference (Jeffreys, 1961; see Wetzels et al., 2011).

In Experiment 1c, we aimed to examine whether attempts at increasing discrepancy detection would still reduce memory errors after longer delays, in this case 1-week. For undescribed photos, participants said "Old" for fewer Old-Negative photos at T2 than T1 ($p < .001$), but there was no difference over time for New-Negative photos ($p = .225$, a Photo Type x Time interaction, $F(1, 90) = 191.72$, $p < .001$, $\eta p^2 = .681$). However, for described photos, participants said "Old" for fewer Old-Negative photos at T2 than T1 ($p < .001$) and said "Old" for more New-Negative photos at T2 than T1 ($p = .014$), similar to Experiment 1a (a Photo Type x Time interaction, $F(1, 90) = 203.50$, $p < .001$, $\eta p^2 = .693$). When we compared memory accuracy on described vs. undescribed photos at T2, we only found a main effect of Photo Type (participants said "Old" for more Old-Negative vs. New-Negative photos), similar to Experiment 1b, $F(1, 90) = 1511.75$, $p < .001$, $\eta p^2 = .944$. There was no main effect of PEI ($F(1, 90) = 1.61$, $p = .208$, $\eta p^2 = .018$) or a Photo Type x PEI interaction, $F(1, 90) = 1.96$, $p = .165$, $\eta p^2 = .021$. Interestingly, pairwise comparisons suggested that while there was no difference in accuracy between described vs. undescribed Old-Negative photos ($p = 1.000$), participants made more errors for described—wrongly identifying 12% of them as Old—compared to undescribed New-Negative photos (10%) at T2 despite our attempts to increase discrepancy detection, $p = .020$. Given the lack of a significant

interaction, however, this difference needs to be interpreted with caution. Further, a Bayes t-test comparing the two means revealed a BF_{01} = 3.55, indicating substantial evidence of a negligible difference between the means (Jeffreys, 1961; see Wetzels et al., 2011). Furthermore, the difference and effect size between errors for described and undescribed New-Negative photos is very small.[23]

Because Experiments 1a-1c were run successively, we were unable to compare them in a single analysis. Therefore, we ran a fourth experiment, randomizing participants into our delay and warning conditions, to determine whether increasing discrepancy detection decreases memory errors compared to when discrepancy detection is not manipulated. In Experiment 2, we also asked participants to determine the source of each photo (whether they: saw the photo at T1, read the description at T2, both, neither or unsure) after making their Old/New judgment. We added this source monitoring test for two reasons. First, the purpose of our experiments is to determine whether memory amplification is specifically caused by source monitoring failures. Thus, to justify our claim that memory errors participants made in Experiments 1a-c were caused by source monitoring failures and that increasing source monitoring ability decreased errors, we needed to measure source judgments. Second, eyewitnesses are often required to evaluate their memory to provide further detail beyond a simple Old/New type judgment Therefore, we were interested in how participants would perform on the memory test if they were forced to evaluate their memory more carefully. Unlike in recognition tests, which may be answered according to how familiar each item is (e.g., Old for all familiar items), a source monitoring test requires people to evaluate their memory in terms of specific sources for each item. This evaluation likely increases discrepancy detection and results in fewer errors overall (see Henkel, 2004; Lindsay

[23] See Supplementary Data for signal detection analyses and results for memory for descriptions compared to photos, and confidence ratings.

& Johnson, 1989; Zaragoza & Koshmider, 1989; Zaragoza & Lane, 1994). Therefore, we were able to determine whether PEI leads to more memory errors despite a source monitoring test.

We also measured PTSD symptoms at T1 as well as T2 because people often mistakenly use their current state to judge past states (Harvey & Bryant, 2000). Therefore, if participants experienced no symptoms when completing the PCL at T2, they may have underestimated any symptoms they experienced during the delay. Measuring symptoms at T1 allowed us to investigate whether immediate symptomology has a later effect on memory. We omitted five items because they did not make sense at T1 (e.g., "repeated, disturbing dreams or nightmares"). Therefore, the scale consisted of 12 items at T1.

Experiment 2

As stated above, in this experiment we combined our manipulations into a single experiment, randomizing participants into our delay and warning conditions. At T2, all participants were asked to indicate the source of each photo (whether they: saw the photo at Time 1 [T1], read the description at Time 2 [T2], both, neither or unsure) after making their Old/New judgment. This experiment was pre-registered (https://osf.io/xubsk/). All analyses below, except those described in the "Emotional Impact of Photos and Descriptions" (which are not part of the primary hypothesis testing) sections were pre-registered. Further, instead of running follow-up one-way analyses for significant interactions, we simply examined Bonferroni-corrected pairwise comparisons.

Method

Participants. An a priori repeated measures, within-between interaction power analysis (f = .25, alpha = .05, power = .95, number of groups = 4, number of measurements = 2, *r* among rep measures = .672 (based on above data), nonsphericity correction = 1) indicated that we needed a total sample size of 52 participants. We then ran an a priori

matched pairs power analysis (to ensure we have enough power to find an effect of PEI for New photos in no warning conditions)—*dz*=.45 (based on previous data), alpha = .05, power = .95—which indicated that we should have a total sample size of 68 participants. Therefore, we decided to recruit at least 68 participants per group (approximately 272 participants in total).

Four hundred and twenty-six participants from Amazon Mechanical Turk—restricted to US participants with an approval rating of at least 95%—completed the experiment. We excluded 150 participants: 89 admitted to looking away from the photos during the tests, 45 failed to accurately complete instructional manipulation checks (Oppenheimer, Meyvis, & Davidenko, 2009), 10 completed T2 beyond the 24-hour deadline, four had technical issues, one restarted one of the surveys, and one was accidentally sent the wrong version at T2. Of the remaining 276 participants, 168 were female, age ranged from 20 to 73, M = 37.66, SD=10.14, 76.4% were White/Caucasian, 6.9% Black, 6.5% Hispanic, 6.5% mixed, 1.8% Latino, and 1.8% Asian. The mean delay period between T1 and T2 was 1.22 days (SD = .26) for the 24-hour delay conditions and 7.32 day (SD = .34) for the 1-week delay conditions. Participants received US$5.00 for their time.

Design and Procedure. This experiment was a 2 (Time: T1, T2; within subjects) × 3 (Photo Type: Old-Negative, New-Negative, New-Neutral filler photos; within subjects) × 2 (PEI: described, undescribed; within subjects) × 2 (Delay: 24 hours, 1 week; between subjects) × 2 (Warning: warning, no warning; between subjects) design. Participants in the warning conditions judged if each description was Old or New (subtly warning them that some were New) while participants in the no warning condition simply rated the emotional arousal of each description.

All procedures were approved by the Social and Behavioural Research Ethics Committee at Flinders University. Participants first provided informed consent. During the

initial photo presentation phase, participants completed the PANAS and saw 48 negative IAPS photos presented for 2.5 seconds each. They rated how emotionally arousing the photos were after each photo before completing the PANAS again and the photo ratings (how unpleasant, distressing and disgusting the photos were overall). After a 20-minute Sudoku filler task, participants completed Test 1 (see Supplementary Data at the end of the chapter for confidence ratings) and the PCL.[24] Photos shown in Test 1 are never shown again. After a 24-hour or 1-week delay, participants completed the PANAS before reading descriptions of some Old-Negative and New-Negative photos. Participants in the no warning conditions rated how emotionally arousing the descriptions were after each description; participants in the warning conditions were instead asked to judge if each description was Old or New. After completing the PANAS again, and the description ratings (how unpleasant, distressing and disgusting the descriptions were overall), there was a brief 5-minute Sudoku filler task. Participants then completed the Test 2 memory task where they had to make an Old/New judgment and a source judgment for each photo and the PCL. Last, they were debriefed and paid for their participation.

Results

Emotional Impact of Photos and Descriptions. We first investigated whether our photos and descriptions acted as an analogue for trauma and trauma-related PEI (Table 1). Participants reported a decrease in positive mood and an increase in negative mood after viewing the photos and reading the descriptions. They rated the photos as very unpleasant, distressing, disgusting, and moderately emotionally arousing, and the descriptions as somewhat unpleasant, distressing, disgusting, and moderately emotionally arousing. The

[24] The scale consisted of 12 items at T1 because five items did not make sense at T1 (e.g., "repeated, disturbing dreams or nightmares").

photos also produced some analogue symptoms among participants across all symptom clusters (re-experiencing, avoidance, and hyperarousal).

Memory for Photos. Using repeated measures ANOVAs (Table 3), we examined participants' memory accuracy on New-Neutral fillers separately from Old-Negative and New-Negative photos (because these fillers were not described/undescribed and, therefore, could not be compared with the other Photo Types). Although errors on New-Neutral fillers were rare, participants were significantly less likely to call New-Neutral fillers Old at T1 compared to T2, $F(1, 272) = 38.51$, $p < .001$, $\eta_p^2 = .12$. There were no main effects of delay or warning or interactions.

Because the two tests comprised different items, all of which were undescribed at T1, whereas half were described and half undescribed at T2, it was again impossible to run a single full factorial analysis with all five independent variables (Time, Photo Type, Warning, Delay, PEI) included. Therefore, we ran multiple repeated measures ANOVAs to analyze our data. First, we ran two separate 2 (Time: T1, T2) × 2 (Photo Type: Old-Negative, New-Negative) × 2 (Warning: warning, no warning) × 2 (Delay: 24 hours, 1 week) repeated measures ANOVAs, one where photos were described (PEI) at T2, and the other where photos were undescribed (no PEI) at T2. We discuss these two analyses together for clarity of interpretation. For both described and undescribed photos, participants responded "Old" to New-Negative photos less than for Old-Negative photos, a large effect of Photo Type, Fs = 4870.48-5437.55, ps < .001, η_p^2s = .95. They responded "Old" less at T2 compared to T1, a large main effect of time, Fs = 111.96-114.58, ps < .001, η_p^2 = .29-30. These patterns were consistent with Experiments 1a-1c.

Table 4

Source monitoring judgments at T2 (correct response bolded) in Experiment 2

	Neutral	Old		New	
	Undescribed M (SD)	Described M (SD)	Undescribed M (SD)	Described M (SD)	Undescribed M (SD)
24-hour delay/No Warning					
Saw photo at T1	.04 (.09)	.27 (.23)	**.64 (.19)**	.06 (.13)	.09 (.14)
Read description at T2	.01 (.03)	.13 (.12)	.06 (.08)	**.55 (.22)**	.19 (.15)
Saw both photo and description	.01 (.03)	**.52 (.27)**	.17 (.16)	.06 (.09)	.03 (.05)
Neither	**.93 (.15)**	.06 (.08)	.09 (.10)	.28 (.19)	**.58 (.21)**
Not sure	.02 (.06)	.03 (.05)	.04 (.08)	.06 (.09)	.10 (.14)
24-hour delay/Warning					
Saw photo at T1	.04 (.08)	.25 (.18)	**.66 (.16)**	.04 (.06)	.08 (.09)
Read description at T2	.01 (.02)	.11 (.10)	.05 (.06)	**.56 (.19)**	.13 (.09)
Saw both photo and description	.00 (.00)	**.57 (.22)**	.15 (.09)	.06 (.08)	.02 (.04)
Neither	**.93 (.11)**	.05 (.07)	.10 (.11)	.27 (.16)	**.68 (.19)**
Not sure	.02 (.05)	.02 (.06)	.04 (.08)	.06 (.10)	.08 (.13)
1-week delay/No Warning					
Saw photo at T1	.04 (.07)	.25 (.18)	**.50 (.21)**	.04 (.07)	.08 (.09)
Read description at T2	.01 (.03)	.24 (.18)	.09 (.10)	**.52 (.26)**	.21 (.16)
Saw both photo and description	.01 (.05)	**.35 (.26)**	.13 (.12)	.07 (.10)	.03 (.06)
Neither	**.90 (.17)**	.11 (.16)	.20 (.18)	.26 (.23)	**.55 (.24)**
Not sure	.04 (.12)	.07 (.11)	.08 (.12)	.11 (.16)	.14 (.17)
1-week/Warning					
Saw photo at T1	.03 (.05)	.21 (.18)	**.51 (.22)**	.03 (.05)	.07 (.09)
Read description at T2	.004 (.02)	.27 (.18)	.10 (.10)	**.58 (.21)**	.22 (.17)
Saw both photo and description	.003 (.01)	**.37 (.21)**	.13 (.12)	.07 (.10)	.02 (.05)
Neither	**.93 (.14)**	.09 (.13)	.19 (.17)	.25 (.20)	**.60 (.23)**
Not sure	.04 (.12)	.05 (.08)	.07 (.11)	.07 (.12)	.09 (.13)

For both described and undescribed photos, we found Time × Photo Type × Delay interactions, *F*s = 41.14-41.38, *p*s = .001, η_p^2s = .13; as well as Time × Delay, Photo Type × Delay, and Time × Photo Type interactions, *F*s = 18.79-456.53, *p*s < .001, η_p^2 = .07-.63. There were no significant effects involving Warning. Unsurprisingly, the delay between T1 and T2 did not affect memory accuracy at T1 for Old-Negative (*p*s = .425, *d* = 0.10) or New-Negative photos (*p*s = .402, *d* = 0.10). However, participants in the 1-week delay condition were more inaccurate for Old-Negative photos at T2 compared to participants in the 24-hours conditions (*p*s < .001, *d*s = 0.93-0.97). In contrast, there was no difference for New-Negative photos at T2 according to delay (*p*s = .341-.659, *d*s = 0.06-0.12). Participants in both delay conditions became more inaccurate over time on Old-Negative photos (*p*s < .001, *d*s = 0.94-2.02). Participants in the 24-hour conditions (but not 1-week conditions; *p*s = .230-.359, *d*s = 0.10-0.13) became more inaccurate over time on New-Negative photos (*p*s = .001-.015, *d*s = 0.27-0.35). This difference is unintuitive and was unexpected given errors were so low across conditions and time, and thus it should be interpreted with caution. Participants in both delay conditions claimed that Old-Negative photos were "Old" more often than New-Negative photos at both times (*p*s < .001). In other words, participants in the longer (vs shorter) delay conditions were less likely to accurately respond "Old" for Old-Negative photos at T2; however, delay-length had no effect on memory for New-Negative photos, likely because errors for these photos were at floor.

To specifically compare memory accuracy on described vs. undescribed photos at T2, we then focused on memory accuracy at T2 using a 2 (Photo Type: Old-Negative, New-Negative) × 2 (PEI: described, undescribed) × 2 (Warning: warning, no warning) × 2 (Delay: 24 hours, 1 week) repeated measures ANOVA. Participants judged Old-Negative as Old more often than New-Negative photos, a main effect of Photo Type, $F(1, 272) = 2683.11$, $p < .001$, $\eta_p^2 = .91$. We found a Photo Type × Delay interaction that mirrored the patterns in our

previous analyses: participants in the 1-week conditions were less accurate than participants in the 24-hour conditions for Old-Negative photos ($p < .001$, $d = 0.95$); participants in both delay conditions were equally accurate for New-Negative photos ($p = .437$, $d = 0.09$). We also found a Photo Type × PEI interaction: participants were less accurate for described Old-Negative photos compared to undescribed Old-Negative photos ($p = .006$, $d = 0.13$); they were equally accurate for described and undescribed New-Negative photos, that is, there was no effect of PEI ($p = .091$, $d = 0.08$), $F(1, 272) = 10.77$, $p = .001$, $\eta_p^2 = .04$. It is possible, therefore, that asking participants to evaluate the source of each photo (the biggest change from Experiments 1a-1c) increased their discrepancy detection, making them resistant to inaccurate PEI.

Source monitoring for Photos. We ran a repeated measures ANOVA to examine participants' source judgments for photos at T2 (regardless of how they responded for the Old/New question; Table 4). Participants selected one of five potential source monitoring (SM) response options for each photo: I saw the photo yesterday, I read the description today, I both saw the photo and read the description, I neither saw the photo nor read the description, Not sure.

We found multiple four-way and three-way interactions with our Photo Type, PEI, and Delay variables.[25] We examined 2 (Photo Type, PEI, Delay) × 4 (SM response) interactions to understand these complex higher-order interactions (see Cohen, 2008). Overall, our results suggest that participants forgot reading some descriptions (or PEI) of Old- and New-Negative photos and forgot seeing some Old-Negative photos. Further, there

[25] We found a Photo Type × PEI × Delay × SM response interaction, $F(3, 816) = 24.03$, $p < .001$, $\eta_p^2 = .08$; and a Photo Type × PEI × Warning × SM Response interaction, $F(3, 816) = 3.03$, $p = .029$, $\eta_p^2 = .01$. We found Photo Type × Delay × SM response and PEI × Delay × SM response, Photo Type × PEI × Warning, PEI × Warning × SM response, Photo Type × PEI × SM response interactions (Fs = 4.25-385.62, ps < .001-.040), but they are likely explained by the 4-way interactions. There was no Photo type × PEI, Photo Type × Delay, PEI × Delay, Warning × Photo Type, Warning × PEI, or Warning by SM Response interactions, so pairwise comparisons were not examined.

were indications that source memory was more accurate for New- vs Old-Negative photos (please see Supplementary Data at the end of this chapter for full write up of source monitoring results).

Overall, the data on people's source judgments suggest that people forgot seeing some Old photos. They responded Description Only for about 19% of described Old-Negative photos, suggesting that they sometimes believed they had seen a description of the photo, but not both the photo *and* description. This pattern may explain why people had worse memory for Old photos for which they received PEI compared to Old photos without PEI. For New-Negative photos, when the photos had been accompanied by PEI (described), people attributed more of their false alarms to Both seen and described than to Photo Only (seen), whereas without PEI (undescribed) people attributed false alarms more often to Photo Only. However, it's important to note that these comparison means were very small (.026-.078). Still, it is interesting that people made any mistakes about source at all given that keeping photos and written descriptions distinct should be a relatively straightforward source judgment, particularly if participants are not asked to imagine the descriptions.

Analogue PTSD Symptoms and Memory for Photos. Incorrectly identifying described and undescribed New-Negative photos as Old was correlated with total and subscale analogue PTSD symptoms (see Table 5). Worsening symptoms (T2 total symptoms minus T1 symptoms with positive scores indicating increased symptomatology) was positively correlated with saying Old for New-Negative photos at T1 (r = .13, p = .03) and described New-Negative photos at T2 (r = .16, p = .01). For source judgments, increased symptoms were positively correlated with inaccurately claiming to see described New-Negative photos at T1 and corresponding descriptions at T2 (r = .12, p = .05) or neither (r = .15, p = .02). Furthermore, increased symptoms were negatively correlated with accurately judging that descriptions of described New-Negative photos at T2 as seen (r = -.18, p = .002).

These findings suggest that experiencing increased PTSD symptomatology is related to memory amplification. However, it is important to note that these effects are small.

Table 5

Correlations between trauma symptoms at T2 and memory errors in Experiments 1b-2

	Total PCL	Re-experiencing	Avoidance	Hyperarousal
Experiment 1b				
Memory errors for New-Negative descriptions	.27**	.29**	.18	.22*
Memory errors for described New-Negative photos (T2)	.46***	.24*	.49***	.40***
Memory errors for undescribed New-Negative photos (T2)	.43***	.14	.48***	.43***
Correct identification of descriptions and corresponding photos (T2)	-.26*	-.21*	-.21*	-.23*
Incorrect identification of descriptions and matching photos (T2)	.29**	.13	.23*	.32**
Experiment 1c				
Memory errors for New-Negative descriptions	.21*	.16	.24*	.20
Memory errors for described New-Negative photos (T2)	.10	.02	.13	.13
Memory errors for undescribed New-Negative photos (T2)	. 21*	.15	.23*	.21*
Correct identification of descriptions and corresponding photos (T2)	-.28**	-.28**	-.25*	-.27*
Incorrect identification of descriptions and matching photos (T2)	.02	-.01	.03	.03
Experiment 2				
Memory errors for New-Negative photos (T1)	.32***	.25**	.33***	.30***
Memory errors for New-Negative descriptions	.02	.02	.04	-.01
Memory errors for described New-Negative photos (T2)	.25***	.27***	.23***	.19**
Memory errors for undescribed New-Negative photos (T2)	.23***	.25***	.22***	.16**
Correct identification of descriptions and corresponding photos (T2)	.08	.08	.08	.05
Incorrect identification of descriptions and matching photos (T2)	.01	-.01	.03	.002

Note. * = $p<.05$, ** = $p<.01$, *** = $p<.001$

Discussion

Overall, our results confirm that memories for potentially traumatic stimuli are malleable. Source monitoring errors appear to be a key mechanism underlying memory amplification. In Experiment 1a, participants falsely remembered 20% more photos after PEI exposure, suggesting their memory expanded to include that PEI. However, similar to Strange and Takarangi (2015), subtle warnings after a short (but not long) delay in Experiment 2 appeared to increase participants' source monitoring ability and resistance to inaccurate PEI. However, we found no significant effect of warning in Experiment 2, suggesting that further research on how to improve source monitoring ability (e.g., via explicit and/or repeated warnings) is necessary.

In Experiment 2, participants did not make more errors for described compared to undescribed New-Negative photos, a result possibly prompted by the source monitoring test (see Henkel, 2004; Lindsay & Johnson, 1989). Consistent with this possibility, when participants in Intraub and Hoffman (1992) read paragraphs describing Old and New slide scenes, at test they often claimed that the corresponding slides of paragraphs they only read about were Old. When Dodson and Johnson (1993) replicated the study but compared an Old/New decision with a source monitoring test, participants made fewer source errors in the source monitoring than Old/New condition, likely because source judgments rely on more specific details of the memory rather than a single feature like overall familiarity (see Lindsay & Johnson, 1989). Therefore, our use of a source monitoring test likely led to no difference in errors between described and undescribed New-Negative photos, and the low level of errors for New-Negative photos in general. However, we note that future research needs to compare Old/New and source monitoring test conditions to make a firm conclusion.

Unexpectedly, participants made more errors for described compared to undescribed Old-Negative photos. The source monitoring test data suggest that some of these errors

occurred when participants forgot having seen the old photos and instead attributed old items to only having read a description. Alternatively, Echterhoff, Groll, and Hirst (2007) found that warned (vs. not warned) participants recognized fewer items at test that were truthfully described in a post-event narrative. Therefore, warnings may lead people to believe that *all* information contained in a post-event source is unreliable and, thus, they ignore such information at test. The source monitoring test may have acted essentially as an additional warning (albeit a subtle one), separate to our warning manipulation, potentially resulting in this "tainted truth" effect. However, the mean differences between described and undescribed Old-Negative photos were very small and, therefore, require further research.

Similar to field research, we found a positive relationship between PTSD symptoms and source monitoring errors. This finding is consistent with Rubin's memory-based PTSD model (Rubin, Bernsten, and Bohni, 2008), which argues that PTSD symptoms derive from the trauma memory rather than the traumatic event itself. People may amplify their memory to justify or make sense of their symptoms (Engelhard et al., 2008). Furthermore, PTSD symptom sufferers may ruminate on, or attempt, but fail, to suppress thoughts about PEI, providing more opportunity for source monitoring errors. Future studies should investigate these potential mechanisms.

Our study has some limitations. Our trauma analogue may not have replicated the stress and emotionality experienced during a trauma event. However, IAPS pictures elicit fear-related physiological responses (e.g., Litz, Orsillo, Kaloupek, & Weathers, 2000; Smith, Bradley, & Lang, 2005) and behavioral responses (e.g., Bywaters, Andrade, & Turpin, 2004). Perhaps the photos were not consolidated in memory after 20 minutes (see Dudai, 2004). Future research should manipulate the delay between initial photo presentation and Test 1 to investigate memory amplification for consolidated versus non-consolidated memories. Having photo rather than text test items is not ecologically valid; it is unlikely witnesses

would be re-exposed to trauma during questioning. That said, people likely try to bring forth an image of the situation they are trying to recall, especially when instructed to remember what happened. Furthermore, photo tests—along with text PEI—may have affected our memory distortion rates (see Takarangi, Oulton, & Strange, 2017). Researchers could also test neutral and positive stimuli to see if PEI influences information with differing valence. Lastly, there was a relatively high number of exclusions due to attention check failures and participants looking away at test. However, this means we only included those who were properly engaged in the experiment. Further, Amazon Mechanical Turk participants are more attentive to attention instructions than undergraduate subjects, suggesting the failure rate would have been even higher if the experiments had been conducted in the laboratory (Hauser & Schwarz, 2016). It is also not surprising that a large number of participants looked away at test given the graphic and explicit nature of the photo stimuli.

Despite these limitations, our findings have important theoretical implications. Our data are the first to suggest that memory amplification could reflect source monitoring errors due to PEI. Our results also have important real-world implications regarding the accuracy of eyewitness testimony. In particular, trauma reports (or at least *some aspects* of these reports) may not always be a reliable indicator of actual trauma exposure, especially if exposure to PEI is likely.

Supplementary Data

Experiments 1a-c

Design and Procedure. The study was entirely within-subjects and comprised of four phases across two separate sessions. Time 1 included the initial photo presentation phase and the first recognition or baseline memory test phase (Test 1). Time 2, which was emailed out after a delay, included the PEI phase and the final memory test phase (Test 2). Participants had 24-hours to complete Time 2 once they received the email. During the initial photo presentation phase, participants in all three experiments completed the PANAS and saw 48 IAPS photos presented for 2.5 seconds each. They rated how emotionally arousing the photos were after each photo. After the photo presentation, participants completed the PANAS again and the photo ratings (how unpleasant, distressing and disgusting the photos were overall). After a 20-minute Sudoku filler task, participants completed Test 1. After a 24-hour (Experiment 1a and Experiment 1b) or 1-week (Experiment 1c) delay, participants completed the PANAS before reading descriptions of some Old-Negative and New-Negative photos. In Experiment 1a, they rated how emotionally arousing the descriptions were after each description. In Experiments 1b and 1c, they were instead asked to judge if each description was Old (i.e., they had seen a photo of the description in the previous session) or New to increase their discrepancy detection. After completing the PANAS again and the description ratings (how unpleasant, distressing and disgusting the descriptions were overall), there was a brief 5-minute Sudoku filler task. We then administered the Test 2 memory task and the PCL. Finally, we debriefed and paid participants for their participation.

Results. Below are the supplementary results for Experiments 1a-c.

Memory for New-Neutral filler photos. Although participants were significantly better at rejecting New-Neutral fillers as Old at T1 compared to T2, means were at floor in all three experiments, *p*s < .001.005-.03, *d* = .21-72. This finding is unsurprising given that, to

reject the New-Neutral filler photos, participants only had to remember that photos in the initial photo presentation stage were all negative.

Signal detection analyses. In Experiment 1a, we hypothesized that participants would have more difficulty discriminating between Old and New photos (i.e., have decreased sensitivity) and have an increased tendency to respond "Old" for photos with PEI compared to no PEI (see Table 1 for all signal detection analyses). Sensitivity (*d*') was significantly higher at T1 compared to T2 for both described photos ($t(92) = 10.73$, $p < .001$, $d = -1.09$ [-1.36, .84]) and undescribed photos, $t(92) = 8.33$, $p < .001$, $d = -.79$ [-1.01, -.57]. These data suggest that regardless of whether participants had seen a description of the photo, they found it harder to differentiate Old and New photos at T2 compared to T1. However, at T2, *d*' was significantly lower for described photos compared to undescribed photos, $t(92) = -4.68$, $p < .001$, $d = -.34$ [-.50, -.19]. Put differently, participants found it more difficult to differentiate Old and New photos when they were exposed to PEI relating to those photos.

Measures of response bias that are calculated using both hit and false alarm rates (e.g., criterion c are problematic after a delay period because hit rates may decrease over time due to forgetting in *addition* to any changes in response bias (see Huff, Bodner, & Fawcett, 2015). This decrease could mask increases in false alarm rates. Lambda (λ) is a bias measure that is calculated using the false alarm rate only (see Wickens, 2002).[26] Higher values reflect a more conservative response bias. When we examined our data using λ, we found, as expected, that participants were less conservative in their responses (i.e., less likely to respond "New") to described photos than undescribed photos at T2, ($t(92) = -4.56$, $p < .001$, $d = .40$ [.22, .59]), suggesting that participants incorporated PEI into the original trauma memory. Both means were lower compared to T1: Described: $t(92) = 5.49$, $p < .000$, $d = -.71$

[26] We thank an anonymous reviewer for this suggestion.

[-.99, -.44]; Undescribed, $t(92) = 2.70$, $p = .008$, $d = -.35$ [-.61, -.09].

Table 1

Signal detection analyses for Experiment 1a-2

	Sensitivity (d')			Response bias (λ)		
	T1 *M* (*SD*)	T2 Described *M* (*SD*)	T2 Undescribed *M* (*SD*)	T1 *M* (*SD*)	T2 Described *M* (*SD*)	T2 Undescribed *M* (*SD*)
Experiment 1a	3.01 (.76)	2.05 (.97)	2.36 (.86)	1.47 (.41)	1.02 (.78)	1.29 (.56)
Experiment 1b	3.11 (.61)	2.38 (.70)	2.45 (.68)	1.46 (.43)	1.33 (.51)	1.35 (.52)
Experiment 1c	3.15 (.52)	2.00 (.70)	2.09 (.57)	1.46 (.48)	1.26 (.50)	1.37 (.48)
Experiment 2						
24-hour delay/No Warning	2.97 (.78)	2.14 (.88)	2.40 (.78)	1.40 (.57)	1.21 (.74)	1.33 (.52)
24-hour delay/Warning	3.15 (.60)	2.35 (.78)	2.40 (.79)	1.53 (.39)	1.30 (.53)	1.31 (.48)
1-week delay/No Warning	2.98 (.80)	1.66 (.78)	1.71 (.77)	1.35 (.63)	1.28 (.61)	1.24 (.55)
1-week/Warning	3.16 (.77)	1.68 (.69)	1.87 (.65)	1.51 (.61)	1.33 (.51)	1.40 (.46)

Similar to Experiment 1a, *d*' was significantly higher at T1 compared to both described ($t(95) = 9.31$, $p < .001$, $d = -1.11$ [-1.39, -.83]) and undescribed photos ($t(95) = 7.76$, $p<.001$, $d=-1.01$ [-1.32, .72]) at T2. Unlike in Experiment 1a, however, *d*' was now almost the same for described and undescribed photos at T2 ($t(95) = -1.03$, $p=.308$, $d=.05$ [-.14, .23]); i.e., there was no significance between the two means and the effect size was very small. A paired samples Bayesian t-test with default Cauchy prior revealed a $BF_{01}=5.32$, indicating substantial evidence of a negligible difference between the means (Jeffreys, 1961; see Wetzels et al., 2011). Therefore, simply asking participants if they had seen photos of the descriptions made differentiating between Old and New photos immediately afterwards easier.

Unlike Experiment 1a, there was a very small effect size and no significant difference in response bias for described and undescribed photos at T2 ($t(95) = -.50$, $p = .621$, $d = .05$ [-.14, .23]), suggesting that participants did not incorporate the PEI into their original trauma memory. A paired samples Bayesian t-test with default Cauchy prior revealed a BF_{01}=8.40, indicating substantial evidence of a negligible difference between the means (Jeffreys, 1961; see Wetzels et al., 2011). Furthermore, although participants were less conservative for described photos at T2 compared to T1 ($t(95) = -23.16$, $p < .001$, $d = -.27$ [-.55, -.002]—suggesting that people may still have incorporated PEI into their memory—they now exhibited no significant difference in response bias for undescribed photos between T1 and at T2, $t(95) = 1.58$, $p = .118$ $d = -.22$ [-.51, .06]. This finding indicates that increasing discrepancy detection led to a more consistent response criterion over the delay period.

In Experiment 1c, at T1, *d*' was significantly higher compared to T2 for both described photos ($t(90) = 15.80$, $p < .001$, $d = -1.83$ [-2.19, -1.49]) and undescribed photos, $t(90) = 14.80$, $p < .001$, $d = -1.91$ [-2.30, -1.55]. Similar to Experiment 1b, *d*' at T2 was the same for described and undescribed photos, $t(90) = -1.23$, $p = .224$, $d = .13$ [-.08, .34]. Therefore, asking participants if they had seen photos of the descriptions made it easier for them to differentiate Old and New photos even with a week delay since the time of encoding.

Unlike Experiment 1b and similar to Experiment 1a, participants were less likely to respond "New" for described compared to undescribed photos at T2 ($t(90) = -2.40$, $p = .019$, $d = 0.23$ [.04, .42]), indicating that participants incorporated PEI into their original memory. Similar to Experiment 1b, participants were less conservative for described photos at T2 compared to T1 ($t(90) = 3.30$, $p = .001$, $d = -.40$ [-.65, -.16]), but exhibited no significant difference in response bias between T1 and undescribed photos at T2, $t(90) = 1.41$, $p = .161$.

Table 2

Memory for descriptions and photos for Experiments 1b-c and warning conditions in Experiment 2

	Experiment 1b *M* (SD)	Experiment 1c *M* (SD)	Experiment 2 Warning conditions M (SD)
Correctly identified Old-Negative description and photo	.65 (.16)	.52 (.17)	.56 (.21)
Correctly identified Old-Negative description, incorrectly identified photo	.09 (.09)	.12 (.11)	.14 (.12)
Incorrectly identified Old-Negative description, correctly identified photo	.17 (.12)	.22 (.13)	.16 (.11)
Incorrectly identified Old-Negative description and photo	.08 (.09)	.14 (.12)	.14 (.13)
Correctly identified New-Negative description and photo	.60 (.19)	.55 (.18)	.56 (.17)
Correctly identified New-Negative description, incorrectly identified photo	.04 (.06)	.05 (.06)	.03 (.05)
Incorrectly identified New-Negative description, correctly identified photo	.30 (.15)	.33 (.15)	.34 (.14)
Incorrectly identified New-Negative photo and description	.07 (.09)	.08 (.09)	.08 (.10)

Memory for descriptions and photos. In Experiments 1b and 1c, we asked participants during the PEI phase whether they had seen a photo of each description in T1. As expected, participants accurately recognized Old-Negative descriptions as Old (Experiment 1b: $M = .74$, $SD = .14$; Experiment 1c: $M = .64$, $SD = .16$). However, they frequently claimed that New-Negative descriptions were Old (Experiment 1b: $M = .37$, $SD = .18$; Experiment 1c: $M = .41$, $SD = .17$). When participants *correctly* identified a description as Old, they tended to correctly identify the corresponding photo as Old as well. When they *incorrectly* identified Old-Negative descriptions as New, they still sometimes correctly identified the corresponding photo as Old. Participants rarely incorrectly identified both the Old-Negative description and the corresponding photo. When participants correctly identified a description as New, they

also tended to correctly identify the corresponding photo as New. However, even when they had *incorrectly* identified the New-Negative description as Old, they still sometimes correctly identified the corresponding photo as New. Participants rarely incorrectly identified both the description and the photo (see Table 2).

Confidence in memory for photos. We also analyzed participants' confidence in their decisions for photos in all three experiments (see Table 3). At T1, within subjects t-tests revealed that participants were significantly less confident for New-Negative photos compared to Old-Negative photos (*p*s < .001, *ds* = -.89--.68, or New-Neutral fillers *ps* < .001, *ds* = -.96--.82. Confidence was higher for New-Neutral fillers than Old-Negative photos, *ps* = .04, *ds* = -.18-.19 in Experiment 1a and 1b; there was no significant difference in Experiment 1c, $t(90) = -.72$, $p = .474$, $d = -0.07$.

At T2, participants were equally confident for described and undescribed Old-Negative photos, *p* = .13-.70, *d* = -.08--.02. They were also equally confident for described and undescribed New-Negative photos, *p* =.25-.54, *d* = .04-.05). Effect sizes were very small. Participants were more confident for photos they identified correctly, compared to photos they identified incorrectly *p* < .001, *d* = -.94--.77. In Experiment 1a and 1b, they were also more confident for correctly identified Old-Negative photos, than correctly identified New-Negative photos (*p* < .001, *d* = -.32-.39. They were equally confident for incorrectly identified Old-Negative and New-Negative photos, *p* = .08-.30, *d* = -.28--.20.[27] In Experiment 1c, participants were equally confident for correctly identified Old-Negative and New-Negative photos, $t(90) = 1.24$, $p < .218$, $d = -.11$. They were also more confident for incorrectly identified Old-Negative photos compared to incorrectly identified New-Negative photos, $t(77) = 2.24$, $p = .028$, $d = -.19$.

[27] *df*'s differ because some participants made no errors for photos and could not be included in paired t-test analyses investigating incorrect responses.

Table 2

Confidence in memory judgments for all experiments

	Preliminary Experiment 1			Preliminary Experiment 2			Preliminary Experiment 3			Current Study
	T1 *M* (*SD*)	T2 Described *M* (*SD*)	T2 Undescribed *M* (*SD*)	T1 *M* (*SD*)	T2 Described *M* (*SD*)	T2 Undescribed *M* (*SD*)	T1 *M* (*SD*)	T2 Described *M* (*SD*)	T2 Undescribed *M* (*SD*)	T1 *M* (*SD*)
New-Neutral filler	9.75 (.76)		9.28 (.89)	9.82 (.52)		9.27 (1.03)	9.91 (.28)		8.82 (1.20)	9.84 (.52)
Old-Negative	9.60 (.81)	8.88 (1.08)	8.80 (1.25)	9.72 (.57)	9.08 (1.12)	9.00 (1.10)	9.88 (.44)	8.37 (1.05)	8.34 (1.18)	9.77 (.54)
New-Negative	8.82 (1.39)	8.57 (1.12)	8.62 (1.27)	9.14 (.99)	8.67 (1.17)	8.73 (1.11)	9.08 (1.18)	8.32 (1.18)	8.35 (1.21)	8.99 (1.19)

Analogue PTSD symptoms and memory for photos. Because previous research has found a relationship between PTSD symptoms and memory amplification, as a secondary interest, we examined this relationship in our data. We correlated PTSD symptoms with Old responses to both described and undescribed New-Negative photos at T1 or T2. In Experiment 1a, there were no significant correlations. In Experiment 1b, erroneously identifying New-Negative descriptions as Old was related to total PCL scores and more symptoms across all subscales except avoidance. Correctly identifying *both* the description and the corresponding photo as Old or New was associated with fewer symptoms, while incorrectly identifying both the description and photo was positively correlated with all symptoms except re-experiencing (see Table 5 in main text). By increasing participants' discrepancy detection, we may have separated out people who are less likely to amplify their memory (i.e., those who make correct decisions when discrepancy detection is improved). It is possible, therefore, that memory amplification is related to symptoms among people who have a strong tendency to amplify regardless of attempts to prevent it, perhaps because they use different strategies at test from everyone else (e.g., using their symptom severity to base their judgments).

Similarly, in Experiment 1c, incorrectly believing that New-Negative descriptions were Old was significantly correlated with total PCL scores and the avoidance subscale. Correctly identifying both the description and corresponding photo as Old or New was negatively correlated with symptoms. Unlike in Experiment 1b, symptoms were not related to wrongly identifying described New-Negative photos as Old. However, all symptoms except re-experiencing were still related to wrongly identifying undescribed New-Negative photos as Old.

Experiment 2

Signal Detection Analyses. For both described and undescribed photos, we found

time x delay interactions (*F*s = 38.69-36.96, *p*s < .001, η_P^2 = .12-.13): participants in both delay conditions exhibited higher sensitivity at T1 compared to T2, *t*s = 10.00-20.67, *p*s < .001; however, those in the 1-week conditions exhibited lower sensitivity at T2 compared to the 24-hour conditions, *t*s = 5.99-6.76, *p*s < .001. We also found main effects of time, with *d*' being significantly higher at T1 compared to T2, *F*(1, 272) = 414.72-537.97, *ps* < .001, η_P^2 s = .61-.66. There was also a main effect of delay condition with participants in the 24-hour conditions exhibiting higher sensitivity compared to the 1-week conditions, *F* = 12.70-17.53, *ps* < .001, η_P^2 s = .05-.06. At T2, *d*' was significantly lower for described photos compared to undescribed photos, *F*(1,271) = 7.79, *p* = .006, η_P^2 = .03. Therefore, participants found it more difficult to differentiate Old and New photos when they were exposed to corresponding PEI. We found an effect of delay, with participants in the 1-week conditions exhibiting lower sensitivity compared to the 24-hour conditions, *F*(1,271) = 55.24, *p* = .006, η_P^2 = .17.

Participants were less likely to respond "New" for described and undescribed photos at T2 compared to T1, *F*s = 11.58-15.45, *p* < .001-.001, η_P^2 = .04-.05. There were also effects of warning: participants in the 1-week (vs 24-hour) conditions were more likely to respond "New" for described for photos (*F*(1, 272) = 4.03, *p* =.046, η_P^2 =.02); participants in the 24-hour (vs 1-week) conditions were more likely to respond "New" for undescribed photos, *F*(1, 272) =4.18, *p*=.042, η_P^2 =.02. There were no main effects or interactions when comparing Lambda for described and undescribed photos at T2.

Source-monitoring for Photos. When SM response is entered as a dependent variable in the mixed ANOVA platform, SPSS considers the fifth response option as redundant because the proportion of responses given to this option is obvious based on proportions for the first four response options. Therefore, to allow SPSS to run our ANOVA, we excluded one response ("Not sure"). In other words, we ran a 2 (Photo Type: Old-

Negative, New-Negative) × (PEI: described, undescribed) × 4 (SM response: I saw the photo yesterday, I read the description today, I both saw the photo and read the description, I neither saw the photo nor read the description) × 2 (Warning: warning, no warning) × 2 (Delay: 24 hours, 1 week) analysis.

We found a Photo Type × SM response interaction, $F(2.75, 746.81) = 1091.93$, $p < .001$, $\eta_p^2 = .80$. The correct responses for undescribed and described *Old*-Negative photos are "Photo Only" and "Both" (both saw the photo and description) respectively. Therefore, if participants' source monitoring was perfect, they should provide these two responses for Old-Negative photos an equal number of times because half of the photos were undescribed and half were described. In contrast, participants judged that they only saw the photo (Photo Only) more than other responses (*p*s < .001, *d*s = 0.43-1.33), despite seeing descriptions for half the Old-Negative photos they saw. This result suggests that they forgot reading some corresponding descriptions for Old-Negative photos. However, they said they had seen the photo *and* read the description ("Both") more often than the remaining two, and completely inaccurate, options for Old-Negative photos (Description Only or neither saw the photo or description (Neither); *p*s < .001, *d*s = 0.82-0.92). Therefore, overall, their source memory for Old photos was relatively accurate. For *New*-Negative photos, the correct responses for undescribed and described New-Negative photos are "Neither" and "Description Only" respectively; therefore, each response should, once again, be given 50% of the time for New-Negative photos. In contrast, people said "Neither" more than the other response options (again suggesting that they forgot reading some descriptions; $p < .001$-$.002$, *d*s = 0.24-1.96), and said they read the Description Only more compared to the remaining two options (*p*s < .001, *d*s = 1.57-1.68). Participants responded Photo Only or Both more for Old-Negative photos compared to New-Negative; they responded Description Only or Neither more for New-Negative vs Old-Negative photos (*p*s < .001, *ds* = 1.13-1.78). Overall, our results

suggest that participants forgot reading some descriptions (or PEI) of Old- and New-Negative photos.

We also found a PEI × SM response interaction, $F(2.51, 682.51) = 713.07$, $p < .001$, $\eta_p^2 = .72$. The correct responses for *undescribed* photos are Photo Only (for Old-Negative photos) and Neither (for New-Negative photos). People responded Neither more than any other response, suggesting they forgot some Old-Negative photos when they didn't also see consistent PEI (*p*s < .001-.003, *d*s = 0.15-1.28). They responded Photo Only more than the remaining two options (*p*s < .001, *d*s = 1.05-1.28), and Description Only more than Both ($p < .001$, $d = 0.38$). The correct responses for *described* photos are Both (Old-Negative photos) and Description Only (New-Negative). Participants responded Description Only more than any other option (again suggesting they forgot some Old-Negative photos; *p*s < .001, *d*s = 0.40-0.99); they said Both more than the remaining two options (*p*s < .001, *d*s = 0.39-0.51). People said Neither and Photo Only more for undescribed than described photos. Finally, they said Both or Description Only more for described vs undescribed photos (*p*s < .001, *d*s = 0.75-1.74). Overall, our results indicate that participants forgot seeing some Old-Negative photos.

We found a Delay × SM response interaction, $F(3, 816) = 13.23$, $p < .001$, $\eta_p^2 = .05$. In the *24-hour delay* conditions, participants said Photo Only (correct response for undescribed Old-Negative photos) and Neither (correct response for undescribed New-Negative photos) more than Both (correct response for described Old-Negative photos; *p*s < .001, *d*s = 0.23-0.25), suggesting they forgot some descriptions for Old photos. In the 1-week conditions, participants responded Description Only (correct response for described New-Negative photos) or Neither (correct response for undescribed New-Negative photos) compared to the other two options ($p < .001$, *d*s = 0.28-0.60) suggesting they had better source memory for New- than Old-Negative photos; they said Photo Only more than Both,

again suggesting they may have forgotten some Old-Negative photo descriptions ($p < .001$, $d = 0.29$). People in the 24-hour delay conditions responded Photo Only or Both more than those in the 1-week conditions; those in the 1-week (vs 24-hour) conditions responded Description Only more (ps < .001, ds = 0.20-0.24). Therefore, our results suggest that participants forgot reading some Old-Negative photo descriptions, and that source memory was more accurate for New- vs Old-Negative photos.

Chapter Eight: When more is not merrier: Do shared stressful experiences amplify?

Author Contributions: Sasha Quayum developed the study concept and design (90% contribution), collected data, performed the data analysis and interpretation (100% contribution) and drafted the manuscript (85% contribution to the writing and editing). Georgia Dornin collected data simultaneously. Melanie Takarangi provided guidance during the design phase and made critical revisions to the manuscript. All authors approved the final version of the manuscript for submission.

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Chapter Nine: Does Perspective-taking Explain the Amplification of Shared Negative Experiences?[28]

Author Contributions: Sasha Quayum developed the study concept and design (90% contribution), collected data, performed the data analysis and interpretation (100% contribution) and drafted the manuscript (85% contribution to the writing and editing). Melanie Takarangi provided guidance during the design phase and made critical revisions to the manuscript. Both authors approved the final version of the manuscript for submission.

Abstract

Stressful experiences can amplify when they are shared with others even without any direct communication (Nahleen, Dornin, & Takarangi, under review; Martin et al., 2015). We aimed to test whether this effect occurs because of a facet of empathy called perspective-taking: thinking about how what others are thinking, feeling, and experiencing. It is possible that people construct mental representations of others' responses and spontaneously incorporate them into their own responses (Smith & Mackie, 2015), thus amplifying an experience (e.g., making it more stressful). Participants completed the Cold Pressor Task at the same time as a confederate after either being explicitly told to take the confederate's perspective (Shared Perspective-taking condition) or not (Shared Control condition), or while the confederate completed another task (Unshared condition). We assessed their reactions immediately after, and 24-hours after the task. We found few differences between groups suggesting that either our manipulation was not strong enough to increase ratings (e.g., perhaps it is too difficult to empathize when in pain) or that perspective-taking may not explain shared amplification after all. We did, however, find evidence that participants across conditions remembered some aspects of the task (e.g., mood) more negatively 24-hours after,

[28] Nahleen, S., Nixon, R. D. V., & Takarangi, M. K. T. (2018). *Sexual assault survivors report experiencing fewer trauma events over time.* Unpublished manuscript.

compared to immediately after, the task, a finding consistent with memory amplification research (e.g., Southwick et al., 1997).

Introduction

Experiences are amplified when shared with others, even when there is no direct communication (e.g., Boothby, Clark & Bargh, 2014). In Boothby et al. (2014), after building rapport, participants liked pleasant chocolate more and unpleasant, overly bitter chocolate less when a confederate ate the chocolate with them versus when the confederate completed another task. Importantly, while "shared amplification" may be beneficial or negligible in mild situations, research shows that it may be damaging in more stressful situations. In Martin et al. (2015), participant-pairs completed the Cold Pressor Task (CPT)—which involves immersing one's hand in cold water to induce pain and stress—once alone and once facing a partner who observed (unshared) or also immersed their hand (shared). Participants reported higher pain intensity when sharing the task versus alone, but only when the partner was familiar (a friend or stranger they bonded with before the task). Further, Nahleen et al. (under review) found that, after building rapport, participants who completed the CPT at the same time as a confederate (vs. when the confederate completed another task) experienced more sensory pain characteristics and more stress over time. It is not clear, however, *why* this shared amplification occurs. One possibility is that people construct mental representations of others' responses and spontaneously incorporate them into their own responses (Smith & Mackie, 2015), thus amplifying the task (e.g., its emotional intensity). In line with this possibility, in Nahleen et al., participants in the shared condition thought more about what the confederate was thinking and feeling than those in the unshared condition; further, this perspective-taking (or "mentalizing"; Boothby et al., 2014; Nahleen et al., under review) mediated the impact of sharing on a number of outcomes (e.g., certain pain ratings, perceived water intensity). In the present study we aimed to directly test the perspective-taking

explanation. Our second aim was to determine whether negative experiences still amplify when shared if there is no prior familiarity (i.e., rapport) between co-experiencers.

Past researchers suggest that an underlying mechanism to shared amplification may be empathy—a multidimensional construct comprised of cognitive (mentalizing) and affective (empathic concern: feelings of warmth and sympathy for others; and personal distress: discomfort when facing others' distress; Davis, 1983) components. Martin et al. (2015) suggested that reducing or blocking a person's own stress (e.g., pharmacologically or by facing a friend during the experience) would allow them to feel empathy towards others in pain and, thus, lead to shared amplification. Boothby, Smith, Clark, and Bargh (2016) argued that because people are more empathic with close (vs. non-close) others (e.g., Bouchard et al., 2013), they may believe others are having similar thoughts and feelings as them regarding a shared experience. This belief then makes it easier to construct and incorporate others' responses into their own (see Smith & Mackie, 2015). Indeed, thinking about another person's thoughts and feelings, a cognitive facet of empathy called perspective-taking (also mentalizing; see Nahleen et al., under review), may be related to shared amplification effects. Boothby et al. (2014) and Nahleen et al. (under review) both found that, after building rapport with a confederate, participants in the shared condition mentalized more during their task compared to participants in the unshared condition. These data support the idea that perspective-taking is one underlying mechanism of shared amplification. However, these studies did not specifically manipulate perspective-taking, making it impossible to determine causality.

Many other studies have investigated the effects of perspective-taking, including its ability to foster empathy for, or positive evaluations of, strangers in audio or video stimuli with no prior communication or opportunity to build rapport (e.g., Batson, Early, & Salvarani, 1997). For example, in Batson et al. (1997), participants listened to a taped

interview of a woman named Katie who discussed the negative consequences of her parents and sister's death in a car crash. Participants were told to either be as objective as possible while listening to the tape or try to imagine how Katie feels about what has happened (imagine-other perspective-taking) or how they themselves would feel in Katie's position (imagine-self perspective-taking). Participants in the perspective-taking conditions felt more empathy and distress for Katie than participants in the objective condition (see also e.g., Davis et al., 2004; Todd, Bodenhausen, Richeson, & Galinsky, 2011). Importantly, perspective-taking can foster connections even with outgroup members. In Müller et al. (2011), White participants were influenced (in the form of faster response times on certain trials in a go/no-go task) by the actions of an animated hand with white skin tone, but not by a hand with black skin tone. However, in a follow-up, White participants were influenced by the Black hand when they were instructed to read a story about a Black out-group member and take his perspective before the task. In other words, imagining what others are thinking and feeling can increase that person's influence on one's own responses to a task, even if the other person is unfamiliar.

Here, we specifically investigated whether perspective-taking, in the form of constructing others' psychological responses during the CPT, would amplify an experience, making it more negative. In Nahleen et al. (under review), participants completed the CPT concurrently with a confederate (Shared condition), or while the confederate completed another task in the same room (Unshared condition). They rated their reactions to the CPT on various measures immediately after the task, and again 24-hours later (to determine whether the experience amplified over time). Here, we replicated and extended Nahleen et al.'s paradigm by adding a third condition to investigate the effect of perspective-taking on shared experiences. Participants in the Unshared condition completed the CPT while the confederate read a calculator catalogue; those in the Shared Control and Shared Perspective-taking

conditions completed the CPT at the same time as a confederate. However, participants in the Shared Perspective-taking condition were explicitly instructed to mentalize during the task. Importantly, to *isolate* the effects of perspective-taking on shared amplification, we removed the rapport-building phase. If perspective-taking alone explains shared amplification, then we would expect participants in our Shared Perspective-taking condition to report higher ratings on our dependent variables compared to both the other conditions.

However, removing the rapport building phase could influence whether we find a difference between the Shared Control and the Unshared condition. One line of research suggests that it is crucial for the co-experiencers to be familiar with each other to find shared amplification effects. Boothby et al. (2016) found that participants who built rapport with the confederate prior to the chocolate eating task rated the chocolate as more enjoyable when the tasting experience was shared compared with when it was unshared; participants who did not build rapport rated the experience equally enjoyable regardless of whether it was shared or not. The researchers suggested that the behavior of a psychologically close partner (e.g., close friend, in-group member) has a greater impact on one's own experience compared to the behavior of someone psychologically distant. Smith and Mackie (2015) have also suggested that people draw on their knowledge about the other person when constructing their response to an experience. Familiarity with others may be especially important during negative experiences, given that attention often narrows to the central parts of an emotionally arousing situation (see Rubin, Berntsen & Bohni, 2008). Specifically, it is unclear whether people would think about the contents of strangers' minds, possibly a peripheral consideration in such situations. Therefore, the Shared Control and Unshared conditions may rate the CPT as equally negative: in other words, there will be no shared amplification of the experience when that experience is shared with an unfamiliar person.

On the other hand, if people construct representations of other's responses spontaneously and without awareness (Smith & Mackie, 2015)—that is, they take the other's perspective even without explicit instruction to do so—then we may expect participants in the Shared Control condition to amplify the experience, unlike the Unshared condition. Shteynberg's shared attention research has elicited shared amplification effects with simple manipulations that do not require participants to interact or be familiar with each other. For example, in Shteynberg (2010), participants were asked to choose an avatar color and were shown that two other participants had chosen the same color, suggesting that they were similar. In another series of online studies, Shteynberg, Bramlett, Fles, and Cameron (2016) simply embedded the number of other "live" viewers watching the same speech stimuli at the corner of the video, thus showing that shared amplification can occur with unfamiliar others as well. If such simple manipulations with strangers can lead to shared amplification, then completing the CPT with a similar (another Flinders University student of the same sex), if not familiar, other in the same room may still elicit shared amplification effects. Indeed, perhaps it is natural during a negative experience to consider others' responses, for example, to determine how serious the event is, thus leading to shared amplification effects regardless of the closeness of co-experiencers. Therefore, it is possible that the Shared Control condition will rate the CPT as more negative compared to the Unshared Condition.

To summarize our predictions, we expected participants in the Shared Perspective-taking condition to amplify the CPT compared to the other two conditions. We expected pain and stress would amplify the most because the CPT specifically induces these responses. We had competing predictions for whether the Shared Control condition would amplify more than, or similar to, the Unshared condition. We assessed participants' reactions again 24-hours later because people often report experiencing more trauma at follow-up compared to

initial assessment, a phenomenon called memory amplification (e.g., Southwick, Morgan, Nicolaou, & Charney, 1997). We expected ratings to amplify over time across conditions.

Method

Participants

To determine sample size, we first entered the difference in mentalizing between the shared and unshared groups in Nahleen et al. (under review) into an a priori two-tailed two-group t-test in G*Power (Faul, Erdfelder, Lang, & Buchner, 2007). Our analysis (d = 0.86, alpha = .05, power = .95) indicated that we should have at least 37 participants per group. However, because we had not previously tested the effect of perspective-taking, we used previous research to help us power for this potential group difference. Perspective-taking manipulations often lead to very large differences (e.g., Batson et al., 1997, Davis et al., 2004; Galinsky, Wang, & Ku, 2008; Oswald, 1996; Wang, Kenneth, Ku, & Galinsky, 2014; *M* for only perspective-taking/naturalistic control groups $d \approx 0.76$). An a priori two-tailed two-group t-test (d = .75, alpha = .05, power = .95) indicated we should have at least 48 per group (144 total). We decided to recruit 48 participants in each group so that both comparisons were powered appropriately.

We recruited female participants only because of gender differences in CPT pain tolerance (Mitchell, MacDonald, & Brodie, 2004). Part 1 was completed by 156 participants, but 12 were excluded: 10 did not complete Part 2 or completed it beyond the deadline (24-hours after receiving the link), one only immersed her fingertips, and one guessed our shared/unshared hypothesis. Our analyses included 144 participants (M_{age} = 21.64, SD = 4.85, range = 17-49). Participants received credit or $15AUD.

Materials[29]

[29] To prevent this experiment's procedure from becoming too lengthy and because we found no effects for the threat subscale in the SAM-adapted and no amplification effects for the AMQ in Nahleen et al. (under review), those measures were not included in this study.

Cold Pressor Task. To avoid ceiling effects that would prevent amplification, we chose the warmest temperature used in a CPT (approximately 7°C; Mitchell et al., 2004; see also Nahleen et al., under review). Participants immersed their non-dominant hand for one minute in a water cooler filled with gel ice cubes, cold water (6.2-7.4°C, *M* = 7.0), and a water-circulating pump. The CPT induces mounting mild to moderate pain and increases physiological stress responses (Schwabe, Haddad, & Schachinger, 2008).

Interpersonal Reactivity Index (IRI). We assessed participants' dispositional empathy at baseline on the 28-item IRI (0 = Does not describe me well; 4 = Describes very well; Davis, 1983; see Appendix M). The scale is comprised of four subscales measuring: the tendency to identify with feelings and actions of fictitious characters (fantasy), feelings of sympathy and concern for others (empathic concern), the tendency to spontaneously adopt the psychological point of view of others (perspective-taking), and feelings of anxiety and unease when exposed to the negative experiences of others (personal distress). The subscales correlate with other empathy measures such as the Empathy Quotient and the Basic Empathy Scale (see Baldner & McGinley, 2014).

Manipulation checks. We measured mentalizing directly (mentalizing questions) and indirectly (closeness ratings) to examine the effectiveness of our manipulation. We also measured how much the participant thought the confederate influenced their ratings to determine whether they were aware how our manipulations affected them.

Perspective-taking ratings. Four questions measured mentalizing directly (e.g., To what extent were you thinking about what the other participant was thinking and feeling while immersing your hand? (Boothby et al., 2014; 0 = Not at all; 6 = A lot; the other two scale questions were adapted from Todd et al., 2011 and Shteynberg et al., 2016); the last question was free recall (similar to Davis et al., 2004; Please write down all thoughts that occurred to you as you immersed your hand in the water).

Closeness ratings. Because constructing and incorporating other people's responses can occur without awareness, we also measured how close the participant felt to the confederate in attempt to indirectly measure increased perceived closeness due to mentalizing. Participants rated their impression of the confederate (Boothby et al. 2014) on four individually analyzed items (e.g., How much do you like the other participant?; 0 = Not at all; 10 = Very/A lot; Table 1). We also included Aron, Aron, and Smollan's (1992) single-item, pictorial Inclusion of Other in Self (IOS) scale which measures perceived closeness with another (see Appendix N). Participants see seven pairs of circles that range from just touching to almost completely overlapping. One circle in each pair is labelled "self," and the other is labelled "other." Participants choose which figure best represents their relationship with the confederate.

Confederate influence. Last, participants rated how much they felt the confederate influenced their CPT experience to check if they were aware how task sharing or mentalizing affected their responses (0 = Not at all; 6 = A lot/Very much so; Boothby et al., 2014).

Positive Affect Negative Affect Schedule (PANAS). Participants rated their positive (e.g., excited) and negative (e.g., distressed) affect (10 items per dimension; 1 = very slightly or not at all; 5 = extremely; Watson, Clark & Tellegen, 1988). The PANAS has good convergent correlations with other mood measures (.76-.92; Watson et al., 1988).

Short-form McGill pain questionnaire. First, participants rated their CPT pain on 15 characteristics (11 sensory e.g., throbbing; four affective e.g., fearful; 0 = None; 3 = Severe). Second, they rated peak pain intensity on the Visual Analogue Scale (VAS) slider (no pain to worst pain imaginable). Third, participants selected the term that best described their pain on the Present Pain Index (0 = No Pain; 5 = Excruciating; Melzack, 1987). Last, we adapted a question from the longer McGill Pain Questionnaire (Melzack, 1975) to assess Pain

Change throughout the CPT (0 = Brief; 2 = Continuous). The questionnaire shows high factorial validity and internal consistency (.76-.78; Wright, Asmundson & McCreary, 2001).

Stress Appraisal Measure (SAM) – Adapted. We assessed participants' stress-related thoughts (e.g., Does this situation create tension in me?) on the CPT with four SAM items (1 = Not at all; 5 = Extremely; Peacock & Wong, 1990). The SAM shows high internal consistency (.65-.90) and good convergent validity with dysphoric mood (r = .19-.58; Peacock & Wong, 1990).

CPT ratings. We used eight CPT experience items (e.g., How cold was the water?), analyzed individually (Table 1), from Nahleen et al. (under review; seven adapted from Boothby et al., 2014). Seven items used an 11-point scale (0 = Not at all/Extremely dislike/Very bad; 10 = Very/A lot/Extremely like/Very good), one used a 7-point scale (0 = Not at all; 6 = A lot).

Impact of Events Scale – Revised. The 22-item IES-R (Weiss & Marmar, 1996) assesses subjective distress caused by traumatic events (see Appendix O). We decided to measure distress to be consistent with the memory amplification literature, which has found that amplification is positively related to distress. It is comprised of three subscales: Intrusions, Avoidance, and Hyperarousal. Participants rated items (e.g., Any reminders brought back feelings about it) on a 5-point scale (0 = Not at all, 4 = Extremely) in relation to the CPT. Five items were removed for the immediate T1 session because they did not make sense without a longer delay (e.g., I had trouble staying asleep"). The measure demonstrates high internal consistency (0.96) and correlates with the PTSD Checklist (0.84; Creamer, Bell, & Failla, 2003)

Procedure

The Flinders University Social and Behavioural Research Ethics Committee approved this research. After providing informed consent, the participant completed the PANAS and

the IRI. They were then moved into the same room as the confederate[30] who seemingly had just completed the same questionnaires. The participant and confederate both picked from two cards to determine their activity. In reality, both cards assigned participants to the CPT, and the confederate to the CPT or catalogue reading; the confederate's chosen card was selected randomly before the session started. The experimenter gave task instructions and told the pair they could no longer speak. The experimenter left the room. In the Unshared condition, the participant completed the CPT while the confederate read a calculator catalogue (Boothby et al., 2014). In the Shared Control condition, the participant and confederate, standing side-by-side, completed the CPT simultaneously (the confederate's cooler contained room temperature water and unfrozen gel ice cubes). In the Shared Perspective-taking, before completing the CPT simultaneously, the pair were instructed to mentalize ("imagine how your partner feels about what is happening without looking at them. Concentrate on how your partner feels while doing the cold water task and how she is affected by it. Imagine your partner's emotional response as she experiences the pain"; instructions adapted from Leong, Cano, Wurm, Lumley, & Corley, 2015; see Appendix P). Participants then filled out all measures on individual computers. Twenty-four hours later, they completed the same questionnaires online in relation to how they had felt during the CPT. We asked what they thought the study was investigating, if they had heard about the study beforehand and, if so, what they heard and from whom. Participants were debriefed and compensated. The experiment was preregistered; see https://osf.io/exqa2/ for de-identified data and materials.

Results

Impact of CPT

[30] We had four confederates. The first participated with 63 participants, the second with 45, the third with 25, and the fourth 11. There were two experimenters, one ran 130, the other ran 14.

We first examined whether the CPT acted as a negative stimulus. Participants' positive mood decreased from pre- (M = 25.48, SD = 7.68) to post-immersion, M = 24.09, SD = 8.69; $t(143) = 2.97$, $p = .003$, d =-0.17 [-0.28, -0.06]. Negative mood increased from pre- (M = 13.27, SD = 3.68) to post-immersion, M = 14.93, SD = 5.18; $t(143) = -4.63$, $p < .001$, d = 0.37 [0.21, 0.53]. These results suggest that the CPT was an appropriate negative stimulus.

Manipulation checks

Perspective-taking ratings. We next examined whether participants in the Shared Perspective-taking condition mentalized more than those in the other conditions. For our first mentalizing item asking participants to what extent they thought about what the confederate was thinking and feeling (see Table 2 for descriptive statistics), we found no effect of time ($F(1, 141) = .004$, $p = .949$, $\eta_p^2 = .000$), condition ($F(2, 141) = 2.39$, $p = .095$, $\eta_p^2 = .033$), or a time x condition interaction, $F(2, 141) = 2.62$, $p = .076$, $\eta_p^2 = .036$.

However, we found an effect of condition on our second mentalizing item, $F(2, 141) = 3.85$, $p = .024$, $\eta_p^2 = .052$. Specifically, participants in the Shared Perspective-taking condition *tried* to imagine what the other participant might be thinking, feeling, or experiencing more than participants in the Unshared condition, $p = .045$; there were no other differences between conditions, ps = .063-1.000. There was no effect of time ($F(1, 141) = .18$, $p = .677$, $\eta_p^2 = .001$) or a time x condition interaction, $F(2, 141) = 1.47$, $p = .234$, $\eta_p^2 = .020$.

For the third mentalizing item, participants reported they thought the confederate liked their own activity less over time, a main effect of time, $F(1, 141) = 4.59$, $p = .034$, $\eta_p^2 = .032$. In addition, participants in the Shared Control ($p = .036$) and Shared Perspective-taking ($p < .001$) conditions thought the confederate liked their activity (CPT) less than those in the Unshared condition (reading the catalogue), a main effect of condition ($F(2, 141) = 10.93$, p

< .001, η_p^2 = .134); there was no difference between the two Shared conditions, p = .107. There was also no time x condition interaction, $F(2, 141) = .31$, p = .736, η_p^2 = .004.

For the free recall responses to the final perspective-taking item where participants wrote all thoughts that occurred to them as they immersed their hand, the first author (blind to condition) coded whether participants' thoughts included perspective-taking (e.g., "I wonder if she is feeling the same way I do", "The other participant has it easy as she only needs to read a catalogue and probably doesn't feel much discomfort", "Is she okay? How's she handling it? Is the water as cold for her as it is for me?") or not. The Shared Perspective-taking condition expressed more perspective-taking than the Shared condition (p = .002), a main effect of condition ($F(2, 141) = 6.20$, p = .003, η_p^2 = .081). There were no other differences, ps = .074-.703.

Therefore, on the scale items, participants in the Shared Perspective-taking condition did not report mentalizing more than those in the Shared Control condition. However, based on the free recall responses, it seems likely that those in the Shared Perspective-taking condition *did* in fact mentalize more than Shared Control participants without necessarily being aware of it. This possibility is consistent with Smith and Mackie's (2015) assertion that constructing representations of others' experiences and responses can occur without awareness. Further, the Unshared condition may have mentalized less than the other two conditions, suggesting that simply sharing the CPT experience leads to more mentalizing.

Closeness ratings. Because mentalizing can occur without awareness, we investigated whether our mentalizing manipulation indirectly affected participants by making them feel closer to the confederate. Participants felt less on the same wavelength as confederates over time, a main effect of time, $F(1, 141) = 8.53$, p = .004, η_p^2 = .057. There was also a main effect of condition with participants in the Shared Perspective-taking condition feeling more on the same wavelength as the confederate that participants in both

the Shared Control (p = .040) and Unshared (p < .001) conditions; the Shared Control condition rated higher than the Unshared condition (p < .001), $F(2, 141) = 56.71$, p < .001, η_p^2 = .446. There was no time x condition interaction ($F(2, 141) = 2.23$, p = .112, η_p^2 = .031).

There was also a main effect of condition for participants "getting" the confederate, with participants in the Shared Perspective-taking condition rating higher than both the Shared Control (p = .037) and Unshared (p < .001) conditions, and the Shared Control condition rating higher than the Unshared condition (p < .001), $F(2, 141) = 21.12$, p < .001, η_p^2 = .231. There was no effect of time ($F(1, 141) = .05$, p = .832, η_p^2 = .000), or a time x condition interaction, $F(2, 141) = 2.41$, p = .094, η_p^2 = .033.

Participants recalled liking the confederate less over time, a main effect of time, $F(1, 141) = 19.41$, p < .001, η_p^2 = .121. There was also an effect of condition: participants in the Shared Perspective-taking condition liked the confederate more than those in the Unshared condition (p = .002); there were no other differences, ps = .165-.403, $F(2, 141) = 5.95$, p = .003, η_p^2 = .078. There was also no interaction, $F(2, 141) = .69$, p = .503, η_p^2 = .010.

Participants recalled trusting the confederate less over time, a main effect of time, $F(1, 141) = 15.83$, p < .001, η_p^2 = .101. There was also an effect of condition: participants in the Shared Perspective-taking condition trusted confederates more than those in the Unshared condition (p = .006); there were no other differences, ps = .159-.714, $F(2, 141) = 5.01$, p = .008, η_p^2 = .066. There was no interaction, $F(2, 141) = 1.60$, p = .206, η_p^2 = .022.

There was an effect of condition on the IOS ($F(2, 141) = 11.67$, p < .001, η_p^2 = .142): participants in the Shared Perspective-taking condition reported feeling closer to the confederates on the IOS than both the Shared Control and Unshared conditions, ps < .001; there was no difference between the Shared Control and Unshared conditions, p = 1.000. There was no effect of time ($F(1, 141) = .27$, p = .603, η_p^2 = .002) or an interaction, $F(2, 141) = 2.88$, p = .060, η_p^2 = .039.

Therefore, overall, those in the Shared Perspective-taking condition felt closer to the confederate compared to those in the other two conditions, suggesting that our mentalizing manipulation influenced participants.

Confederate influence. There was no effect of time ($F(1, 141) = .39$, $p = .533$, $\eta_p^2 = .003$), condition ($F(2, 141) = 1.08$, $p = .343$, $\eta_p^2 = .015$), or a time x condition interaction ($F(2, 141) = .03$, $p = .973$, $\eta_p^2 = .000$) for how much participants felt the confederate influenced their ratings. Ratings across time and conditions were also low, further suggesting that participants were not aware of how sharing the experience or mentalizing influenced their responses. This pattern is consistent with Smith and Mackie's (2015) suggestion that others' responses are likely to affect one's own responses without awareness.

Overall, our results suggest that asking participants to take the perspective of the confederate may not have led to increased perspective-taking or mentalizing. However, based on the free recall and closeness ratings data, the manipulation may have influenced participants without their awareness, making them feel closer to the confederate.

Pain and Stress Responses

Pain (Short-form McGill pain questionnaire). Inconsistent with Nahleen et al., who found that participants reported more total and sensory pain characteristics over time and also in the shared (vs. unshared) condition, we found no main effects or interactions for total pain characteristics scores, or sensory and affective pain characteristics subscale scores (*p*s = .210-.901). There were no effects for the pain VAS (*p*s = .224-.948), PPI (*p*s = .136-.656), or Pain Change ratings (*p*s = .162-.326).

Table 1

Means and 95% confidence intervals for all questionnaire ratings

Questionnaire item	Part 1			Part 2		
	Unshared	Shared Control	Shared Perspective-taking	Unshared	Shared Control	Shared Perspective-taking
Mentalizing: To what extent were you thinking about what the other participant was thinking and feeling while immersing your hand?	2.60 [2.08, 3.13]	3.04 [2.56, 3.53]	3.58 [3.19, 3.98]	2.92 [2.36, 3.47]	3.00 [2.54, 3.46]	3.29 [2.83, 3.75]
Mentalizing: While immersing your hand in the water, to what extent did you try to imagine what the other participant might be thinking, feeling, or experiencing?	2.73 [2.18, 3.27]	2.96 [2.42, 3.49]	3.75 [3.34, 4.16]	2.96 [2.41, 3.50]	2.81 [2.31, 3.32]	3.52 [3.08, 3.96]
Mentalizing: How much do you think the other participant liked their activity (i.e., immersing their hand in the water or reading the catalogue)?	2.27 [1.79, 2.76]	1.63 [1.26, 1.99]	1.23 [.89, 1.57]	2.63 [2.13, 3.13]	2.02 [1.63, 2.41]	1.38 [0.95, 1.80]
Mentalizing: Please write down all thoughts that occurred to you as you immersed your hand in the water (coded: 0 = thoughts indicate no perspective-taking; 1 = indicates perspective-taking)	.67 [.53, .80]	.40 [.25, .54]	.75 [.62, .88]	.54 [.40, .69]	.42 [.27, .56]	.67 [.53, .80]
Closeness rating: Did you feel like you and the other participant were on the same wavelength?	2.04 [1.43, 2.65]	5.79 [5.18, 6.40]	6.58 [5.84, 7.33]	1.90 [1.28, 2.51]	4.88 [4.17, 5.58]	6.27 [5.50, 7.04]
Closeness rating: To what extent do you feel like you "get" the other participant?	2.29 [1.65, 2.93]	4.56 [3.92, 5.21]	5.83 [4.99, 6.67]	2.85 [2.02, 3.69]	4.29 [3.57, 5.02]	5.42 [4.60, 6.23]
Closeness rating: How much do you like the other participant?	5.48 [5.01, 5.95]	6.02 [5.51, 6.53]	6.44 [5.83, 7.05]	4.63 [4.06, 5.19]	5.40 [4.84, 5.95]	6.00 [5.45, 6.55]

	Part 1			Part 2		
Questionnaire item	Unshared	Shared Control	Shared Perspective-taking	Unshared	Shared Control	Shared Perspective-taking
Closeness rating: How much do you trust the other participant?	4.35 [3.78, 4.93]	5.35 [4.77, 5.94]	5.54 [4.83, 6.25]	3.81 [3.12, 4.50]	4.46 [3.84, 5.08]	5.27 [4.57, 5.97]
IOS	1.42 [1.16, 1.68]	1.69 [1.44, 1.94]	2.50 [2.05, 2.95]	1.54 [1.28, 1.80]	1.50 [1.32, 1.68]	2.48 [1.99, 2.97]
Influence rating	1.50 [1.06, 1.94]	1.79 [1.30, 2.29]	1.94 [1.47, 2.41]	1.56 [1.02, 2.10]	1.83 [1.35, 2.32]	2.04 [1.54, 2.54]
PANAS positive mood (post-immersion)	25.65 [23.18, 28.11]	24.00 [21.32, 26.68]	22.63 [20.23, 25.02]	23.10 [20.80, 25.41]	23.90 [21.40, 26.39]	21.25 [19.02, 23.48]
PANAS negative mood (post-immersion)	17.00 [15.22, 18.78]	14.00 [12.56, 15.45]	13.79 [12.76, 14.83]	18.42 [16.36, 20.47]	16.00 [14.20, 17.80]	16.29 [14.86, 17.72]
Total pain characteristics	16.33 [13.93, 18.74]	13.48 [10.82, 16.14]	16.10 [13.80, 18.41]	15.79 [13.01, 18.58]	13.71 [10.74, 16.67]	15.27 [12.74, 17.80]
Sensory characteristics	13.33 [11.45, 15.22]	10.90 [9.00, 12.79]	12.96 [11.19, 14.73]	12.90 [10.83, 14.96]	10.90 [8.70, 13.10]	12.31 [10.36, 14.26]
Affective characteristics	3.00 [2.13, 3.87]	2.58 [1.59, 3.57]	3.15 [2.36, 3.93]	2.90 [1.95, 3.84]	2.81 [1.87, 3.75]	2.96 [2.12, 3.80]
Visual Analogue Scale (VAS)	5.53 [4.97, 6.10]	5.23 [4.57, 5.88]	6.04 [5.51, 6.57]	5.58 [5.02, 6.14]	5.32 [4.61, 6.03]	5.91 [5.37, 6.45]
Present Pain Index (PPI)	2.79 [2.54, 3.05]	2.58 [2.29, 2.88]	2.71 [2.44, 2.98]	2.65 [2.39, 2.90]	2.50 [2.18, 2.82]	2.69 [2.41, 2.97]
Pain change	1.65 [1.49, 1.80]	1.42 [1.19, 1.64]	1.56 [1.38, 1.74]	1.54 [1.37, 1.71]	1.35 [1.15, 1.56]	1.58 [1.40, 1.77]
SAM stress subscale	2.58 [2.33, 2.83]	2.23 [1.95, 2.52]	2.62 [2.37, 2.87]	2.56 [2.29, 2.83]	2.27 [2.02, 2.51]	2.61 [2.35, 2.88]
CPT rating: How much did you want to take your hand out of the water?	7.71 [6.99, 8.43]	7.06 [6.32, 7.80]	7.06 [6.23, 7.89]	7.04 [6.32, 7.76]	6.19 [5.38, 6.99]	7.48 [6.77, 8.19]
CPT rating: How cold was the water?	8.96 [8.64, 9.28]	8.40 [7.93, 8.87]	8.50 [8.13, 8.87]	8.27 [7.87, 8.68]	8.06 [7.47, 8.65]	8.44 [8.03, 8.84]

	Part 1			Part 2		
Questionnaire item	Unshared	Shared Control	Questionnaire item	Unshared	Shared Control	Questionnaire item
CPT rating: How intense was the water?	8.44 [8.03, 8.84]	7.90 [7.36, 8.43]	7.92 [7.34, 8.50]	7.56 [7.04, 8.09]	7.42 [6.89, 7.95]	7.85 [7.22, 8.49]
CPT rating: How much would you be willing to immerse your hand in the water again?	4.25 [3.49, 5.01]	4.17 [3.37, 4.97]	3.48 [2.73, 4.23]	3.58 [2.82, 4.34]	3.40 [2.63, 4.16]	3.06 [2.36, 3.76]
CPT rating: How absorbed were you in the experience of immersing your hand in the water?	7.13 [6.36, 7.89]	7.13 [6.51, 7.74]	7.27 [6.66, 7.88]	6.67 [5.96, 7.37]	6.33 [5.66, 7.01]	7.27 [6.65, 7.90]
CPT rating: How focused were you on the water while immersing your hand?	4.56 [4.15, 4.97]	4.31 [3.87, 4.76]	4.10 [3.61, 4.60]	4.44 [4.03, 4.84]	4.40 [4.03, 4.76]	4.35 [3.92, 4.79]
CPT rating: How much did you like immersing your hand in the water?	2.10 [1.56, 2.65]	2.67 [1.90, 3.43]	1.77 [1.28, 2.27]	2.17 [1.48, 2.85]	2.23 [1.63, 2.83]	1.67 [1.23, 2.10]
CPT rating: How did you feel right after taking your hand out of the water?	5.79 [5.22, 6.36]	6.04 [5.52, 6.57]	5.92 [5.33, 6.50]	6.19 [5.38, 6.99]	5.48 [4.82, 6.14]	5.73 [4.90, 6.56]
IES distress[31]	.86 [.63, 1.09]	.97 [.71, 1.23]	.84 [.66, 1.02]	.66 [.45, .87]	.65 [.40, .91]	.41 [.25, .57]
Intrusions subscale	.77 [.54, .99]	.93 [.68, 1.18]	.95 [.72, 1.18]	.62 [.40, .84]	.64 [.40, .88]	.47 [.30, .65]
Avoidance subscale	1.01 [.75, 1.28]	1.06 [.77, 1.35]	.95 [.73, 1.17]	.75 [.52, .98]	.76 [.46, 1.06]	.46 [.28, .64]
Hyperarousal subscale	.78 [.53, 1.02]	.91 [.63, 1.19]	.57 [.39, .74]	.60 [.36, .84]	.53 [.28, .78]	.25 [.10, .40]

[31] Part 2 scores are for all items including the five removed for Part 1.

Stress (SAM-adapted). There was no effect of time or condition or interactions for stress-related thoughts (*p*s = .076-.940). This pattern is inconsistent with Nahleen et al.'s finding that participants in the unshared condition reported no difference in stress between the two time-points but those in the shared condition recalled *more* stress a day later compared to immediately after (i.e., a time x condition interaction).

Other Appraisals of the Task

CPT ratings. There was a condition x time interaction for wanting to take their hand out of the water, $F(2, 141) = 3.43$, $p = .035$, $\eta_p^2 = .046$. Pairwise comparisons showed that participants in the Shared Control condition recalled wanting to take their hand out of the water less 24-hours later, compared to immediately after the CPT ($p = .021$). The other two conditions reported wanting to take their hand out equally at both times (*p*s = .077-.268). Further, at T2, participants in the Shared Perspective-taking condition recalled wanting to take their hand out of the water more than the Shared Control condition ($p = .046$); there were no other differences (*p*s = .319-1.000). There was no main effect of time ($F(1, 141) = 3.01$, $p = .085$, $\eta_p^2 = .021$) or condition ($F(2, 141) = 1.55$, $p = .215$, $\eta_p^2 = .022$).

Unlike in Nahleen et al., we found a time x condition interaction for how cold the water was, $F(2, 141) = 3.40$, $p = .036$, $\eta_p^2 = .046$. Participants in the Unshared condition recalled the water as being less cold 24-hours later, compared to immediately after the task ($p < .001$); there was a strong trend for the same pattern in the Shared Control condition although this difference did not reach significance ($p = .052$). Participants in the Shared Perspective-taking condition, however, recalled the coldness consistent with their immediate ratings ($p = .714$). There was also a main effect of time with participants recalling the water being less cold over time, $F(1, 141) = 13.52$, $p < .001$, $\eta_p^2 = .088$. There was no effect of condition, $F(2, 141) = .96$, $p = .387$, $\eta_p^2 = .013$. Also differing from Nahleen et al., we found a time x condition interaction for how intense the water was, $F(2, 141) = 3.77$, $p = .025$, $\eta_p^2 =$

.051. Participants in both the Unshared ($p < .001$) and Shared Control conditions ($p = .023$) recalled the water as being less intense over time. Participants in the Shared Perspective-taking condition recalled the intensity consistently over time ($p = .766$). There was a main effect of time with participants recalling the water as less intense over time, $F(1, 141) = 15.29$, $p < .001$, $\eta_p^2 = .098$. There was no effect of condition, $F(2, 141) = .51$, $p = .605$, $\eta_p^2 = .007$. These data suggest that perspective-taking makes it difficult for participants to *forget* negative aspects of experiences.

Participants across conditions recalled being less willing to immerse their hand in the water again 24-hours (compared to immediately) after the task, $F(1, 141) = 19.75$, $p < .001$, $\eta_p^2 = .123$. There was no effect of condition ($F(2, 141) = .91$, $p = .404$, $\eta_p^2 = .013$), or a time x condition interaction, $F(2, 141) = .57$, $p = .566$, $\eta_p^2 = .008$. Participants across conditions recalled being less absorbed in the task over time ($F(1, 141) = 5.08$, $p = .026$, $\eta_p^2 = .035$); there was no effect of condition ($F(2, 141) = .91$, $p = .405$, $\eta_p^2 = .013$), or a time x condition interaction, $F(2, 141) = 1.54$, $p = .218$, $\eta_p^2 = .021$.

There was no effect of time ($F(1, 141) = .31$, $p = .581$, $\eta_p^2 = .002$), condition ($F(2, 141) = .55$, $p = .580$, $\eta_p^2 = .008$), or a time x condition ($F(2, 141) = .75$, $p = .476$, $\eta_p^2 = .010$) for how focused participant were on the water. There was no effect of time ($F(1, 141) = .83$, $p = .364$, $\eta_p^2 = .006$), condition ($F(2, 141) = 2.04$, $p = .134$, $\eta_p^2 = .028$), or a time x condition ($F(2, 141) = .70$, $p = .497$, $\eta_p^2 = .010$) for how much participants liked the water. There was no effect of time ($F(1, 141) = .29$, $p = .590$, $\eta_p^2 = .002$), condition ($F(2, 141) = .18$, $p = .832$, $\eta_p^2 = .003$), or a time x condition ($F(2, 141) = 1.63$, $p = .200$, $\eta_p^2 = .023$) for how participants felt (very bad to very good).

Mood (PANAS). Consistent with Nahleen et al., all participants recalled experiencing lower positive mood ($F(1, 141) = 9.28$, $p = .003$, $\eta_p^2 = .062$) and higher negative mood ($F(1, 141) = 30.10$, $p < .001$, $\eta_p^2 = .176$) 24-hours later, compared to immediately after the CPT.

Interestingly, there was a main effect of condition for negative mood: participants in the Unshared condition reported more negative mood than both the Shared Control (Bonferroni corrected pairwise comparison $p = .034$), and Shared Perspective-taking ($p = .038$) conditions, $F(2, 141) = 4.33$, $p = .015$, $\eta_p^2 = .058$, perhaps simply reflecting a displeasure participants felt in the Unshared condition when they compared their negative task compared to the confederate's neutral task. There was no effect of condition for positive mood ($F(2, 141) = 1.29$, $p = .279$, $\eta_p^2 = .018$), or any significant interactions (ps = .081-.470).

Distress (IES). We only included items measured at both times in this analysis (i.e., without the five items removed for T1). People recalled experiencing less total CPT-related distress over time, $F(1, 141) = 35.56$, $p < .001$, $\eta_p^2 = .201$. There was no effect of condition ($F(2, 141) = 1.01$, $p = .366$, $\eta_p^2 = .014$) or a time x condition interaction, $F(2, 141) = 2.59$, $p = .079$, $\eta_p^2 = .035$. We then examined the three subscales. We found a time x condition interaction for intrusions, $F(2, 141) = 3.18$, $p = .045$, $\eta_p^2 = .043$. Participants in the Shared Perspective-taking condition recalled experiencing fewer intrusions over time ($p < .001$); the Unshared ($p = .514$) and Shared Control ($p = .077$) conditions recalled consistent with their initial ratings of intrusion severity. There was also a main effect of time with participants recalling fewer intrusions over time ($F(1, 141) = 14.45$, $p < .001$, $\eta_p^2 = .093$), but no effect of condition ($F(2, 141) = .32$, $p = .726$, $\eta_p^2 = .005$). Participants also recalled fewer avoidance symptoms over time, $F(1, 141) = 37.09$, $p < .001$, $\eta_p^2 = .208$; there was no effect of condition ($F(2, 141) = 1.05$, $p = .352$, $\eta_p^2 = .015$) or a time x condition interaction, $F(2, 141) = 2.49$, $p = .087$, $\eta_p^2 = .034$. Participants also recalled fewer hyperarousal symptoms over time, $F(1, 141) = 24.87$, $p < .001$, $\eta_p^2 = .150$; there was no effect of condition ($F(2, 141) = 2.67$, $p = .073$, $\eta_p^2 = .037$) or a time x condition interaction, $F(2, 141) = 1.33$, $p = .267$, $\eta_p^2 = .019$.

Table 2

Correlations between IRI subscales and dependent measures regarding the CPT experience

Questionnaire item	Fantasy Subscale	Empathic Concern	Perspective-taking	Personal Distress
Total pain characteristics	.19*/.11	.28**/.17*	.12/.07	.34***/.33***
Sensory characteristics	.17*/.10	.26**/.16	.08/.03	.27**/.28**
Affective characteristics	.17*/.10	.23**/.16*	.15/.12	.38***/.35***
Visual Analogue Scale (VAS)	.11/.07	.23**/.14	.04/.02	.23**/.30***
Present Pain Index (PPI)	.05/-.04	.19*/.11	.04/-.03	.26**/.25**
Pain change	.07/.06	.11/.11	.08/.01	.16/.12
CPT rating: How much did you want to take your hand out of the water?	.08/.03	.18*/.12	.05/-.02	.18*/.19*
CPT rating: How cold was the water?	.002/.10	.19*/.24**	.06/.19*	.19*/.18*
CPT rating: How intense was the water?	.16/.13	.24**/.26**	.10/.19*	.19*/.18*
CPT rating: How much would you be willing to immerse your hand in the water again?	-.08/-.06	-.07/-.12	.05/.13	-.22**/-.22**
CPT rating: How absorbed were you in the experience of immersing your hand in the water?	.23**/.10	.22**/.16	.22**/.12	.07/.10
CPT rating: How focused were you on the water while immersing your hand?	.17*/.16	.27**/.18*	.12/.02	.06/.22**
CPT rating: How much did you like immersing your hand in the water?	-.05/-.08	-.11/-.13	.06/.10	-.09/-.16
CPT rating: How did you feel right after taking your hand out of the water?	.03/.08	.000/-.01	.06/-.03	-.15/.03
SAM stress subscale	.20*/.16	.28**/.26**	.03/-.003	.41***/.42***
PANAS positive mood (post-immersion)	.10/.07	.21*/.19*	.17*/.19*	-.19*/-.13
PANAS negative mood (post-immersion)	.05/.08	.17*/.19*	.03/-.02	.36***/.43***
IES distress	.10/.12	.15/.18*	.11/.09	.29***/.18*
Intrusions subscale	.13/.15	.14/.18*	.15/.11	.22*/.16
Avoidance subscale	.12/.08	.13/.16*	.06/.10	.29***/.20*
Hyperarousal subscale	.01/.11	.14/.18*	.08/.04	.30***/.13

Dispositional empathy (IRI)

To determine whether dispositional empathy is related to amplification, we correlated our IRI subscale scores with our dependent measures assessing possible amplification for

different aspects of the CPT experience (Table 2). Interestingly, correlations between our measures and the perspective-taking subscale were mostly non-significant and small, perhaps suggesting that perspective-taking or mentalizing is not the most critical component of empathy when it comes to amplification. Instead, the correlations between the dependent measures and the personal distress subscale—the tendency to feel distress at others' negative experiences—were the largest and most consistent across measures, followed by the empathic concern subscale.

Discussion

We had two main aims in this paper: to determine whether perspective-taking could account for shared amplification, and whether shared amplification still occurred for negative experiences without prior familiarity between co-experiencers. We predicted that participants in the Shared Perspective-taking condition would find the CPT more negative compared to the other two conditions. We also believed that participants in the Shared Control condition would find the CPT either more negative, *or* equally negative compared to those in the Unshared condition. Overall, we did not find much evidence that perspective-taking (at least with our manipulation) leads to shared amplification.

Unlike in Nahleen et al., we did not find shared amplification for pain and stress in the Shared (Control) condition compared to the Unshared condition. This difference is likely due to the removal of the rapport building phase before the CPT task; these conditions were otherwise identical to Nahleen et al. It is interesting that completing the CPT with someone similar but not familiar did not elicit shared amplification effects given that previous studies have found effects even when participants did not interact with each other first, or were unfamiliar with their co-experiencers (e.g., Shteynberg, 2010; Shteynberg et al., 2016). However, perhaps for a negative stimulus such as the CPT—where people are likely more focused on their own pain rather than the other person—a strong rapport-building task,

familiarizing participants with each other is required to elicit enough empathy to result in shared amplification effects (Martin et al., 2015). Future research could manipulate rapport to determine its importance in shared amplification.

The removal of rapport may also explain why we did not find shared amplification effects in the Shared Perspective-taking (vs. Shared Control) condition for most of our measures: simply asking participants to take the perspective of another person, especially of a stranger, may not have been a strong enough manipulation on its own to increase ratings. Indeed, participants in the perspective-taking condition did not rate any of the perspective-taking manipulation check scale ratings higher than the control condition. However, perspective-taking participants reported feeling closer to the confederate than those in the control condition, even without rapport building beforehand, suggesting that simply feeling closeness or familiarity to partners does not necessarily lead to shared amplification. Further, based on our free recall data, our manipulation appears to have increased perspective-taking without conscious awareness: more participants in the perspective-taking (vs. control) condition had thoughts containing elements of perspective-taking. Therefore, it is possible that our manipulation did increase perspective-taking, but this increased perspective-taking, in turn, did not lead to increased ratings for the experience. One explanation may be that our manipulation did not increase perspective-taking *enough* to increase ratings. For example, even though participants in the perspective-taking condition rated feeling closer on the IOS than participants in the control condition, their mean rating was still below the mid-point at both times, perhaps suggesting that they did not feel particularly close to the confederate. Future research should explore how different levels of perspective-taking influence ratings.

There may be an alternative reason why perspective-taking did not increase pain and stress. Boothby et al. (2014) argue that perspective-taking (which they refer to as "mentalizing"), presumably facilitated by rapport (although this is not explicitly stated), leads

to shared amplification. However, rapport may have instead facilitated other facets of empathy besides perspective-taking. Based on the lack of correlations between the perspective-taking IRI (our measure of dispositional empathy) subscale scores and our dependent measures, it is also possible that perspective-taking is not the most important facet of empathy involved in shared amplification. Instead, our dependent measures correlated most consistently and strongly with the IRI personal distress subscale scores—the tendency to experience unease or distress in response to others' negative experiences. This pattern is perhaps consistent with past research that has found higher personal distress is associated with an inability to regulate or control emotional responses to a tolerable range (Okun, Shepard, & Eisenberg, 2000). It is possible that people who have difficulty controlling their distress responses found the CPT experience more negative compared to those who have no such difficulty. Future research could manipulate this form of affective empathy to further explore the underlying mechanism of shared amplification.

Interestingly, participants in the Shared Perspective-taking condition did recall wanting to take their hand out more than those in the Shared Control condition. This finding suggests that while mentalizing alone does not increase pain and stress, it does increase other negative aspects of a shared painful experience. Furthermore, while participants in the Unshared and Shared Control conditions recalled the water being less cold and intense over time, those in the Shared Perspective-taking recalled water temperature consistent with their initial ratings. These results may have important implications given that previous research has found that forgetting or appraising negative events as less important or severe over time may be an adaptive adjustment mechanism that helps people regulate negative emotion (see Levine, Lench, & Safer, 2009).

It is important to note, however, that participants in the perspective-taking condition recalled experiencing fewer intrusions over time, unlike the other two conditions. Perhaps

purposefully trying to think about the content of the confederate's mental state encouraged conceptual processing of the task—processing the meaning of the experience, and in an organized manner, and placing the experience into context—rather than only processing the sensory impressions. According to Ehlers and Clark's (2000) model of posttraumatic stress disorder, conceptual processing reduces perceptual priming for stimuli, that is, it will likely reduce intrusions. Future research should investigate this possibility.

Perhaps unsurprisingly, participants *de*-amplified on many ratings over time, that is, they rated lower at follow-up than at initial assessment. This decrease may be explained by the fact that most people did not find the CPT distressing: past research has found that it is people who are distressed by their trauma experiences who have a propensity to amplify trauma over time (e.g., King et al., 2000; Roemer et al., 1998; Southwick et al., 1997). Compared to traumatic events, a) most adults have experienced some form of brief, cold-induced pain in the hand due to everyday events (e.g., while washing dishes), making cold water a familiar, unremarkable stimulus and b) participants were informed that physiological responses would return to normal once they withdrew their hand and that they could withdraw their hand at any time without penalty. However, consistent with Nahleen et al., participants *did* recall their mood during the CPT as more negative over time. This finding further suggests that participants may have drawn on their memory of the emotional intensity experienced during the CPT when attempting to remember past mood (see Kaplan, Levine, Lench, & Safer, 2015). This finding has real-world implications because it shows that people can also come to remember or report aspects of a stressful experience—other than what was experienced (e.g., Southwick et al., 1997)—as worse over time.

There are a few limitations to our study. First, we powered for large effects, which may have resulted in a smaller sample size than adequate to detect smaller yet meaningful effects. However, we relied on the large perspective-taking effects found in past research

when powering for our sample. Second, the CPT is only mildly stressful, unlike real trauma events. Future research could use a more stressful stimulus. Third, future research should also investigate the effect of rapport to determine the impact of familiarity of partners completing a negative experience together. Despite these limitations, our findings have important implications, suggesting that sharing experiences can amplify certain negative aspects of a shared painful experience. We also found preliminary evidence that a tendency to feel empathy, specifically personal distress, makes negative experiences seem more negative.

Chapter Ten: General Discussion

This chapter ties together the findings of the seven studies in this thesis. I will discuss the results in light of relevant theory and research and examine the important theoretical, clinical, and legal implications of my overall findings. Last, I will discuss limitations of the methodological approaches.

Summary of Findings

This thesis aimed to investigate three proposed explanations for trauma memory amplification: 1) people may relabel trauma events as significant over time to understand or explain their distress, 2) post-event information may be inadvertently incorporated into their trauma memory, leading to source monitoring errors and 3) when the event is shared with others, people may construct mental representations of others' responses and incorporate them into their own. These explanations for the memory amplification of negative and potentially traumatic experiences have not been empirically investigated in past research. The findings from my thesis studies provide preliminary support for all three explanations.

In Studies 1 and 2 (Chapters 3 and 4), I first examined three potential explanations of amplification with two longitudinal studies. First, past research may have found amplification consistently because victims in previous studies often shared the trauma event with others and were likely exposed to additional post-event information (e.g., through media reports). Therefore, my aim was to examine whether memory amplification still occurs for more personal, unshared traumatic life events, where victims would be exposed to minimal to no post-event information. That is, I investigated whether memory amplification still occurs when the potential underlying mechanisms of source errors caused by post-event information exposure (Explanation 4) and shared effects (Explanation 5) are removed. I also correlated distress measures with amplification (No-to-Yes) errors to determine whether amplification is

related to distress, particularly at follow-up (Explanation 3).[32] Participants reported at two time-points, with a six-month delay, whether they had experienced certain sexual assault events in the last five years. Importantly, I did not find an overall amplification effect: subjects reported fewer events at follow-up compared to initial assessment. We replicated this "de-amplification" effect in Study 2 (Chapter 4). Therefore, unlike some other traumatic events like military deployment (e.g., Southwick et al., 1997; Engelhard et al., 2008), sexual assault memories do not appear to generally amplify over time. Importantly however, participants still made No-to-Yes (NY) errors on some items (e.g., in Study 1, 56.2% reported at least one NY change, and 34.5% reported two or more). Further, in Study 1, participants who were exposed to media reports about their, and similar, experiences made more No-to-Yes errors, suggesting that exposure to potential post-event information might play an important role in memory amplification. In other words, any additional or inaccurate post-event information embedded in the media reports may have been incorporated into their trauma memory and mistaken for their actual experience (Explanation 4). But we did not replicate this finding in Study 2, suggesting that it is not a consistent effect. It is possible that the significant result found in Study 1 was spurious. The difference in mean NY errors made by those who were exposed to reports about their trauma versus those who were not, despite being significant, was very small (M_{diff} = 0.68 items). In Study 2, we did find, however, that people who were exposed to media reports about similar events failed to forget events over time unlike those who were not exposed to such reports. Therefore, the media reports may not have contained additional information, instead only strengthening memory for original event details. A similar explanation may apply to why we did not find that discussing their trauma with others leads to more amplification errors: because sexual assault is often private

[32] Recall that I did not investigate Explanation 1 due to the lack of laboratory evidence for repressed memory. I also did not investigate the possibility that intrusions and flashbacks may contain extended details that may then be incorporated into memory, resulting in memory amplification (Explanation 2) because one group of researchers have already researched this explanation.

and unshared, it may be less likely survivors of sexual assault (compared to other traumas) would be exposed to additional perspectives or information while discussing their specific, unique trauma experience. In Study 1 (although not replicated in Study 2), we found that participants who discussed their trauma during the 6-month delay (vs. those who did not) actually experienced *fewer* NY errors. Therefore, discussing trauma may increase conceptual processing of the event (processing the meaning of the event and putting it into context), which leads to fewer intrusions, a symptom of distress according to the *DSM-5*. Therefore, experiencing less distress due to such processing may explain why people experienced less amplification. We also found, in both studies, that the number of amplification errors made was positively related to various symptoms of distress, especially at follow-up (Explanation 3). Put differently, amplification is related to overall current distress, suggesting that people may amplify or exaggerate trauma events over time to make sense of their symptomatology. Interestingly, in Study 2 there was some suggestion that increased distress could be related to more amplification or a failure to forget events, unlike those exhibiting less distress. This pattern was the first indication that forgetting events may specifically be related to adjustment or recover from trauma.

Importantly, there is little research on how well people remember past, personally experienced trauma-related symptoms. I addressed this gap by investigating whether recollections of one's own PTSD symptoms (and not just memory for trauma exposure) distorts over time. My aim was to determine whether the reappraisal process of amplification—whereby traumatic events are relabeled as significant to make sense of distress—also applies to symptoms. In Chapter 5 (Study 3), I asked participants to recollect, at follow-up, their reported symptom severity at initial assessment. As expected, current symptom severity influenced memory for perceived past symptom severity. Participants who were PTSD-negative at follow-up underestimated past PTSD symptom severity while those

who were PTSD-positive overestimated past symptoms (consistent with Explanation 3). Therefore, my data show that memory for symptoms can change over time.

Taken together, the results from my first three studies suggest that people take into account their current concerns to reconstruct memory of past trauma-related experiences. For example, they reconstruct how many trauma events they experienced in the past, and also past symptom severity, based on how distressed they currently feel.

Due to the field nature of Study 1 and 2, I could not corroborate what participants actually experienced, making it impossible to determine whether they became more accurate or inaccurate over time. In other words, I could only examine the inconsistency, not inaccuracy, of trauma memories. I addressed this limitation by running a series of analogue laboratory experiments where analogue trauma exposure was controlled. Further, my previous studies used written, verbal test items (e.g., "being shot at"), which may have been ambiguous and subjective enough to be vulnerable to reinterpretation (see Engelhard et al., 2015). When Engelhard and McNally (2015) asked Dutch soldiers to explain the discrepancy in their memory reports over time, the most common explanation was that they had interpreted items differently. For example, one participant, who was shot at from friendly fire, thought that the item "being shot at" was about enemy fire specifically at Time 1, but not at Time 2. In Takarangi et al. (2017), participants viewed negative photos and completed an old-new photo or description test (comprised of written descriptions of the photos). Those in the description test condition were more biased to report they had seen negative photos compared to participants in the photo test condition. This finding suggests that verbal-item tests used in field amplification research is particularly vulnerable to response biases, leading responders to report experiencing more events than they necessarily did. Critically, in Study 5 (Chapter 6), I used photo test items, removing the possibility of a reinterpretation bias based on a verbal description.

In Studies 4 and 5 (Chapters 6 and 7), I focused on the post-event information explanation; in other words, I examined whether post-event information exposure causes amplification (Explanation 4). In Study 4, across three experiments, participants viewed a traumatic film with some scenes removed. Twenty-four hours later, they were given three "eyewitness" reports describing the film's events; either zero, one, or all three of the reports described removed scenes. As expected, misinformation exposure led to more source monitoring errors; however, this misinformation effect disappeared after increasing people's discrepancy detection, providing preliminary evidence that increased source monitoring is directly related to decreased memory errors. However, repeated misinformation exposure did *not* lead to higher error rates compared to single exposure, suggesting that negative experiences do not further amplify after repeated (vs. single) misinformation exposure. Therefore, in Study 5, we only exposed participants to each piece of post-event information once.

In Study 5, I aimed to find evidence that: a) memory amplification is directly caused by source-monitoring errors after post-event information exposure and thus will be reduced when source monitoring ability is increased and, b) memory is reconstructed to take into account the person's current goals and motivations, in this case improving discrepancy detection during post-event information exposure to make fewer errors. I showed participants traumatic IAPS photos and tested their memory at two time-points with a delay in between. Before the second test, I exposed participants to PEI through photo descriptions for some of the photos they would see in the second test. This design allowed me to examine whether people remembered more photos over time, both naturally (i.e., for New photos that were not accompanied with PEI) and after PEI exposure. As expected, participants inadvertently incorporated post-event information into their memory, that is, they made source monitoring errors and, thus, amplified their trauma memory. In follow-up experiments, I subtly warned

participants to increase their discrepancy detection by forcing them to decide whether they had seen photos of the post-event information in the encoding phase. Post-event information no longer distorted traumatic memory with a short (24-hours) delay but did distort memory after a long delay period (1-week). In other words, increased source monitoring helps reduce memory distortion but not when the original memory has decayed from the passing of a longer time period. My last experiment found that evaluating the source of photos increased source monitoring ability, making people resistant to inaccurate post-event information. Therefore, my four experiments in Chapter 7 show source monitoring errors due to PEI appear to be a key mechanism underlying memory amplification (see also Chapters 3 and 6 which both show that source monitoring errors after PEI is a key mechanism; Explanation 4). Further, despite using photo rather than verbal test items, I still found amplification errors. Therefore, reinterpretation is unlikely to be the only explanation of amplification. Last, similar to field research, I found a positive relationship between PTSD symptoms and source monitoring errors, further suggesting that amplification is related to current distress (Explanation 3).

In Study 6 (Chapter 8), I investigated whether amplification occurs because the events investigated in field research are usually shared in nature (Explanation 5). Participants completed the stressful Cold Pressor Task at the same time as a confederate (shared condition) or while the confederate completed another task (unshared condition). They then rated their emotional reactions to the task immediately after and again 24-hours later. Overall, I found some evidence that sharing a stressful experience amplifies that experience: sensory characteristics of pain were higher, and stress increased over time, in the shared condition. Importantly, participants in the shared condition reported thinking more about the confederate's thoughts and feelings. This mentalizing also sometimes mediated relationships between condition and dependent measures (e.g., peak pain intensity), suggesting participants

may have constructed mental representations of others' CPT experience (e.g., that it hurt) and incorporated it into their own responses (Smith & Mackie, 2015), thus amplifying the impact of the task.

In Study 7 (Chapter 9), I specifically investigated whether constructing other's psychological responses (via mentalizing) amplifies experiences. I replicated my previous study (Study 6) with an additional third condition whereby participants were explicitly instructed to take the confederate's perspective (mentalize) during the CPT. In other words, although mentalizing may happen spontaneously to some extent, I added a condition where participants' explicit goal was to mentalize during the task. I also removed the rapport building phase so that I could determine whether this perspective-taking alone leads to amplification of experiences. Interestingly, participants who were instructed to mentalize remembered wanting to take their hand out more than those who were not, suggesting that mentalizing increases certain negative aspects of a shared painful experience. Furthermore, I found evidence that people who were instructed to mentalize found it harder to forget certain negative aspects of the experience such as how cold and intense the water was, compared to the other conditions. However, unlike the previous experiment, I did not find that either sharing the event or mentalizing amplified pain or stress.

Theoretical Implications of Findings

Trauma memories are not special. Many clinicians and theorists believe that our memories for trauma are "special" and PTSD models often reflect this assumption. For example, according to Brewin's dual representation theory of PTSD, two types of memory representations are encoded during the trauma event: sensory-bound representation includes sensory details and emotional states experienced during the traumatic event, and contextual representation includes spatial and personal context of the person experiencing the event. While retrieval of contextual representations is under voluntary control, sensory

representations are accessed via associated contextual representations. The model argues that, in people with PTSD, consciously controlled episodic memories for the traumatic events are often impaired, whereas intrusive sensory representations are frequent. There is an assumption that these intrusions are accurate (Brewin & Burgess, 2014; Brewin, Dalgleish, & Joseph, 1996).

Ehlers and Clark's (2000) cognitive model also suggests, similar to Brewin's model, that people with PTSD will experience recurrent involuntary intrusions but have difficulty intentionally retrieving a complete memory of the event. Instead, their intentional recollections will be fragmented, out of order, and contain missing details (e.g. Foa, Molnar, Cashman, 1995; van der Kolk & Fisler, 1995). The inability to remember details of the trauma can be appraised negatively by survivors (e.g., something is wrong with them), maintaining the sense of current threat. In terms of memory accuracy, Ehlers and Clark point out that certain appraisals may lead them to remember details selectively (e.g., a person who thinks no one cares about her will only remember occasions when people were unfriendly), although this possibility does not suggest that such recollections are inaccurate. There is also an underlying assumption that intrusions are not only accurate, but almost immune to change: original emotions and sensory information are re-experienced even if new information discovered after the event contradicts those impressions or prove them untrue.

The assumption that trauma memory is accurate is further reflected in the *DSM-5*, used to ultimately diagnose people with PTSD. The manual posits that PTSD occurs when a person is exposed to a traumatic event (death, threatened death, actual or threatened serious injury, or actual or threatened sexual violence) and then experiences specific symptoms, that is, there is a direct dose-response relationship between objective trauma exposure and subsequent symptoms. However, this relationship does not take into account the fact that trauma memory can distort, making it difficult to give an accurate recollection of the event.

Therefore, the direct link between trauma exposure, which may be inaccurately remembered, and symptomatology may be tenuous.

Early research also suggested that trauma memory is special: as discussed in Chapter 1, Brown and Kulik (1977) found that people vividly and confidently recalled shocking, emotional events and concluded that there is a special brain mechanism that preserves memories of such events, making them immune to decay or distortion. The findings from the studies in this thesis are, overall, inconsistent with the special mechanism view. The special mechanisms view refers to multiple PTSD theories that all suggest that integration of specifically trauma events in memory is disrupted, leading to impaired voluntary access to the trauma memory, but enhanced involuntary remembering (e.g., though intrusions), especially in those with PTSD (e.g., Brewin & Burgess, 2014; Brewin, Dalgleish, and Joseph, 1996; van der Kolk & Fisler, 1995). Importantly, information accessed involuntarily via situational cues is difficult to edit, that is, trauma memory is accurate.

My findings are inconsistent with repression theory. This theory suggests the mind can bury traumatic information into the unconscious, leading to various psychological problems until it is recovered. Importantly, some researchers have suggested that memory amplification may be a reflection of repression, that is, people repress their memories initially but, over time, those memories come into consciousness (Southwick et al., 1997). Of course, without being able to corroborate trauma exposure in field studies, it is impossible to ascertain whether people are more accurate at the first or second assessment. With my analogue studies in Chapter 7, I was able to address this limitation. By controlling trauma analogue exposure, I was able to determine that people can come to remember seeing trauma stimuli that they did not actually experience at encoding, that is, they became more inaccurate over time.

Last, my findings are inconsistent with the theories suggesting that having a complete memory of the trauma is necessary to allay trauma related psychological problems. Instead, I found preliminary evidence that forgetting trauma may be related to an adaptive adjustment mechanism that helps people regulate negative emotion and decrease distress. However, it is important to note that my findings are correlational, making it impossible to determine causality. Further, the purpose of this thesis was not to examine the effects of forgetting trauma and, therefore, further research should be conducted before considering further theoretical or clinical implications.

Overall, my data adds to the growing body of literature that has found that trauma memory can be inconsistent over time and inaccurate; my research is thus consistent with the basic mechanisms view (Rubin, Boals, & Berntsen, 2008), which suggests that memory processes underpinning traumatic events are not special compared to memory for other events. My findings provide support for Rubin, Berntsen, and Bohni's (2008) memory-based model of PTSD which deviates from the models described above. The memory-based model suggests that memory of the trauma event, rather than objective trauma exposure, predicts the development of PTSD symptoms. Because memory is reconstructive, trauma memories will change and distort. Specifically, such memories will be influenced by various factors such as current goals, attitudes, concerns, and emotions. My findings suggest that factors such as re-appraisal of trauma due to increased current distress, source monitoring errors caused by post-event information (and decreased source errors when goals are manipulated to increase source monitoring), and possibly concerning oneself with what others are thinking and feeling while sharing trauma experiences, may further lead to memory distortion of trauma exposure.

Amplification does not occur consistently. First, I found that certain aspects of stressful or trauma events "de-amplify" over time even while other aspects amplified as

expected. In Study 6 (Chapter 8), for example, sensory pain and stress amplified while memory characteristics and perceived intensity and coldness of the water de-amplified. These findings may be a reflection of the stimuli, however. The Cold Pressor Task is used to specifically elicit and study pain and stress responses and may not prompt other distress responses. Indeed, cold water, as painful and stressful as it can be, is a familiar stimulus for most people, and is seems unlikely to distress participants emotionally. Further, it is important to remember that memory fading over time may be a normal response when there is no attempt to retain it (Oberauer & Lewandowsky, 2008).

Second, inconsistent with past field studies, my field studies (Studies 1 and 2) suggest that people report fewer events at follow-up (versus initial) assessment or, in other words, they "de-amplify" over time for entire experiences. This finding may indicate that women are reluctant to report sexual assault trauma compared to other types of trauma. In another longitudinal study, Mollica, Caridad, and Massagli (2007) examined trauma memory consistency over a three-year period in a sample of highly traumatized refugees. They also found that people reported fewer events at follow-up, compared to initial assessment, although the change was small. They suggested that this finding was best illustrated by the extreme decreased reporting for sexual abuse and rape specifically: there was a 100% decrease in endorsement of the item asking whether participants had witnessed the rape or sexual abuse of another, with 115 participants endorsing it at initial assessment and then denying it three years later. The researchers suggested that this inconsistency may be influenced by stigma. While I acknowledge this very real possibility, this explanation is beyond the scope of this thesis. I will instead attempt to explain my findings in relation to the three separate explanations of amplification I posit in this thesis: current distress, source monitoring errors caused by PEI exposure, and shared effects.

The three explanations for memory amplification. Now I will discuss my results in light of the three specific explanations of amplification that this thesis aimed to test.

Amplification and current PTSD symptomatology. We often found that increased distress (e.g., PTSD) symptoms at follow-up were related to amplification for trauma events and symptoms both in the field studies and in the laboratory experiments. Therefore, it is possible that increased memory of trauma and trauma-related symptoms is a result of a reappraisal process: at the later time point(s), distressed people may appraise trauma as worse than initially reported to make sense of, or justify, current symptomatology. People who are not currently experiencing distress, on the other hand, may appraise trauma as less negative, leading them to remember it as less severe or comparable over time. Indeed, this process may help explain why I found that people reported fewer sexual assault events over time in Study 1: across all participants, participants reported experiencing less distress at follow-up versus initial assessment. In both Studies 1 and 2: forgetting or minimizing errors (YN) was related to less distress at follow-up. Further, in Study 3, people who were PTSD-negative at follow-up recalled experiencing fewer symptoms in the past than initially reported, suggesting that forgetting past symptoms is related to less distress.

In a similar vein, Lommen, van de Schoot, and Engelhard (2014) pointed out that a respondent's experiences could change the meaning of a measure's constructs, that is, after experiencing a traumatic event, the probability of answering "yes", and the importance of questions, may increase or decrease. For example, soldiers who complete a questionnaire assessing PTSD symptoms after, compared to before, deployment are likely to specifically respond in reference to life-threatening warzone experiences (e.g., being shot at) that may have drastically changed their view on the world and themselves. In my study, both assessments were given after the trauma events had occurred. However, a similar explanation may apply here to explain why people reported fewer events over time in my field studies:

after a six-month delay people may have begun to try and "put the past behind them," and not report their trauma (Mollica et al., 2017) or reinterpret the items (Engelhard et al., 2015). If so, it would suggest that amplification, or de-amplification in this case, is not necessarily caused by memory errors but instead by other factors such as adjustment to trauma or response biases for example.

I should note that, as discussed in Chapters 1 and 7, researchers have suggested that memory amplification may reflect a source monitoring error (Strange & Takarangi, 2012, 2015) due to re-experiencing symptoms only. Intrusions of an experienced traumatic event may produce other imagined thoughts and images related to the event (Reynolds and Brewin, 1998), which may then be inadvertently incorporated into memory. However, I found that other clusters of PTSD symptoms besides intrusions, and indeed other symptoms of distress, are related to amplification, suggesting that source errors caused by re-experiencing symptoms cannot alone entirely explain amplification. Further, in Oulton, Strange, Nixon, & Takarangi (2017), participants asked to imagine details beyond the trauma analogue witnessed experienced fewer intrusions and a decreased tendency to endorse trauma exposure over time. It is possible that imagining new details led to conceptual processing of the trauma analogue, leading to fewer intrusions and, thus, less endorsement of exposure. However, this explanation requires further research and is beyond the scope of this thesis.

Amplification and post-event information. Consistent with my expectations, I found that exposure to post-event information leads to memory distortion. First, people who were exposed to media reports about their trauma experienced more amplification errors in Study 1. Therefore, post-event information may be incorporated into memory for the original event, thus amplifying the trauma. However, it is important to remember that participants in Studies 1 and 2 did not amplify overall. Further, in Study 2, I found that people who were not exposed to media reports about similar events underreported over time, whereas those who

were exposed to such reports actually remembered their trauma consistently over time. Therefore, it is possible that these reports contained details consistent with their own trauma, or it led them to remember and rehearse their own trauma. These rehearsal effects may strengthen event representations and memory traces for their trauma event, making it difficult to forget those experiences over time unlike those who were not exposed to media reports.

In my laboratory experiments in Study 5, I found that people remembered more trauma analogue photos over time when those photos were earlier described (post-event information) compared to when they were not described. Usually, people can often accurately attribute the source of their memories because the characteristics of memories from different sources differ in quality. However, overlap in characteristics of memories from two sources can lead to source confusions (Johnson, Foley, & Leach, 1988). The features of a traumatic event and subsequent post-event information overlap considerably. They are nearly identical because both sources of information are about the same event. Further, when asked about the event, victims need to retrieve and reconstruct the original event while also processing the post-event information. Imagining the post-event information can make it more familiar and similar to details from the original event memory. Therefore, simple descriptions of trauma photos led participants to expand their memory for old photos to include some of those descriptions. Contrary to Paterson and Kemp's (2006) finding that correct post-event information can increase accuracy, however, correct post-event information did not improve memory overall possibly because accuracy was already very high at initial test, making it difficult to improve participants' memory any further after a delay once memory has started to decay. Regardless, these findings are worrying given that people, over time, are likely to provide more inaccurate but not more accurate information after post-event information.

It is important to note, however, that memory for the photos was not perfect even for Old photos. For example, at Test 2, people forgot around 20% of photos after a short delay

(24-hours) and even more after a longer delay. Therefore, in Study 5, Experiment 1a, where participants remembered 20% more photos after post-event information, they were actually remembering as many photos as they had forgotten. Therefore, an alternate explanation for amplification is related to the similarity between the pictures. Studies have shown that people have difficulty keeping related items distinct from each other due to the semantic overlap between items. This overlap leads people to believe that they have seen previously unseen items that are consistent with the schema of previously studied items (Friedman, 1979; Hannigan & Reinitz, 2001). In other words, people form a semantic gist of the items and anything similar to the gist is falsely recognized because it feels familiar. Specifically, in Study 5, participants may have falsely remembered photos that reflected the emotional tone or gist of those seen at encoding.

I found evidence that judgment accuracy is influenced by retrieval circumstances: I found misinformation effects with old-new tests but not source monitoring tests or after subtle warnings that the PEI might be inaccurate. Unlike in recognition tests, where participants can respond according to how familiar each item is (e.g., Old for all familiar items), a source monitoring test requires people to evaluate their memory further. This evaluation increased discrepancy detection and resulted in fewer errors overall. Similarly, the warning appeared to make participants more careful and deliberate at test and, thus, increased their likelihood of detecting discrepancies and resisting inaccurate post-event information with a short delay. After a week, however, a subtle warning was less helpful, suggesting that the original memory traces had degraded to a point where the subtle warning could no longer improve source ability. Further, it is important to note the potential downside of trying to increase source monitoring because it may lead people to believe that all information contained in a post-event source is unreliable and, thus, ignore the post-event information at test, even if that information is accurate.

Interestingly, we did not find that people naturally remembered more photos at follow-up (i.e., for photos that were no described earlier). This finding is inconsistent with Strange and Takarangi (2012), where participants falsely recognized 26% of the missing clips as being part of the trauma film they had watched earlier (where those missing clips did not appear). The researchers in that study proposed that the participants may have realized that there were gaps in the film and generated content to fill in the gaps. At test, the missing clips would have probably resembled the internally generated content because they may have been able to guess what occurred based on what happened at the beginning and end of each clip they saw. Therefore, the missing clips could have felt familiar, and participants could have mistaken this sense of familiarity for genuine remembering (Jacoby, Kelley, Brown & Jasechko, 1989; Johnson et al., 1993). In our study there was no narrative link between the pictures we used; they depicted distinct, unrelated traumatic scenes (e.g. corpses, animal cruelty, burn victims and so on). Participants were unlikely to engage in rehearsal to generate content to complete a coherent narrative as no such narrative existed, leading to no natural amplification.

Amplification and sharing experiences. My findings for shared amplification were mixed across Studies 6 and 7. In Study 6, participants in the shared (vs. unshared) condition experienced more sensory pain characteristics and reported more stress over time. Importantly, they mentalized more as well. Because mentalizing sometimes mediated effects between the independent variable (whether the task was shared or not) and dependent measures (e.g., pain), I hypothesized that those in the shared condition may have constructed mental representations of others' CPT experience (e.g., that it hurts) and incorporated it into their own responses (Smith & Mackie, 2015), thus amplifying the impact of the task. In the follow-up, where I removed rapport and added a third condition in which participants were explicitly told to mentalize, I found two important overall findings. First, pain and stress no

longer amplified in the shared (control) versus unshared condition unlike the previous study. This difference is could be due to the removal of the rapport building phase before the CPT task because these conditions were otherwise identical to the previous study. Therefore, familiarity between those sharing the experience appears necessary to find shared effects. Indeed, Boothby et al. (2016) suggested that the behavior of a psychologically close partner has a greater impact on one's own experience compared to that of a stranger. In other words, it is likely easier to mentalize for someone closer to you, thus increasing shared effects. However, further research on the effect of familiarity on negative experiences is necessary. Martin et al. (2015) found that participants reported higher pain intensity when sharing the Cold Pressor Task versus alone, but only when the partner was familiar, that is, a friend or a stranger they bonded with before the task. The bonding task involved playing the video game, Rock Band, presumably a pleasant and positive experience. In Borsook & MacDonald (2010), participants rated the intensity and unpleasantness of painful stimuli before and after engaging in a structured question task (similar to Boothby et al., 2014) with a confederate who was told to either be friendly or indifferent. The researchers found that those who experienced the mildly negative rapport task reported lower pain intensity and unpleasantness after the encounter compared to baseline, whereas those who experienced the positive exchange exhibited no change in pain ratings. This finding is consistent with the evidence that even slight social disconnection and isolation can be stressful and elicit physiological reactions, including decreased pain sensitivity, similar to when confronted with a physical threat (Borsook & MacDonald, 2010). Therefore, familiarity alone may not lead to shared effects, that is, the type of connection between people sharing an experience may also affect reactions. Future research should investigate this possibility further.

Interestingly, however, I did not find much evidence that the perspective-taking manipulation (mentalizing) leads to amplification. There are two possible reasons why this

might be so. First, my perspective-taking manipulation may not have been "strong" enough. Importantly, this type of manipulation has been used successfully in multiple past studies, leading to increased empathy and distress toward other people who are not even present, but instead featured in audio or film stimuli. However, we know that attention often narrows to the central parts of an emotionally arousing situation (see Rubin, Berntsen & Bohni, 2008), and thinking about what strangers are experiencing may be a peripheral consideration during a personally painful experience. Therefore, even with the instructions to explicitly mentalize, participants may have found it difficult to do so. Indeed, participants did not report mentalizing more than those in the shared control condition. However, more participants in the perspective-taking (vs. control) condition had thoughts containing elements of perspective-taking, suggesting that they experienced perspective-taking without necessarily being aware of it. It is possible, therefore, that our manipulation increased perspective-taking to some extent but not enough to increase ratings.

Another possibility is that perspective-taking does not lead to shared effects after all. First, mentalizing did not mediate all effects in Study 6. Indeed, I found that it did not mentalize the effect of sharing the task on pain and stress, my two main variables of interest. Importantly, Shtyenberg (2015) suggested that mentalizing is not necessary to induce shared effects. Instead, when people co-attend to an experience with those they will act collectively with in future, the shared attention state 'switches on,' channeling cognitive resources to the shared stimuli. However, in my studies, co-experiencers were complete strangers and participants likely realized that they may never act with the confederate after the experiment session was over, thus leading to almost no shared amplification. Second, I found preliminary evidence that empathy may be related to shared amplification, but not the mentalizing aspect. Rather, it is possible that personal distress at others' negative experiences may be key.

However, because I did not manipulate this aspect of empathy specifically, it is not possible to come to any firm conclusions. Rather, future research should investigate this possibility.

Real-world Implications

Clinical Implications. The issue of trauma memory inaccuracy is critical in clinical scenarios. To be diagnosed with PTSD, clients must have experienced a traumatic event that resulted in PTSD symptoms (*DSM-5*; American Psychiatric Association, 2013). However, in practice, clinicians are not present during the event and often only have the client's report of the event and initial symptoms. This may be especially the case when it comes to victims' remember their own symptoms: the persistence of PTSD symptoms over 1-month is required for a PTSD diagnosis. Overall, my data contribute to clinical psychological science by proposing that clinicians should be aware that some people may not remember past trauma or symptoms accurately and that any additional corroborating evidence where possible may be useful. Despite the fact that there is no convincing evidence that trauma memory is special, 60.3% of clinical practitioners still assume that trauma memories can be repressed and 43.1% believe that such memories can be retrieved in therapy accurately (Patihis, Tingen, Lilienfeld, & Loftus, 2014). Further, we know that not only can memory details change and distort, but entire new events can be implanted into memory. Memory researchers have asserted that some psychological treatments involve suggestive techniques that may have led clients to falsely remember entire episodes of trauma (see Loftus, 1994). Therefore, some memories "recovered" in therapy may very well be inaccurate. Indeed, research has found that some people who have retracted earlier claims of having been sexually abused reported being the subject of memory recovery techniques (Ost, 2016).

Interestingly, only 19.4% of clinical psychologist researchers believe in repressed memory, and 16.1% believe they can be recovered accurately (Patihis et al., 2014). Therefore, there is a clear scientist-practitioner gap in beliefs about trauma mental health.

Therefore, we need to narrow this gap by communicating memory research findings to practitioners or preferably to students training to become practitioners. Being exposed to research showing that trauma memory is not special will allow them to: a) avoid using suggestive techniques, and b) sensitively discuss with clients that past trauma and symptomatology may not always be as severe as their memory.

Legal implications. Critically, victims often need to present their recollections of an experienced traumatic event in the legal system, for example, in police interviews and court trials. Their statement is often used as an objective indictor of trauma exposure, especially when there is a lack of physical evidence, regardless of how much time has elapsed between the event and the time it is remembered. Often, cases can take years from arrest to sentencing, meaning that survivors may need to provide statements long after the event occurred. As discussed, however, memory distorts and decays over time because of various factors. For example, people may have shared the trauma experience (e.g., soldiers in a unit, other passengers in a car crash), or been exposed to post-event information regarding the trauma, both factors that can distort memory. Then, due to the highly negative and emotional nature of the trauma, they may experience symptoms of psychopathological disorders such as PTSD. Further, people can be exposed to additional information about the traumatic event they experienced via media reports, suggestive questioning, and discussion with co-witnesses or even other victims. All of these factors can distort and inflate memory, potentially making testimonials inaccurate.

Certain factors that lead to memory distortion, such as whether the trauma event was experienced with others or not, cannot be controlled by the time the police get involved. However, other factors can be controlled to a certain extent, again starting with the dissemination of memory research findings. For example, police officers be made aware to minimize post-event information exposure (e.g., suggestive questioning) as much as possible

and jurors should know that trauma memory can be distorted even when testimonials are given with high confidence. Otherwise, these memory distortions can have serious consequences, leading to false accusations and wrongful convictions.

Limitations

Overall limitations. First, I recognize that there are likely other potential mechanisms for amplification beyond the ones we investigated. However, the purpose of this thesis was to investigate the most obvious explanations provided in the amplification literature (e.g., Southwick et al., 1997). The studies described in this thesis are some of the first to systematically investigate explanations for memory amplification. Future research could identify other potential explanations by asking participants why their reports changed over time (see Engelhard & McNally, 2015), although not all participants might have insight for the change. I also note that, across studies, my samples were comprised mostly or completely of females, which may have influenced findings. For example, I only recruited females in my CPT studies (Studies 6-7) because of gender differences on the task: men can tolerate the CPT significantly longer than women (e.g., Mitchell et al., 2004). It is possible, therefore, that men would not have found the task painful or stressful (or at least not as painful or stressful compared to females), making it perhaps less likely they would amplify the task given that distress symptoms and amplification are positively related.

Limitations of the field studies (Studies 1-3). I must acknowledge several limitations of my field research. First, I could not determine causality (e.g., whether post-event information exposure *leads* to amplification) given the correlational nature of the research. Second, due to the self-report nature of this research, I could not corroborate that trauma events really occurred and, if so, whether the events occurred in the last five years. Therefore, it is impossible to determine if people were becoming more accurate or inaccurate over time. However, this is an issue found in all field studies, which I addressed by running

experimental studies (Studies 4-7); however, using analogue trauma stimuli has its own problems given that it is unlikely to elicit the same emotionality and stress as real trauma events. Nevertheless, using a mixture of field and analogue methodologies is a strength of this thesis as it allows to me examine and determine the strength of the evidence for amplification across different experiences and stimuli (see Roediger, 2008). Third, responding to the same exposure scale items twice may have led to additional source confusion for participants. Indeed, we know that overlap in characteristics of memories from two sources can lead to source confusions (Johnson et al., 1988). The experienced trauma and exposure scale already overlap given that both are about the trauma. While responding to the exposure scale, participants may need to actively retrieve their trauma while also processing the scale items, likely increasing the overlap between the event and the scale further. It would not be surprising if participants confuse scale items for experienced events, especially if the scale item describes an event similar to one that was actually experienced. This confusion may increase when participants respond to the scale a second time. Fourth, items on my exposure scale may have been ambiguous and subjective enough to be vulnerable to reinterpretation (see Engelhard et al., 2015; Takarangi et al., 2017). Therefore, report inconsistency may reflect reinterpretation or misclassification of items as already discussed. Fifth, I did not measure all factors that may be related to memory distortion such as other trauma events beyond sexual assault. Last, my findings across Study 1 and 2 were not completely consistent and, with my data, it is unclear why. Future research should systematically investigate these differences.

Limitations of the post-event information experiments (Studies 4-5). The decision to use film clips IAPS photos in Studies 4 and 5 was made for ethical reasons because I obviously could not expose participants to real traumatic events that I could also corroborate. Such stimuli may not have replicated the stress and emotionality experienced during a

negative event, making it impossible to determine whether findings can be generalized to real trauma victims. However, film content depicting actual or perceived threat and serious injury has been found to elicit responses analogous to symptoms experienced after actual trauma e.g., intrusions, physiological arousal, negative cognitions and mood (see James, Lau-Zhu, Clark, Visser, Hagenaars & Holmes, 2016). IAPS pictures have also been shown to elicit fear related physiological responses (e.g., Litz, Orsillo, Kaloupek, & Weathers, 2000; Smith, Bradley, & Lang, 2005) and behavioral responses (e.g., Bywaters, Andrade, Turpin, 2004). Further, repeated exposure to traumatic work-related photos is now considered a Criterion A stressor according to the DSM-5.

However, even if these negative photos and film clips are considered traumatic, such stimuli are still very different from items used in classic amplification studies where participants are asked if they have experienced specific traumas with verbal test items. Using different stimuli may make it difficult to compare my studies to field studies. For example, after a trauma event, people may try to avoid internal (e.g., thoughts about the trauma) and external (e.g., where the trauma occurred) reminders of the event (*DSM-5*). Research shows that trying to suppress thoughts ironically increases frequency of those thoughts (Wegner, Erber, & Zanakos, 1993), which may strengthen the memory of the trauma (via rehearsal mechanisms) or even amplify the trauma memory if thoughts include imagined details (see Oulton et al., 2018). If participants did not feel compelled to avoid thoughts about the negative photos and film clips (because they are likely less negative than personally experienced trauma), it could explain why we did not find natural amplification effects in Study 5 (i.e., amplification without post-event information exposure), unlike in field amplification research.

Last, having photo rather than text test items may not be ecologically valid because victims and witnesses are unlikely to be re-exposed to trauma during questioning. However,

these people are likely to attempt to mentally picture the event they are trying to remember (see Johnson et al., 1993). Furthermore, photo tests may have affected our memory distortion rates given that people are biased to remember more text test items compared to photo items (see Takarangi et al., 2017).

Limitations of the shared amplification experiments (Studies 6-7). I decided to use the CPT in Studies 6-7 to maintain experimental control of trauma analogue exposure while also using a more ecologically valid stimulus where participants *experience*, rather than only witness, the negative event. However, using this task means that I could not investigate memory "accuracy," only consistency of self-reported feelings regarding the task. Further, the CPT is only mildly stressful, unlike real trauma events that, like using film clip and photo stimuli, could have influenced results. Future research could use a more stressful stimulus (although this may lead to overall ceiling effects) or investigate the effect of real shared trauma. However, similar to field studies, it would be difficult to corroborate events and ensure that no communication occurred between victims who shared the trauma. Second, I powered for large effects, which may have resulted in a smaller sample size than adequate to detect smaller yet meaningful effects. However, this sample size was based on the large effect found by Martin et al. (2015). Second, I discovered that rapport may be crucial to finding shared amplification effects, consistent with Martin et al. (2015) and Boothby et al. (2016) findings. However, I did not manipulate rapport itself, making it statistically impossible to determine its importance in shared amplification.

Conclusion

Overall, my findings provide preliminary evidence for all three explanations for memory amplification. Specifically, I found that: 1) feeling distressed, including experiencing PTSD symptoms, is related to amplification, 2) exposure to post-event information can distort memory, leading to increased remembering of trauma analogue,

unless source monitoring can be increased, and 3) sharing a negative experience with another person may amplify the experience, making it more negative. Although some of my findings were mixed, they have important implications and warrant further research.

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Appendix A: Positive Affect Negative Affect Schedule (PANAS)

This questionnaire has been removed due to copyright restrictions.

Appendix B: Depression, Anxiety, and Stress Scale-Short form (DASS-21)

Instructions

Please read each statement and click a number 0, 1, 2 or 3 that indicates how much the statement applied to you over the past week. There are no right or wrong answers. Do not spend too much time on any statement.

0 - Did not apply to me at all

1 - Applied to me to some degree, or some of the time

2 - Applied to me to a considerable degree, or a good part of time

3 - Applied to me very much, or most of the time

	0	1	2	3
1. I found it hard to wind down				
2. I was aware of dryness of my mouth				
3. I couldn't seem to experience any positive feeling at all				
4. I experienced breathing difficulty (e.g., excessively rapid breathing, breathlessness in the absence of physical exertion)				
5. I found it difficult to work up the initiative to do things				
6. I tended to over-react to situations				
7. I experienced trembling (eg, in the hands)				
8. I felt that I was using a lot of nervous energy				
9. I was worried about situations in which I might panic and make a fool of myself				
10. I felt that I had nothing to look forward to				
11. I found myself getting agitated				
12. I found it difficult to relax				
13. I felt down-hearted and blue				
14. I was intolerant of anything that kept me from getting on with what I was doing				
15. I felt I was close to panic				
16. I was unable to become enthusiastic about anything				
17. I felt I wasn't worth much as a person				
18. I felt that I was rather touchy				
19. I was aware of the action of my heart in the absence of physical exertion (eg, sense of heart rate increase, heart missing a beat)				
20. I felt scared without any good reason				
21. I felt that life was meaningless				

Appendix C: PTSD Checklist-5 (PCL-5)

Instructions

Below is a list of problems that people sometimes have in response to extremely stressful experiences: keeping in mind the sexual experience from the list above that bothers you the most, please read each problem carefully and mark the appropriate answer in the space next to the word to indicate how much you have been bothered by that problem in the past month (or "6 months ago" in Study 3 when participants recalled past symptoms at follow-up).

	Not at all (0)	A little bit (1)	Moderately (2)	Quite a bit (3)	Extremely (4)
1. Repeated, disturbing, and unwanted memories of the stressful experience?					
2. Repeated, disturbing dreams of the stressful experience?					
3. Suddenly feeling or acting as if the stressful experience were actually happening again (as if you were actually back there reliving it)?					
4. Feeling very upset when something reminded you of the stressful experience?					
5. Having strong physical reactions when something reminded you of the stressful experience (for example, heart pounding, trouble breathing, sweating)?					
6. Avoiding memories, thoughts, or feelings related to the stressful experience?					
7. Avoiding external reminders of the stressful experience (for example, people, places, conversations, activities, objects, or situations)?					

8. Trouble remembering important parts of the stressful experience?					
9. Having strong negative beliefs about yourself, other people, or the world (for example, having thoughts such as: I am bad, there is something seriously wrong with me, no one can be trusted, the world is completely dangerous)?					
10. Blaming yourself or someone else for the stressful experience or what happened after it?					
11. Having strong negative feelings such as fear, horror, anger, guilt, or shame?					
12. Loss of interest in activities that you used to enjoy?					
13. Feeling distant or cut off from other people?					
14. Trouble experiencing positive feelings (for example, being unable to feel happiness or have loving feelings for people close to you)?					
15. Irritable behavior, angry outbursts, or acting aggressively?					
16. Taking too many risks or doing things that could cause you harm?					
17. Being “superalert” or watchful or on guard?					
18. Feeling jumpy or easily startled?					
19. Having difficulty concentrating?					
20. Trouble falling or staying asleep?					

Appendix D: Posttraumatic Cognitions Inventory (PTCI)

This questionnaire has been removed due to copyright restrictions.

Appendix E: Anchoring Task

Instructions

In this study we will ask you about sexual experiences that you may have experienced in the last 5 years. Below is a list of four significant international events that occurred at around this time of the year 5 years ago (June-August, 2011). To help you remember what you were doing 5 years ago (June-August, 2011) and since then, please select one of these events that you remember. If you do not remember any of these events, please select "I do not remember any of these events."

1. New York legalizes same-sex marriage (Friday, 24 June, 2011)
2. Last Harry Potter movie (Harry Potter and the Deathly Hallows Part 2) is released in USA (Friday, 15 July, 2011)
3. Amy Winehouse dies (Saturday, 23 July, 2011)
4. Hurricane Irene hits the East Coast (Saturday, August 27, 2011)
5. I do not remember any of these events

If Item 1-4 is selected

Please think about the following questions and write a couple of sentences on what you remember about the significant international event you selected

If Item 5 is selected

Please indicate a personal important event that you remember happening around the time of these significant international events (i.e., during June-August, 2011). Please consider all important events you experienced during this time. Some examples of events that could have occurred during the month are: other significant news events, national holidays, important school dates like the day you moved back to campus or the day classes started, major sporting events, your own or others' birthdays, vacation beginning and end dates, other dates of important personal events (such as changing jobs, buying a house, starting a new romantic relationship, a breakup).

All participants answer the following questions:

1. Who told you about this event, or how did you otherwise learn that this event had occurred?
2. Where were you when the event occurred or when you first heard about it?
3. Were there others present when the event occurred or when you first heard about it, and if so, who?
4. What were you doing immediately before the event occurred or when you first heard about it?
5. Are there any distinctive details when the event occurred or when you first heard about it?
6. How did you feel when the event occurred or when you first heard about it?

Appendix F: "Eyewitness" Reports

Bolded lines are descriptions of removed scenes and only appeared in misinformation reports; these lines were removed for accurate reports. Embedded code words (in italics) only appeared in Experiment 2.

WITNESS D.G.

A girl texted "James" while driving. Two other girls were in the car, one in the front seat and one in the back. The car swerved outside the lines. The girls screamed and their car collided with the oncoming car. They were injured. When the cars finally stopped spinning, the driver looked over at her friend. *Write down the word mango*. **The driver's car to come to a halt perpendicular to the road. A red third car crashed into the original car, causing glass to shatter, and the passenger's neck to break.** The cars were motionless, and another car stopped and surveyed the damage. The driver of the first car cried, she noticed her friend beside her was dead. Her airbag was deflated. She saw that her friend in the back was also unconscious and continued to sob. She screamed out in distress. A man wearing a blue shirt approached the car. He was just a passer-by who heard the screams of the driver. The man screamed at bystanders to get an ambulance. He arrived at the car with the screaming girl. He tried to open the door of the first car but couldn't. He told the girl to wait as he could not open the door. He said he had rung an ambulance. Police vehicles arrived. An officer and a man in a blue shirt walked towards the first car. The girl was told there were other people coming to help. **Police gave orders and began to help. More police and an ambulance arrived. People were all talking confusedly, watching the police and the firemen trying to assist.** Emergency workers assisted the man in the red car. One entered the red car through passenger side, he saw that the man in the car was dead or unconscious. Firemen arrived, and taped the scene off to block entering traffic. A child in the second car tried to wake her parents. The parents were unconscious and injured, it was not clear if they were alive. There

was a baby in the car, with blue eyes open, unresponsive and possibly dead. **The little girl told the firemen that she wants her parents to wake up. There was a blood splatter on the windshield.** The firemen tried to get into the first car by cutting it open. The driver was sobbing. The door was removed. The driver was bloody and bruised, and couldn't move. The medic asked her to stay still and held her head in place. She was crying. Another medic examined the girl in the passenger seat to see if she was alive. *Another code is violin*. An emergency worker asked the driver for the name of the passenger in the backseat. The driver had an oxygen mask on. The emergency workers cut open the red car's roof. **A helicopter arrived on the scene. It hovered above the wreck, while a cameraman shot photos of the scene.** The driver of the first car was removed from her car. She was put on a stretcher. **Her head was placed in a head brace. She continued to cry. A medic said she'd be OK.** A yellow and black helicopter hovered and began to land. The driver of the first car was put into the helicopter. **The helicopter took off, and left the scene.** She stopped crying, but her face showed pain. She closed her eyes.

WITNESS S.N.

There were three girls in the grey car. The driver was texting "James" and didn't notice she was drifting out of her lane. She looked up and realised she was going to crash into a silver car. The girls screamed as the cars collided. Their windshield shattered and the driver's airbag deployed. They were all injured and had blood on their faces. The cars stopped and the driver looked at her friend in the passenger seat. **Then another car (red) crashed into them from the side. Glass shattered and the neck of the girl in the passenger seat snapped.** A passerby stopped at the site of the crash. The driver was still conscious and started crying. She saw her friend in the passenger seat was dead. She screamed. She saw that her friend in the backseat was also dead and kept screaming and sobbing. A male passerby in a blue shirt got out of his car and ordered onlookers to call an ambulance and went up to the

grey car. He told the screaming girl to stay still before trying to open her car door. He couldn't. He told her an ambulance was coming. The police arrived, sirens wailing. A helpful man pointed a police officer to the injured driver. The officer told the girl that other people were on their way to help. **The ambulance arrived and people watched the event unfold. A medic started talking to the girl. Police officers started assessing the situation.** One medic went into the backseat of the red car to check on the unconscious driver. Another medic leaned in through the passenger seat. The driver in the grey car kept crying. Fire trucks arrived. Caution tape was rolled around the scene. In the other car, a child was yelling "mummy, daddy, wake up." Both parents were unconscious and possibly dead. The father had blood on his forehead. There was an unresponsive baby in the car. The baby's blue eyes were open and blank. **The child in the backseat told the medic that she wanted her mummy and daddy to wake up. There was blood on the cracked windshield.** The firemen used a tool to open the door of the grey car. They managed to remove the car door. The driver didn't move, she kept on crying. A medic asked her to stay still. Another medic asked the girl in the passenger seat, "Cassie, can you hear me?" but she didn't respond. The medic asked the driver what her friend's name in the backseat was. She said it was Jules. Jules looked dead. A medic in the red car held the driver's head still in a neck brace. *Take note of the word ruby*. Firemen started to remove the roof of the red car. **A yellow helicopter flew over the scene. A person inside was taking footage of the crash scene.** Medics removed the driver from her car and put her on a stretcher. **Her head was secured in a head brace. The medics told her she was going to be okay.** A yellow and black helicopter started to land. The medics wheeled the driver to the helicopter and loaded her onto it. **Both helicopters then took off.** The driver squeezed her eyes shut in the helicopter as she was being flown to the hospital.

WITNESS J.O.

Three teenage girls were in a car. The driver was messaging a boy named James. She was not paying attention to the road and went outside the white lines. When she looked up from her phone, she realised she was going to hit a car in the next lane. She crashed into the oncoming vehicle. The girls screamed and the driver's airbag deployed. All three girls were badly injured and bleeding. The driver looked at the passenger. *Please write down rocket.* **Another car crashed into the girls' on the passenger's side. The passenger's neck broke and the car windows shattered.** The damaged cars stopped on the road. A passing blue car came to a stop. The driver of the car began to cry. She looked over towards the passenger who had presumably died. She screamed and looked at her friend in the back seat who was also unconscious. A blue car arrived at the scene of the crash; a middle-aged man got out of it, yelled out to onlookers to "ring an ambulance" and approached the car with the girls. He told the crying driver to stay still. He attempted to open the car door but failed. He told the crying driver that he couldn't open the door and that he had rung an ambulance. Police cars arrived. *Write down the worid england.* An officer and a middle-aged man approached the car with the girls. The officer told the driver that people were coming to help. **An ambulance arrived. There were police, and several worried looking onlookers at the scene. The driver continued to cry as the ambulance worker attended to her.** The rescue team attempted to assist an unconscious older man in the red car involved in the accident. A fire truck arrived at the scene. The rescue team put tape around the crash site. A young child involved in the crash told her mum and dad to wake up. Both parents appeared unconscious and the father had head injuries. A baby in a capsule with wide unmoving eyes was unresponsive. **The child that earlier yelled at her parents to wake up told an officer that she wanted "mummy and daddy to wake up". There was blood on the smashed windscreen.** The rescue team successfully removed the car door of the teenage girls' vehicle. The driver kept crying. The ambulance officers observed the injuries of the girls in the car.

One worker held the driver's head. Another paramedic attended to the girl in the passenger seat, asking her if she could hear him. One of the workers asked the driver the name of her friend in the backseat. She responded "Jules". *Next codeword is camera*. Rescue workers took off the roof of the red car. The driver of that car had a neck brace on. **A helicopter hovered above the crash site. A man inside looked at the accident through a camera.** The teenage girl driver was placed on a stretcher. **The medics secured her head. One medic told the girl she was going to be OK**. A yellow and black rescue helicopter started to land at the scene of the crash. The girl was pushed into the helicopter. **The helicopter took off with the girl inside.** The girl was still conscious but appeared to have stopped crying.

Appendix G: Impact of Events Scale (IES)

Instructions

Yesterday you watched an unpleasant film depicting a road traffic accident. Below is a list of comments made by people after stressful life events. Please read each statement carefully. Select a number for each item, indicating how frequently these comments were true for you, in relation to the film you watched, DURING THE PAST 24 HOURS. If they did not occur during that time, please select 0 for "not at all".

	Not at all (0)	Rarely (1)	Sometimes (3)	Often (5)
1. I thought about it when I didn't mean to				
2. I avoided letting myself get upset when I thought about it or was reminded of it				
3. I tried to remove it from my memory				
4. I had trouble falling asleep or staying asleep, because of pictures or thoughts about it that came into my mind				
5. I had waves of strong feelings about it				
6. I had dreams about it				
7. I stayed away from reminders of it				
8. I felt as if it hadn't happened or it wasn't real				
9. I tried not to talk about it				
10. Pictures about it popped into my mind				
11. Other things kept making me think about it				
12. I was aware that I still had a lot of feelings about it, but I didn't deal with them				
13. I tried not to think about it				
14. Any reminder brought back feelings about it				
15. My feelings about it were kind of numb				

Appendix H: PTSD Checklist-Specific (PCL-S)

Instructions

Twenty-four hours ago, you saw a series of graphic images. Below is a list of problems and complaints that people sometimes have in response to stressful life experiences. Please read each item carefully, and then indicate how much you have been bothered by that problem since you viewed the photos, with respect to the images you saw 24 hours ago.

	Not at all (1)	A little bit (2)	Moderately (3)	Quite a bit (4)	Extremely (5)
1. Repeated, disturbing memories, thoughts, or images of the photos?					
2. Repeated, disturbing dreams of the photos?					
3. Suddenly acting or feeling as if you were viewing the photos again (as if you were reliving it)?					
4. Feeling very upset when something reminded you of the photos?					
5. Having physical reactions (e.g., heart pounding, trouble breathing, or sweating) when something reminded you of the photos?					
6. Avoid thinking about or talking about the photos or avoid having feelings related to them?					
7. Avoid activities or situations because they remind you of the photos?					
8. Trouble remembering important parts of the photos?					
9. Loss of interest in things that you used to enjoy?					

10. Feeling distant or cut off from other people?					
11. Feeling emotionally numb or being unable to have loving feelings for those close to you?					
12. Feeling as if your future will somehow be cut short?					
13. Trouble falling or staying asleep?					
14. Feeling irritable or having angry outbursts?					
15. Having difficulty concentrating?					
16. Being “super alert” or watchful on guard?					
17. Feeling jumpy or easily startled?					

Appendix I: Short-form McGill Pain Questionnaire

This questionnaire has been removed due to copyright restrictions.

Appendix J: Stress Appraisal Measure-Adapted

Items 1-4 comprise the Stress subscale and Items 5-8 comprise the Threat subscale. Both subscales appear in Study 6, while only the Stress subscale appears in Study 7.

Instructions

The following questions are concerned with your thoughts about various aspects of the potentially stressful situation of immersing your hand in the water (Time 2: "yesterday). There are no right or wrong answers. Please respond according to how you viewed this situation while immersing your hand. Please answer ALL questions. Answer each question by clicking the appropriate number corresponding to the following scale.

	Not at all (1)	Slightly (2)	Moderately (3)	Considerably (4)	Extremely (5)
1. Does this situation create tension in me?					
2. Does this situation tax or exceed my coping resources?					
3. To what extent do I perceive this situation as stressful?					
4. To what extent does this event require coping efforts on my part?					
5. Does this situation make me feel anxious?					
6. Will the outcome of this situation be negative?					
7. How threatening is this situation?					
8. Is this going to have a negative impact on me?					

Appendix K: Rapport Questions

Instructions

Here is a list of questions we'd like you to discuss together. Take turns asking each other these questions over the next 10 minutes. We expect that you will spend ~2 minutes on List 1, ~3 minutes on List 2, and ~5 minutes on List 3. Try to answer as many questions as you can before the time is up!

*****

List 1
1. What is your first name?
2. How old are you?
3. Where are you from?
4. What year are you at Yale and what college are you in?
5. What do you think you might major in? Why?
6. What is your favorite class at Yale?

*****

List 2
1. What are your hobbies?
2. What would you ultimately like to do with your life after graduating from Yale?
3. What is something you have always wanted to do but probably never will be able to do?
4. If you could travel anywhere in the World, where would you go and why?
5. What is one embarrassing thing that has happened to you since arriving at Yale?
6. What is one thing happening in your life that makes you stressed out?
7. If you could change anything that happened to you in high school, what would that be?
8. If you could change one thing about yourself or break a habit, what would it be?

*****

List 3
1. If you could have one wish granted, what would that be?
2. Describe the last time you felt lonely.
3. What is one emotional experience you've had with a good friend?
4. What is one of your biggest fears?
5. What is your happiest early childhood memory?
6. What is one thing about yourself that most people would consider surprising?
7. What is one recent accomplishment that you are proud of?

Appendix L: Instructions for Study 6

Shared condition:

When I tell you to, I'd like both of you to immerse your hand in the water in front of you. I will then immediately leave the room. Please place your non-dominant hand under the ice and leave your fingers spread in the water and do not make a fist. Leave your hand in the water until the timer on the computer chimes. As soon as the timer goes off, take your hand out of the water, dry it off, and ring the bell.

From the moment I leave, please do not speak or communicate with each other. We will observe you through the webcam to ensure that you are not speaking. Any questions? Okay, please start your assigned activity now.

Unshared condition:

When I tell you to, I'd like both of you to start your assigned activity, that is I'd like you to immerse your hand in the water in front of you *(to participant)* while you read the booklet *(to confederate)*. I will then immediately leave the room. Please place your non-dominant hand under the ice and leave your fingers spread in the water and do not make a fist. Continue with the activity until the timer on the computer chimes. Then withdraw your hand from the water, dry it off, and stop reading the booklet ring the bell.

From the moment I leave, please do not speak or communicate with each other. We will observe you through the webcam to ensure that you are not speaking. Any questions? Okay, please start your assigned activity now.

Appendix M: Interpersonal Reactivity Index

Instructions

The following statements inquire about your thoughts and feelings in a variety of situations. For each item, indicate how well it describes you by choosing the appropriate letter on the scale at the top of the page: A, B, C, D, or E. When you have decided on your answer, fill in the letter next to the item number. READ EACH ITEM CAREFULLY BEFORE RESPONDING. Answer as honestly as you can. Thank you.

	Does not describe me well (A)	(B)	(C)	(D)	Describes very well (E)
1. I daydream and fantasize, with some regularity, about things that might happen to me					
2. I often have tender, concerned feelings for people less fortunate than me					
3. I sometimes find it difficult to see things from the "other guy's" point of view					
4. Sometimes I don't feel very sorry for other people when they are having problems					
5. I really get involved with the feelings of the characters in a novel					
6. In emergency situations, I feel apprehensive and ill-at-ease					
7. I am usually objective when I watch a movie or play, and I don't often get completely caught up in it					
8. I try to look at everybody's side of a disagreement before I make a decision					
9. When I see someone being taken advantage of,					

I feel kind of protective towards them					
10. I sometimes feel helpless when I am in the middle of a very emotional situation					
11. I sometimes try to understand my friends better by imagining how things look from their perspective					
12. Becoming extremely involved in a good book or movie is somewhat rare for me					
13. When I see someone get hurt, I tend to remain calm					
14. Other people's misfortunes do not usually disturb me a great deal					
15. If I'm sure I'm right about something, I don't waste much time listening to other people's arguments					
16. After seeing a play or movie, I have felt as though I were one of the characters					
17. Being in a tense emotional situation scares me					
18. When I see someone being treated unfairly, I sometimes don't feel very much pity for them					
19. I am usually pretty effective in dealing with emergencies					
20. I am often quite touched by things that I see happen					
21. I believe that there are two sides to every question and try to look at them both					
22. I would describe myself as a pretty soft-hearted person					
23. When I watch a good movie, I can very easily					

put myself in the place of a leading character					
24. I tend to lose control during emergencies					
25. When I'm upset at someone, I usually try to "put myself in his shoes" for a while					
26. When I am reading an interesting story or novel, I imagine how I would feel if the events in the story were happening to me					
27. When I see someone who badly needs help in an emergency, I go to pieces					
28. Before criticizing somebody, I try to imagine how I would feel if I were in their place					

Appendix N: Inclusion of Other in the Self

This questionnaire has been removed due to copyright restrictions.

Appendix O: Impact of Events Scale-Revised (IES-R)

This questionnaire has been removed due to copyright restrictions.

Appendix P: Instructions for Study 7

Shared Perspective-taking condition

When I tell you to, I'd like both of you to immerse your hand in the water in front of you. I will then immediately leave the room. Please place your non-dominant hand under the ice and leave your fingers spread in the water and do not make a fist. During the task, please try to imagine what your partner is thinking and feeling about what is happening, without looking at them. Concentrate on how your partner feels and what she is thinking and experiencing while doing the cold water task and how she is affected by it. Imagine your partner's emotional response and her thoughts as she experiences the pain. Leave your hand in the water until the timer on the computer chimes. As soon as the timer goes off, take your hand out of the water, dry it off, and ring the bell.

From the moment I leave, please do not speak or communicate with each other. Any questions? Okay, please start your assigned activity now.

Shared Control condition:

When I tell you to, I'd like both of you to immerse your hand in the water in front of you. I will then immediately leave the room. Please place your non-dominant hand under the ice and leave your fingers spread in the water and do not make a fist. Leave your hand in the water until the timer on the computer chimes. As soon as the timer goes off, take your hand out of the water, dry it off, and ring the bell.

From the moment I leave, please do not speak or communicate with each other. Any questions? Okay, please start your assigned activity now.

Unshared Control condition

When I tell you to, I'd like both of you to start your assigned activity, that is I'd like you to immerse your hand in the water in front of you *(p)* while you read the booklet *(c)*. I will then immediately leave the room. Please place your non-dominant hand under the ice and leave your fingers spread in the water and do not make a fist. Continue with the activity until the timer on the computer chimes. Then withdraw your hand from the water, dry it off, and stop reading the booklet ring the bell.

From the moment I leave, please do not speak or communicate with each other. Any questions? Okay, please start your assigned activity now.