The Coherence of Traumatic and Non-Traumatic Memories:

A Tale of Equivalence

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Abstract

There is contention in the scientific literature about the mechanisms by which our memories for traumatic events operate. In one view, the extremely negative nature of traumatic events results in incomplete encoding, leaving memories for these events incoherent. In this view, incoherence promotes distress and therefore must be targeted over the course of treatment to enable recovery. In another view, traumatic memories operate using the same basic set of mechanisms as memories for non-traumatic events; traumatic memories are not uniquely incoherent, and evidence does not support the idea that treatment boosts coherence in a way that supports recovery. But across the literature, several counter-explanations hinder our interpretation of the existing findings. Over five experiments, we[1] addressed these counter-explanations. We found little evidence to suggest people's memories for traumatic events were incoherent, both in absolute terms and in relation to memories for non-traumatic events. Across our experiments, coherence did not vary in a systematic way depending on whether a memory was traumatic or non-traumatic. Moreover, we found evidence to suggest the way people recalled a memory affected their sense of how coherent that memory was. We also found that repeatedly recalling a memory, as happens in some therapies for posttraumatic stress disorder (PTSD), did not boost coherence. Instead, the data suggest repeated recall prevented a sense of lost coherence seen in memories not repeatedly recalled. Furthermore, we consistently found that memory coherence was not related to the degree of posttraumatic stress a person reported about that event. Mini meta-analyses on the data gathered across these five experiments, and their associated pilot studies, converged on the conclusions

[1] Here, my use of the word "we" reflects that, although the research in this thesis is my own, I conducted it in a lab where I supervised a team comprised of undergraduate and Honours students. I also received advice and direction from post-doctoral fellows in the lab, my supervisors, and co-authors. For those reasons, I often use the words "our" and "we" in this thesis. Elsewhere in this thesis, I use the word "we" in a different sense; for example, to refer to what is or is not known in the wider scientific community.

that a) traumatic memories are just as coherent as their non-traumatic counterparts, and b) there is no relation between coherence and symptomatology. Considered together, these findings are consistent with a basic mechanisms account of traumatic memories, and have implications for our conceptualisation of PTSD and the therapies designed to treat it.

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Outline of the Thesis

The first chapter of this thesis provides an overview of the literature of autobiographical memories, posttraumatic stress disorder, and memory for traumatic events. The chapter then outlines the rationale for the manuscripts presented in this thesis. Chapters 2 through 4 present the results of our research in the form of three papers published in, or in preparation for submission to, peer-reviewed academic journals. Chapter 2 presents our first paper, published in the *Journal of Applied Research in Memory and Cognition*. Chapter 3 presents our second paper, published in *Clinical Psychological Science*. Chapter 4 presents our third paper, a manuscript in preparation for submission. Chapter 5 presents two mini meta-analyses of data from this thesis. Finally, Chapter 6 integrates the findings reported in these chapters and discusses their contributions to the literature.

Chapter 1: Introduction

People tend to think of memory as a storehouse of our personal experiences: one we can draw on to recollect specific events from our lives. When we recall these "autobiographical memories," we typically feel a sense of reliving; we bring to mind pieces of visual imagery from the original event, as well as some of the thoughts and feelings we experienced at the time (Brewer, 1986). Evidence suggests recalling these memories helps us to bond with others, guides our planning for the future, and informs our identities (Pillemer, 1992; Bluck, 2003). And it is not just memories of positive events we bring to mind to do these things; people also report recalling memories for traumatic events in these ways. When asked about the impact of traumatic events on their lives, people report their "traumatic memories" represent turning points and become lenses through which they make sense of our experiences, in turn forming central aspects of their identities (Berntsen & Rubin, 2008).

But the way we encode and subsequently recall memories of traumatic events are contentious issues in the psychological literature. In one corner of the literature, people's traumatic memories are incoherent—fragmented and jumbled, coming to mind in bits and pieces with crucial parts of the memory missing (Brewin, 2001; Foa & Riggs, 1993; Freud & Breuer, 1895/2004; Janet, 1919/1925; Nadel & Jacobs, 1998; van der Kolk & Fisler, 1995; Whitfield, 1995). Theories seeking to explain this apparent incoherence suggest traumatic memories are encoded, processed, and recalled using a set of "special mechanisms"; mechanisms that operate differently under the conditions produced by traumatic versus non-traumatic events (Brewin et al., 1996, 2010; Ehlers & Clark, 2000; Foa & Kozak, 1985, 1986; Foa et al., 1989; van der Kolk, 1987, 1994, 1997). In another corner of the literature, the evidence suggests people's traumatic memories are just as coherent as their memories for other emotional and important, but non-traumatic, events

(Porter & Birt, 2001; Porter & Peace, 2007; Rubin, 2011; Rubin et al., 2004; Rubin et al., 2016; Wenzel et al., 2004). In this corner, differences in the qualities of traumatic versus non-traumatic memories can be resolved by drawing on our knowledge of the basic set of mechanisms thought to operate on our autobiographical memories more broadly (Rubin, Berntsen, & Bohni, 2008; Rubin, Boals, & Berntsen, 2008; Rubin et al., 2011). To evaluate these claims about traumatic memories, it is important to first establish what these mechanisms are.

Autobiographical Memory

The study of autobiographical memories pre-dates psychology. Historians and biographers have relied on autobiographical memories to document the past for thousands of years, and early theorising about the process of encoding and recalling events from our lives dates right back to Aristotle (Brennan, 2014; Robinson, 1986). For example, in *De Memoria*, Aristotle noted:

> just as no impression would be formed if the movement of the seal were to impinge on running water; while there are others in whom, owing to the receiving surface being frayed, as happens to (the stucco on) old (chamber) walls, or owing to the hardness of the receiving surface, the requisite impression is not implanted at all. Hence both very young and very old persons are defective in memory; they are in a state of flux, the former because of their growth, the latter, owing to their decay. (Aristotle, 350 b.c/2010, p. 88)

Here, Aristotle acknowledges two phenomena now widely studied by psychological scientists. First, the apparent inability of young children to encode lasting memories, now referred to as “infantile amnesia” (Freud, 1916/1966; Miles, 1895; Sheingold & Tenney, 1982). And second, the evidence of a decline in our abilities to encode and retrieve memories as we age (Light et al., 2000; Rönnlund et al., 2005; Salthouse, 1991).

Autobiographical memory was not systematically examined until the late 1800s, when Galton studied his own and others' autobiographical recollections (Galton, 1880). Galton asked people questions about the quality of their mental imagery when bringing a memory to mind. He then categorised people's imagery abilities into high, mediocre, and low, depending on their answers to these questions, and concluded their abilities varied widely. For example, on the one hand, someone who responded "I can see my breakfast table or any equally familiar thing with my mind's eye, quite as well in all particulars as I can do if the reality is before me" was categorised as having high mental imagery abilities (Galton, 1880, p. 310). On the other hand, someone who responded "My powers are zero. To my consciousness there is almost no association of memory with objective visual impressions. I recollect the breakfast table, but do not see it" was categorised as having low mental imager abilities (Galton, 1880, p. 310).

Shortly thereafter, Colegrove published *Individual Memories* in which he reported his findings from studies of people's memories for different types of events (Colegrove, 1899). Colegrove plotted the frequency of pleasant and unpleasant memories across the lifespan by both sex and race, finding, for example, that females report a greater quantity of unpleasant memories than do males. Colegrove also observed that most people interviewed about Abraham Lincoln's assassination reported vivid memories for the event, despite it having happened over 30 years prior; memories we would now call "flashbulb memories" (Brown & Kulik, 1977).

In contrast, Freud claimed memory was a way to explain and treat people's neuroses (Freud, 1910). For example, Freud wrote "The nature of the symptoms became clear through their relation to the scene which caused them. They were, to use the technical term, "determined" by the scene whose memory traces they embodied" (Freud, 1910, p. 185) and "Our hysterical patients suffer from reminiscences. Their symptoms are

the remnants and the memory symbols of certain (traumatic) experiences" (Freud, 1910, p. 187). Despite a lack of good scientific evidence for these ideas, Freud's theories have had a lasting impact on the field of autobiographical memory—particularly memories of traumatic events (for a review, see Kihlstrom, 2006).

As the field shifted towards a focus on behaviourism in the early 20th Century, psychological scientists moved away from studying autobiographical memory (Brennan, 2014). It was not until the cognitive revolution of the 1950s and 60s that interest was seemingly reignited in memory research. A short time later, Tulving distinguished between memories for specific events ("episodic memories") and memories for facts and knowledge about the world ("semantic memories"; Tulving, 1972). Using this distinction, autobiographical memories form a subset of episodic memories. This distinction, although now more nuanced, continues to influence psychological research (see for example, Rubin & Umanath, 2015). Scientists published about the types of memories elicited by different word prompts (Crovitz & Schiffman, 1974; Robinson, 1976), the distribution of memories across the lifespan (Franklin & Holding, 1977), and the retention and accuracy of autobiographical memories over time (Linton, 1975). It was at this time the study of memories for negative experiences became more prominent. For example, in one study, people's reports of "stressful events" declined over an 18-month period, suggesting people are less likely to report experiences of stressful events over time; perhaps due to memory decay (Uhlenhuth et al., 1977).

From here, the field of autobiographical memory boomed: the number of peer-reviewed articles listing autobiographical memory as a keyword increased dramatically between 1980 and 2010 (Berntsen & Rubin, 2012). This growth paralleled the significant progress in our understanding of the processes by which memories are constructed.

The Constructive Nature of Autobiographical Memory

Contrary to the common misconception that our memories are stored as complete representations of events, evidence now suggests they are constructed at retrieval as the various components of a memory are activated, integrated, and recalled (for reviews, see Conway & Howe, 2022; Moskovitch et al., 2016). Bartlett was the first to suggest memory is constructive in *Remembering,* proposing that "remembering is not a completely independent function, entirely distinct from perceiving, imaging, or even from constructive thinking, but it has intimate relations with them all" (Bartlett, 1932, p. 13).

To test this idea, Bartlett examined people's recall of material that as closely as possible resembled the types of material encountered in real life. The most widely known of these experiments is Bartlett's study on the reproduction of *The War of the Ghosts* story, in which British subjects read and then repeatedly retold the following piece of Native American Folklore:

> One night two young men from Egulac went down to the river to hunt seals, and while they were there it became foggy and calm. Then they heard war-cries, and they thought: "Maybe this is a war-party". They escaped to the shore, and hid behind a log. Now canoes came up, and they heard the noise of paddles, and saw one canoe coming up to them. There were five men in the canoe, and they said: "What do you think? We wish to take you along. We are going up the river to make war on the people".
>
> One of the young men said: "I have no arrows".
>
> "Arrows are in the canoe", they said.
>
> "I will not go along. I might be killed. My relatives do not know where I have gone. But you", he said, turning to the other, "may go with them."
>
> So one of the young men went, but the other returned home.

> And the warriors went on up the river to a town on the other side of Kalama. The people came down to the water, and they began to fight, and many were killed.
>
> But presently the young man heard one of the warriors say: "Quick, let us go home: that Indian has been hit". Now he thought: "Oh, they are ghosts." He did not feel sick, but they said he had been shot.
>
> So the canoes went back to Egulac, and the young man went ashore to his house, and made a fire. And he told everybody and said: "Behold I accompanied the ghosts, and we went to fight. Many of our fellows were killed, and many of those who attacked us were killed. They said I was hit, and I did not feel sick".
>
> He told it all, and then he became quiet. When the sun rose he fell down.
>
> Something black came out of his mouth. His face became contorted. The people jumped up and cried.
>
> He was dead. (Bartlett, 1932, p. 65)

After reading the story twice, subjects were asked to repeatedly retell the story from memory over various delays. When comparing subjects' retellings to the original story, Bartlett noted they varied greatly: he reported the retellings were shorter and less accurate, and in the instances in which a detail of the story presumably did not fit with a subject's knowledge or beliefs, the detail was either omitted from the retelling completely, or changed so it made better sense to the subject. Bartlett concluded that when people encounter an unfamiliar story or idea, they put in effort to assimilate that story or idea with their current beliefs, thoughts, and feelings, so it makes better sense to them. That is, they put in "effort after meaning."

More specifically, Bartlett observed that subjects would transform the story in similar ways. Sometimes these transformations arose when subjects left out details Bartlett thought they might have deemed unimportant to the story or confusing, such as

the names of the towns. This process results in a more concise and more understandable story, and has since been called “levelling” (Allport & Postman, 1945). But other times, people would highlight or feature specific details of the story, a process now called “sharpening” (Allport & Postman, 1945). Certain details might have been sharpened because they were salient, helped to explain the story (created meaning), or fit with the reteller’s schema. For example, when retelling *The War of the Ghosts*, Bartlett noted some subjects elaborated on certain parts of the story, dramatising the death at the end, or recasting the ghosts not as supernatural beings but instead as a clan. These findings have since been replicated under stricter experimental conditions, using specified instructions and holding the conditions at encoding and recall constant (Bergman & Roediger, 1999). Furthermore, examples of changes consistent with levelling and sharpening have been found in people’s retellings of rumours and their drawings of previously seen images (Allport & Postman, 1945; Carmichael et al., 1932; Holzman & Gardner, 1960). Taken together, these findings provide support for the idea that memory is a constructive process, but they do not tell us about *how* memories are constructed.

How Memories are Constructed. Neuroscientific evidence suggests the hippocampus is key to the construction of episodic memories. This evidence fits with the idea that shortly after an event, various pieces of sensory information associated with the event are sent from their corresponding locations in the cortex to the hippocampus (for a review, see Moscovitch et al., 2016). As well as these sensory details, the hippocampus is thought to receive spatial and temporal information about the event, and information from the amygdala about the event’s emotional aspects. Evidence suggests these details are bound together to build a single complex representation of the event. To later recall the memory of a specific event, the hippocampus is then thought to reactivate the original details in the parts of the cortex responsible for those details and reconstructs the memory

(Tanaka et al., 2014; for a review see Moscovitch et al., 2016). This reactivation is hypothesised to create the feeling of re-experiencing the feelings, thoughts, and perceptions associated with the event (Conway, 1992; for reviews, see Moscovitch, 1995, 2008). Over time, as the hippocampus repeatedly reactivates the original sensory features of the memory, these patterns of activation are said to become more durable and stronger. It is during this stage—known as "consolidation"—that memories are thought to be converted from a "labile" state, in which they are vulnerable to transformation or destruction, to a more enduring, stable representation (Walker et al., 2003; for reviews, see Lechner et al., 1999; McGaugh, 2000; Squire et al., 2015). More recent evidence, however, supports the possibility memories can re-enter this labile state when they are retrieved; I return to this issue shortly (for a review, see Nader, 2015).

In the case of autobiographical memories specifically, several scientists have sought to explain how this reactivation occurs and results in the retrieval of memories of specific events from our lives. For example, one prominent model proposes autobiographical memories are constructed from knowledge stored in an underlying "autobiographical knowledge base" (Conway, 2005; Conway et al., 2019; Conway & Pleydell-Pearce, 2000). This knowledge base is thought to contain information about different chapters in our lives, general knowledge about events we have experienced repeatedly, and details of single, specific events. Which knowledge is reactivated and retrieved from the autobiographical knowledge base, and how this retrieval happens, is said to depend on the rememberer's current goals: the goals of the "working self."

Evidence suggests retrieval from this "self-memory system" can be either generative or direct (Addis et al., 2012; Conway, 2005; Conway & Pleydell-Pearce, 2000; Harris & Berntsen, 2019; Harris et al., 2015). According to the model, in the case of generative retrieval: 1) a memory cue is generated by the working self, 2) the

autobiographical knowledge base is searched for the various components of an event matching that cue, and 3) the record accessed is checked against the cue's criteria (Burgess & Shallice, 1996; Conway, 1996, 2005; Conway and Loveday, 2010; Conway & Pleydell-Pearce, 2000; Norman and Bobrow, 1979). If this verification is successful, the memory is retrieved; if not, the process begins again (Conway & Pleydell-Pearce, 2000). In contrast, direct retrieval accounts for the spontaneous memories that sometimes come to mind without being specifically searched for (Berntsen, 1996; Conway, 1997, 2005; Conway and Loveday, 2010; Conway & Pleydell-Pearce, 2000; for a review, see Berntsen, 2021). The idea here is that we are constantly exposed to memory cues at varying levels of abstraction which produce patterns of activation across the autobiographical knowledge base (for a review, see Conway, 2001). If at any time one of these patterns of activations matches the working self's goals or beliefs, a memory would automatically be retrieved (Conway, 2005; Conway & Pleydell-Pearce, 2000). This model, then, defines autobiographical memories as patterns of activation across components of events that are recalled only when they match the working self's goals.

In contrast, a more recent model suggests autobiographical memories are not stored and retrieved at all, and are instead constructed at recall (Rubin, 2019). In this model, Rubin proposes memories vary along three dimensions: explicit processes, self-reference, and event memory (Rubin, 2019). Autobiographical memories can be thought of as memories at the high end of all three dimensions; they are explicit, highly self-referent "event memories." That is, our autobiographical memories are mentally constructed scenes of events from our past that we create at recall using basic cognitive processes (Rubin, 2005; Rubin, 2019; Rubin & Umanath, 2015). Most information involved in constructing one of these event memories is said to come from general knowledge of the scene that has been extracted from multiple exposures to that scene

(Rubin & Umanath, 2015). The rememberer places herself in the scene, where she assumes a viewpoint relative to the other contents. In the case of autobiographical memories, the construction of this viewpoint is thought to produce the feeling of reliving people often report experiencing at recall (Rubin & Umanath, 2015).

According to this model, one of the basic cognitive systems that can be involved in the construction of our autobiographical memories is narrative (Rubin, 2005, 2006). Memories from a person's lifetime are organised into overarching life stories as well as stories for life chapters, and memories for individual events tend to be communicated to others as narratives (Rubin, 2006). But Rubin proposes that autobiographical remembering does not require the existence of a narrative; narratives are not an inherent component of an autobiographical memory (Rubin & Umanath, 2015). Instead, narratives might be added during construction, which makes them liable to change between instances of recall.

Reconsolidation of Memories. After a memory has been constructed and retrieved, some evidence fits with the idea that the memory can re-enter a labile state before it is reconsolidated (Nader et al., 2000; Walker et al., 2003; for a review, see Nader, 2015; but see also Elsey et al., 2018). This reconsolidation window is thought to leave our retrieved memories vulnerable to transformation and reconstruction. More specifically, interfering with reconsolidation is thought to dampen the strength of a memory (Nader et al., 2000). Furthermore, additional pieces of information that are activated at retrieval can become associated with the retrieved memory, and in turn be integrated into the reconsolidated memory trace (Alberini & LeDoux, 2013; Hupbach et al., 2007; Lee et al., 2017; Winocur & Moscovitch, 2011). As a result, it is possible that, in some instances when we recall a memory, we integrate unrelated or inaccurate details into that memory during reconsolidation and in turn distort our memories of the past.

Work on the conditions under which reconsolidation takes place, and which of our memories are vulnerable to transformation at retrieval, is ongoing (for reviews, see Nader, 2015; Roediger & Abel, 2022).

Although this constructive nature of memory can have troubling consequences, it is thought to serve many adaptive functions. For example, when our memories are in their vulnerable labile state following recall, we can update our memories to incorporate additional details we have since learned about (for a review, see Lee et al., 2017). Likewise, evidence suggests reactivating the neural traces of details from previous events allows us not only to recall memories but also to combine these details in unique ways to construct simulations of events we might experience in the future (Schacter et al., 2007; Schacter & Madore, 2016). But sometimes this constructive process goes wrong. For example, we can mistakenly come to believe we have experienced events we have, in fact, only simulated (Garry et al., 1996; Goff & Roediger, 1988; Seamon et al., 2006; Thomas & Loftus, 2002).

Furthermore, there is evidence to suggest these same processes apply to memories of non-traumatic and traumatic events alike. For example, people have reported coming to falsely recall aspects of traumatic events, just as they have with non-traumatic events (Nourkova et al., 2004; Southwick et al., 1997; Strange & Takarangi, 2012). And yet, the idea that traumatic events are encoded and retrieved by way of a unique mechanism is rife in the beliefs of both the general public and a subset of mental health professionals (Houben et al., 2021; Ost et al., 2017; Patihis et al., 2014). This belief is also prevalent in certain corners of the psychological literature (see for example Brewin et al., 1996; Foa & Riggs, 1993; van der Kolk, 2014). Before turning to these ideas, we must first define "traumatic events".

Traumatic Events and Memory

Psychological trauma and its effects have been a topic of interest for popular culture from the very beginnings of Greek mythology (Homer, 1598/1974). But psychological trauma did not appear in the scientific literature until the late 1800s, when two surgeons proposed conflicting explanations for the symptoms of "railroad spine"; a term used to refer to the posttraumatic reactions of people involved in railroad accidents (for a review, see van der Kolk et al., 1996). While one of these surgeons claimed the psychological symptoms he was seeing were the result of physical injuries inflicted in the railroad accidents (Erichsen, 1866, 1886), another suggested these symptoms were the result of shock from the accidents (Page, 1885). During this era, most medical professionals continued to believe any posttraumatic psychological symptoms were the result of changes in bodily function (see for example, Da Costa, 1871; Myers, 1870). This belief hit a stumbling block during World War I, when soldiers began to experience psychological symptoms even though they had never been exposed to gunfire (Myers, 1915, 1940). This observation led to a shift in thinking. Physiological injury was no longer necessary to explain subsequent psychological distress; instead, the emotional toll of traumatic events sufficed as an explanation.

This transition from an emphasis on the physical to the psychological brought with it the question: what constitutes a psychologically "traumatic" event? On the one hand, trauma might include only those events that met specific objective criteria, such as events that led to serious bodily injury, or actual or threatened loss of life. On the other hand, trauma might include any event that evoked a specific emotional reaction and led someone to experience certain symptoms of distress. Even over the last 50 years our definition of traumatic events has changed considerably, beginning with the establishment of the diagnosis for posttraumatic stress disorder in the third edition of the Diagnostic and

Statistical Manual of Mental Disorders (DSM-III; American Psychiatric Association, 1980).

Posttraumatic Stress Disorder

Posttraumatic stress disorder (PTSD) is defined as a psychiatric disorder that develops as an extreme psychological reaction to a traumatic event (American Psychiatric Association, 2013). The diagnostic criteria for PTSD are characterised by four clusters of symptoms: a) symptoms in which the traumatic event is repeatedly re-experienced, b) symptoms in which the person avoids stimuli associated with the traumatic event, c) symptoms in which the person's mood or cognitions change for the worse, and d) symptoms in which the person displays changes in their emotional or physical reactivity (American Psychiatric Association, 2013; see Table 1 for full diagnostic criteria). The data suggest most people are resilient to traumatic events, experiencing minimal psychological and physical symptoms (for a review, see Galatzer-Levy et al., 2018). But a small number of people experience symptoms of distress to a clinically-relevant level for an extended period of time, and go on to receive a diagnosis of PTSD. Among those exposed to trauma, the lifetime prevalence of PTSD in high-income countries is 6.9%; in New Zealand, it is slightly higher at 7.6% (Koenen et al., 2017).

The establishment of the PTSD diagnosis in the DSM-III (American Psychiatric Association, 1980) was highly politicised, and anti-war psychiatrists lobbied for the inclusion of a PTSD-like disorder in the wake of the Vietnam War (McNally, 2003a; Scott, 1990). These psychiatrists noted that many soldiers were experiencing prolonged symptoms of distress long after returning from war; others seemed fine when they returned but developed symptoms later (Archibald et al., 1962; Archibald & Tuddenham, 1965; Lifton, 1969; Talbott, 1969). As it stood at the time, there was no existing diagnosis that adequately reflected the nature of this new disorder. Although those on the DSM-III

Table 1

Diagnostic Criteria for Posttraumatic Stress Disorder (DSM-5; American Psychiatric Association, 2013, pp. 271–272)

	Diagnostic Criteria	Ways to meet criteria
A.	Exposure to actual or threatened death, serious injury, or sexual violence (in one or more of the following ways:	1. Directly experiencing the event. 2. Witnessing the event, in person, as others experienced it. 3. Learning the event happened to a close family member or friend—death related event must have been violent or accidental. 4. Experiencing repeated or extreme exposure to aversive details of the event—but not via exposure to electronic media, etc., unless work related).
B.	Experience at least one of the following Intrusion symptoms about the event:	1. Recurrent, involuntary, intrusive distressing memories of the event. 2. Recurrent distressing dreams in which content or affect of the dream are related to the event. 3. Dissociative reactions, such as flashbacks, in which the individual feels or acts as if the event were recurring. 4. Intense or prolonged psychological distress at exposure to cues that symbolise or resemble an aspect of the event. 5. Marked physiological reactions to cues that symbolise or resemble an aspect of the event.
C.	Persistent avoidance of stimuli associated with the event, in at least one of the following ways:	1. Avoidance of, or effort to avoid, distressing memories, thoughts, or feelings about or closely associated with the event. 2. Avoidance of, or effort to avoid, external reminders that arouse distressing memories, thoughts, or feelings about or closely associated with the event.
D.	Negative alterations in cognition and mood associated with the event, in at least two of the following ways:	1. Inability to remember an important aspect of the event (not due to factors such as head injury, alcohol, or drugs).

Table 1 (continued)

		2. Persistent and exaggerated negative beliefs or expectations about oneself, others, or the world. 3. Persistent, distorted cognitions about the cause or consequences of the event that lead the individual to blame himself/herself or others. 4. Persistent negative emotional state. 5. Markedly diminished interest or participation in significant activities. 6. Feelings of detachment or estrangement from others. 7. Persistent inability to experience positive emotions.
E.	Marked alterations in arousal or reactivity associated with the event, in at least two of the following ways:	1. Irritable behaviour and angry outburst (without provocation)—typically verbal or physical aggression toward people or objects. 2. Reckless or self-destructive behaviour. 3. Hypervigilance. 4. Exaggerated startle response. 5. Problems with concentration. 6. Sleep disturbance.

Note. Symptoms must have persisted for more than one month and caused clinically significant distress or impairment in functioning. Symptoms must not be because of substances or other medical conditions.

task force were initially hesitant to include a diagnosis tied to a specific event, they were eventually persuaded on the basis that these symptoms of distress were not unique to veterans and were also experienced by people after other traumatic events, such as sexual assault or natural disasters (McNally, 2003a).

Across its various editions, the DSM has taken different approaches to defining trauma. Traumatic events were first defined in the DSM-III as "recognisable stressor[s] that would evoke significant symptoms of distress in almost everyone" (American Psychiatric Association, 1980, p. 238). In this first attempt at defining trauma, the emphasis was on events that were "outside the range of usual human experience" (American Psychiatric Association, 1980, p. 236). But these criteria were criticised for

several reasons, one of which was that they were largely unhelpful for identifying whether an event is traumatic (Weathers & Keene, 2007). How are clinicians to know what constitutes a "recognisable stressor," or what meets the criterion of "outside the range of usual human experience?" (American Psychiatric Association, 1980, pp. 236–238).

As a result of these issues, the DSM-IIIR expanded on these criteria, providing specific examples of the types of events that would constitute trauma, such as:

> serious threat to one's life or physical integrity; serious threat or harm to one's children, spouse, or other close relatives and friends;… seeing another person who has recently been, or is being, seriously injured or killed as the result of an accident or physical violence. (American Psychiatric Association, 1987, p. 250)

In this edition of the DSM, the definition of trauma was also expanded to include "learning about a serious threat or harm to a close friend or relative," and specified that traumatic events are "usually experienced with intense fear, terror, and helplessness" (American Psychiatric Association, 1987, pp. 247–248).

When the definition of a traumatic event was again revised in the DSM-IV, the criteria for what constitutes a trauma were split into two components (American Psychiatric Association, 1994, pp. 427–428). First, the event had to involve "actual or threatened death or serious injury, or a threat to the physical integrity of self or others," and second, the person experiencing the event had to have had a response at the time of the event that "involved intense fear, helplessness, or horror" (American Psychiatric Association, 1994, pp. 427–428). The addition of this second component led to criticism that the DSM was moving away from using objective criteria to define trauma and toward a more subjective approach that puts emotional reactions at the core of the definition (McNally, 2004; Weathers & Keene, 2007).

This subjective component of the definition was removed in the DSM-5 and the criteria instead specified four ways in which one might be exposed to "actual or threatened death, serious injury, or sexual violence" that would constitute a trauma (see Table 1; American Psychiatric Association, 2013).[2]

Over the years following its establishment in the DSM, the PTSD diagnosis has received a lot of critical attention (Rosen, 2004). As the assumptions and theories underlying PTSD have undergone scrutiny, little about the diagnosis has escaped criticism. Critics have been especially concerned with the diagnosis's reliance on the experience of a specific triggering event (McNally, 2003a, 2004). Indeed, PTSD is unique among psychiatric disorders in that it is the only disorder where one must experience a certain type of event to qualify for a diagnosis. The existence of this criterion means that regardless of impairment and symptomatology, if a person has not experienced an event that satisfies this criterion, they cannot be diagnosed with PTSD (McNally, 2004).

Issues With Criterion A. There are at least three problems with this assumed causal relationship between traumatic events and PTSD symptomatology. First, events that satisfy the diagnostic criteria for PTSD range from experiencing sexual violence and being kidnapped to learning about someone else's assault (American Psychiatric Association, 2013). With such a wide range of events believed to play a causal role in PTSD symptomatology, it is difficult to identify the disorder's underlying causal mechanisms (McNally, 2003a).

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[2] For the remainder of this thesis, when referring to a "traumatic event," I am referring specifically to events that satisfy this DSM-5 definition and would therefore qualify someone for a diagnosis of PTSD were they to meet the criteria for resultant symptoms of distress.

Second, Criterion A emphasises the role of the event over the role of a person's underlying dispositions (McNally, 2004). For example, for an event to be psychologically traumatic, people need to interpret the event as traumatic. Some people are more prone to interpreting an event as traumatic than others; these people might, in turn, overestimate the level of threat involved in an event and be traumatised by an event that otherwise would not satisfy Criterion A (McNally, 2003b, 2004). It is sometimes the case, then, that people's underlying dispositions are more predictive of their subsequent distress than the level of threat posed by the event said to trigger the distress.

Third, exposure to traumatic events, as described in the DSM, is not rare. In fact, in a survey of adults from the United Sates, almost 90% of respondents had experienced at least one of the DSM-5's Criterion A events (American Psychiatric Association, 2013; Kilpatrick et al., 2013). And yet, only a small percentage then go on to develop PTSD (Koenen et al., 2017). Taken together, these findings suggest the same traumatic event can have a different effect on different people (Rubin et al., 2011).

How, then, are we to explain the development of PTSD? What separates those who are resilient in the face of trauma from those who go on to suffer extreme psychological distress? At the core of many theories of PTSD is the idea that memory is key to understanding people's symptoms of distress. That is, a person's subsequent psychological stress is thought to be a function of the way in which that person remembers their traumatic event, including how coherent their memory for the event is.

The Coherence of Traumatic Memories

The idea that, among people with PTSD, memories for traumatic events are uniquely incoherent is prevalent in some corners of the clinical literature and founded in clinical observations of people recalling their traumatic memories for the first time (van der Kolk & Fisler, 1995). Those prescribing to this view of traumatic memory claim that

for people to recover from PTSD, among other things this incoherence must be resolved (Brewin, 2014; Brewin et al., 1996; Ehlers et al., 2005). This view has historic roots in the psychodynamic approaches of Freud and Janet, both of whom mentioned observations of incoherence in their clinical notes. For example, Freud wrote:

> the individual hysterical symptoms disappeared immediately and did not recur if we succeeded in wakening the memory of the precipitating event with complete clarity, arousing with it the accompanying affect, and if the patient then depicted the event in the greatest possible detail and put words to the affect. Remembering without affect almost always fails to be effective; the psychical process that had originally taken place has to be repeated in as vivid a way as possible, brought to its status nascendi, and then 'talked through.' (Freud & Breuer, 1895/2004, pp. 84–85)

Likewise, Janet claimed that in the case of traumatic memories, patients were "incapable of making the necessary narrative which we call memory regarding the event" (Janet, 1919/1925, p. 663).

At its extreme, this view is consistent with "repression": the idea that when someone experiences a traumatic event, their memory for that event is pushed into the unconscious where it is inaccessible for recall (Erdelyi, 1985; Erdelyi & Goldberg, 1979). The memory is said to reside in the unconscious for years until it might, eventually, be "recovered" and become accessible for recall (Bower, 1990; Loftus, 1993). This repression is supposedly an automatic coping mechanism by which people are protected from recalling the overwhelming and devastating nature of the event. Nonetheless, the trauma is said to manifest itself in a range of psychological and physical symptoms (Erdelyi, 1985; Erdelyi & Goldberg, 1979). To find relief from these symptoms, then, people must make the unconscious conscious (Freud & Breuer, 1895/2004). But there is

no good scientific evidence to suggest this special repression mechanism exists, and plenty of counter-explanations for apparent cases of repression (for a review, see Otgaar, Howe, & Patihis, 2022).

As thinking about repression evolved in light of research, some scientists reported scepticism about the idea that traumatic memories are incoherent (see, for example, Berntsen et al., 2003; Byrne et al., 2001; Gray & Lombardo, 2001; Rubin, Berntsen, & Bohni, 2008). In light of scientific evidence, some clinical researchers likewise updated their thinking about the relative coherence of traumatic and non-traumatic memories (I return to this scientific evidence shortly; for reviews, see Crespo & Fernández-Lansac, 2016; O'Kearney & Perrott, 2006; Sotgiu & Mormont, 2008). Consider, for example, that prolonged exposure therapy therapist guides from 2019 reduce the focus on resolving incoherence versus the same guides from 2007 (Foa et al., 2007, 2019). But still in some corners of the literature the incoherence view persists and causes controversy (see, for example, McNally et al., 2022).

This "incoherence" view of traumatic memories has also persisted in the DSM-5 criteria for PTSD in Criterion D as an "inability to remember an important aspect of the traumatic event(s) (typically due to dissociative amnesia…)" (American Psychiatric Association, 2013, p. 271). Here, dissociative amnesia refers to "an inability to recall autobiographical information that is inconsistent with normal forgetting" (American Psychiatric Association, 2013, p. 291).

The "Special Mechanisms" View of Traumatic Memories

Several "special mechanisms" accounts of PTSD and traumatic memories suggest incoherent traumatic memories are implicated in the development and maintenance of PTSD (Brewin et al., 1996, 2010; Ehlers & Clark, 2000; Foa & Riggs, 1993; van der Kolk, 1987, 1994, 1997). These accounts propose that a failure to successfully process

and encode a traumatic event plays a role in the development of PTSD. When one fails to adequately process a trauma, the mechanisms set into action supposedly operate differently than they would if the event was of a non-traumatic nature. These mechanisms are said to result in incomplete encoding, and therefore, a memory for the event that is fragmented and incoherent (Brewin et al., 1996, 2010; Ehlers & Clark, 2000; Foa & Riggs, 1993; van der Kolk, 1987, 1994, 1997). The emphasis placed on the role of incoherence in PTSD varies across these theories. Here I focus on the aspects of these theories that are concerned with the coherence of memory. I progress from theories in which incoherence plays a relatively minor role to those in which it plays a more substantial role.

Emotional Processing Theory. The original iteration of Foa's emotional processing theory centred around how people represent the concept of fear in memory (Foa & Kozak, 1985, 1986; Foa et al., 1989). More specifically, the theory proposed fear is stored as a cognitive structure that is activated to escape danger (Foa & Kozak, 1985, 1986). This cognitive structure is said to contain representations of several components of fear: the feared stimuli (for example, a snake), the physiological responses to fear (for example, sweaty palms), and the meaning assigned to both the stimuli (for example, snakes could kill me) and the responses (for example, sweaty palms mean that I am scared).

For the most part, Foa suggests these fear structures are normal and reflect realistic threat. But they are said to become pathological when the structure contains components not representative of realistic threat, leading people to develop anxiety disorders such as PTSD (Foa & Kozak, 1986). In PTSD, specifically, Foa claims people develop fear structures containing many different non-threatening stimuli. Their

representations of fear are thought to manifest as the physiological and behavioural symptoms of PTSD (Foa & Kozak, 1985).

This theory was subsequently revised to account for clinical observations that traumatic memories are often incoherent and disorganised. To explain this observed incoherence, Foa & Riggs (1993) proposed that incoherent traumatic memories are the result of the way information is processed while someone is distressed. This theory highlights five mechanisms by which distress could operate to affect information processing and, in turn, produce incoherent memories:

1. Anxiety affects the direction of attention during an event—people are more likely to attend to threatening stimuli and less likely to attend to neutral stimuli, resulting in gaps in the subsequent memories.
2. Anxiety leads people to be more distractible and less able to concentrate, resulting in disorganised memories.
3. At extreme levels, anxiety might narrow the focussing of attention so much that pieces of both relevant and irrelevant information are neglected and therefore cannot be encoded into memory.
4. Anxiety requires so much of working memory's attentional capacity that there is not enough left to process all the details of a traumatic event.
5. People might dissociate during the traumatic experience, leading to a discrepancy in memory representations encoded at the time of the trauma.

Because the resulting apparently incoherent memories are difficult to reconcile with one's schemas about the world, Foa claims fully engaging in emotional processing of the event is necessary to resolve this incoherence and recover (Foa et al., 2005). In the 2006 update to the theory, Foa and colleagues outlined three factors associated with spontaneous recovery from PTSD over time and recovery from PTSD over therapy (Foa et al., 2006).

One of these three factors was the development of a coherent memory for the trauma (Foa et al., 2006).

The Psychobiology of Traumatic Memory. In van der Kolk's account of traumatic memories and PTSD, traumatic events leave persistent records on both our biological and psychological systems (van der Kolk, 1987, 1994, 1997). Among them are several factors that affect the processes by which traumatic memories are encoded (van der Kolk, 1987, 1994, 1997). For example, van der Kolk claims the threat posed by a traumatic event leads people to narrow their attention to focus on certain details to the detriment of others (van der Kolk & Ducey, 1989). Furthermore, in this account, traumatic events are highly emotional and these emotions result in higher levels of activation in the amygdala (a part of the brain associated with emotion). This activation supposedly interferes with the function of the hippocampus, which plays a crucial role in encoding (van der Kolk, 1994, 1997). These trauma-specific processes are said to result in traumatic memories that are poorly elaborated, lacking temporal organisation and meaning. van der Kolk suggests the memories are unable to be consciously recalled in narrative form; this lack of narrative leads aspects of the memory to be relived involuntarily, intruding into people's consciousness as a series of fragmented images, sensations, and feelings (van der Kolk, 1987, 1994, 1997).

A Cognitive Model of PTSD. Ehlers and Clark's (2000) model of PTSD proposes that PTSD arises when people process a traumatic event in a way that leads them to feel a continuing threat. One of the reasons for this persistent feeling of threat is a reliance on "data-driven processing" at the time of a traumatic experience (Ehlers & Clark, 2000, p. 331; Halligan et al., 2002). A focus on processing sensory details to the detriment of conceptual details is said to create a deficit in autobiographical memory such that memories of the traumatic event are poorly elaborated and incompletely incorporated

into the autobiographical knowledge base. This lack of integration means the memory for the event will be difficult to retrieve intentionally, and the memory will feel incoherent.

Dual Representation Theory. Brewin's dual representation theory makes a distinction similar to van der Kolk's theory, in that it too proposes traumatic events are recalled as involuntary sensory memories, unable to be recalled in narrative form. This theory suggests two representations of traumatic events exist in memory—contextual memories and sensation-based memories (previously called "verbally accessible memories", and "situationally accessible memories"; Brewin et al., 1996, 2010). Contextual memories are said to contain information about the event's context that a person can deliberately retrieve from their autobiographical knowledge base, including information about sensory aspects of the event, emotional and physical reactions experienced during the event, and the meaning of the event (Brewin et al., 2010). These memories are thought to be integrated with the remainder of the autobiographical knowledge base and can be consciously edited. By contrast, the theory states sensation-based memories cannot be deliberately retrieved and are instead likely to be retrieved in response to internal situational cues (for example, thinking about the traumatic event) or external situational cues (for example, seeing a similar event on the television; Brewin et al., 2010). These memories are thought to contain information about sensory, physiological, and motor aspects of the traumatic event, but are difficult to verbalise and poorly integrated with the autobiographical knowledge base.

According to Brewin's theory, it is typical for people to undergo a period of emotional processing after experiencing a traumatic event (Brewin et al., 1996, 2010). This processing is said to fulfil two aims: 1) prevent the recall of sensation-based memories in the form of intrusive flashbacks, and 2) allow for a conscious attempt to search for meaning and resolve the conflict between the traumatic experience and one's

expectations and beliefs about the world. But Brewin suggests this emotional processing does not always lead to a "normal" or healthy trauma response (Brewin et al., 2010). While healthy processing would hypothetically result in contextual and sensation-based memories that are connected, pathological processing is said to be characterised by an impaired connection between the two memory representations. This impaired connection is thought to result in flashbacks as visual imagery is activated without an associated contextual memory (Brewin et al., 2010). Furthermore, because the contextual memories are apparently weak and incomplete, these memories are said to remain incoherent with certain pieces of the event missing.

A Note About the Effects of Stress on Memory. It is important to note that, at first glance, some of these theories seem to be founded in the scientific knowledge of how stress, more broadly, affects memory. Consider, for example, van der Kolk's suggestion that when people experience a trauma their attention narrows to focus on certain details to the detriment of others, or Foa's suggestion that during a trauma people are more likely to attend to threatening stimuli and less likely to attend to neutral stimuli, resulting in memory gaps (Foa & Riggs, 1993; van der Kolk & Ducey, 1989, van der Kolk, 1994, 1997). Both suggestions are consistent with the well-established finding that suggests arousal narrows attention; evidence fits with the idea that when exposed to stress, people tend to narrow their attention to focus on central or emotional information and fail to attend to neutral or peripheral information (Christianson, 1992; Easterbrook, 1959; Herten et al., 2017). Of course, what is not attended to during the event will not be encoded, so findings suggest this narrowing of attention has downstream effects on memory; in stressful situations, people tend to remember central or emotional information better than they do neutral or peripheral information (Christianson & Loftus, 1987, 1991; Christianson & Nilsson, 1984; Loftus & Burns, 1982; Shields et al., 2021; but see also,

Wessel & Merckelbach, 1994; Wessel et al., 2000). In this way, stress might indeed result in memory incoherence.

Where these accounts diverge from the established literature on stress and memory is in proposing that these mechanisms would result in memory incoherence for traumatic memories alone, and that this memory incoherence underlies the distress people might experience after the trauma. Of course, stress is not unique to traumatic events; people might experience stress during events we could otherwise consider positive, such as a wedding or childbirth. Furthermore, the special mechanisms accounts rest on the presupposition that by processing the event in therapy, people can fill in any gaps in their memories. Some specific therapeutic procedures encourage this processing, and list among their aims a resolution of incoherence to aid recovery from PTSD. I focus here on the aspects of these therapies that are said to resolve incoherence.

Therapies for PTSD that Target Coherence

Eye-Movement Desensitisation and Reprocessing Therapy (EMDR). EMDR was designed to resolve the symptoms of stress associated with people's memories for traumatic events (F. Shapiro, 1989, 1995, 2001). In EMDR, clients identify the most distressing moment of their traumatic event and bring to mind the image of that moment while attending to external stimuli; most commonly, therapists direct the client to move their eyes laterally while recalling aspects of the event, but hand-tapping and audio material might also be used. Treatment is said to help clients engage in "appropriate information processing," with therapists directed to identify targets to "gain access to the dysfunctional memory networks" (F. Shapiro, 2018, p. 47). "Memory lapses, dissociations, and the fact that access is restricted to negative material" are all said to be "indications of obstructed memory networks" (F. Shapiro, 2018, p. 47). Therapists are instructed to interpret "any inappropriate dissociative response, either an over- or

underreaction to a traumatic event" as indicative of an obstructed memory, and therefore to target this reaction in therapy to help resolve the obstruction and facilitate access to the memory (F. Shapiro, 2018, p. 49). Perhaps unsurprisingly, then, one study found a majority of EMDR therapists reported belief in the existence of repressed memories; a non-trivial minority endorsing the idea that when traumatic memories are not fully processed, the memories can manifest as psychological or psychosomatic symptoms (Houben et al., 2021).

Furthermore, specific protocols for early EMDR intervention have been developed for use with people who experienced trauma within the last two to three months, because the supposed incoherence of traumatic memories means the typical protocols might not be effective (E. Shapiro, 2009; F. Shapiro, 1995). More specifically, recent traumas are said to be stored "as a fragmented experience that has not yet consolidated" (E. Shapiro, 2009, p. 142)[3]; as a result, the image of the most distressing moment of the event will not necessarily be associated with other aspects of the event. All aspects, then, must be targeted separately in therapy. Processing begins with the most distressing moment of the event, and clients are then asked to recall the remaining aspects in chronological order. Therapists work to process each aspect to "facilitate integration and consolidation" (E. Shapiro, 2009, p. 142). Clients repeat this process in therapy until they can recall the entire event from beginning to end without upset, suggesting incoherence has resolved (E. Shapiro, 2009).

Brainspotting. An offshoot of EMDR, Brainspotting is a relatively new treatment for PTSD (Grand, 2013). While treating clients with EMDR, Grand observed his clients'

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[3]It is important to note the protocols do not seem to be using the word "consolidated" to refer to memory consolidation in the neuroscientific manner, but instead to refer to the linking of aspects into "an integrated whole"; a process F. Shapiro suggests, in the case of trauma, happens over two to three months (F. Shapiro, 2018, p. 222).

eye movements tended to wobble and freeze momentarily, before they "processed traumatic material which had not been accessible previously" (Corrigan & Grand, 2013, p. 759). Aspects of traumatic events are said to have a location in the visual field—a "brainspot"—that is associated with a specific neurophysiological response (Grand, 2013). Over the course of brainspotting, clients' eye movements follow the therapist's hand as it waves back and forth horizontally across the client's visual field. When the therapist observes an eye reflex or body movement, a brainspot is apparently identified. Clients are then instructed to focus on what they are feeling as they attend to, and process, the traumatic memory. According to Brainspotting's creator, clients may hold their "emotional trauma in [their] nervous system," with the traumatic memories manifesting as physical pain (Grand, 2013, p. 112). Brainspotting, then, is said to help with the "opening up" of painful memories (Grand, 2013, p. 113). Other times, Grand claims, clients have believed gaps in their memories were due to loss of consciousness, but over the course of brainspotting "the veil of dissociation lifted in front of their eyes" as they began to remember aspects of the trauma they previously could not (Grand, 2013, p. 117). In these instances, brainspotting therapy begins from the assumption that incoherent traumatic memories underlie clients' distress and seeks to resolve that incoherence in treatment.

Prolonged Exposure Therapy. Over the last 40 years, prolonged exposure therapy has emerged as one of the most effective treatments for PTSD (McLean & Foa, 2022; Powers et al., 2010). The treatment program was developed using principles from exposure therapy for anxiety disorders more generally and adapted to account for the nature of PTSD and maladaptive cognitions thought to underlie the disorder (Foa, 2011; Foa et al., 2007, 2019; Foa & Kozak, 1985). Prolonged exposure therapy consists of four separate components: 1) educating the client about typical reactions to traumatic events,

2) training the client on relaxation breathing techniques, 3) encouraging the client to expose themselves to situations or objects that are related to their traumatic event ("in vivo exposure"), and 4) asking the client to repeatedly recall the memory of their traumatic event ("imaginal exposure"). Here, I focus on the imaginal exposure component because of its implications for the coherence of memory.

Imaginal exposure is introduced to clients in their third session of therapy and occurs during every remaining session (Foa et al., 2019). Clients are told that "imaginal exposure aims to enhance your ability to digest and process the traumatic memory" (Foa et al., 2019, p. 45). One of the three goals of imaginal exposure is "enhanc[ing] the patient's ability to access all of the salient aspects of the trauma memory" (Foa et al., 2019, p. 93). Therapy guides provide therapists with examples of metaphors they might use to explain the value of imaginal exposure, including the metaphor below which emphasises the supposed incoherence of traumatic memories:

> Imagine that your traumatic event is recorded in your brain like a book, with paragraphs, pages, and sections. Like all books, your traumatic event book is a story with a beginning, middle, and end. Because you try to avoid thinking about your trauma, you have not read the book from the beginning to the end since the trauma occurred. Whenever you have a flashback, the book is opened automatically at the paragraph where the flashback is written, and you read this paragraph. Reading this part of the book is painful and distressing. So you say to yourself, "I don't like this book," and with some effort you manage to close the book. The same thing happens when you have the next flashback or thought about your trauma. And this is why you have not read what is written in the book. (Foa et al., 2019, p. 94)

When recalling their memories, clients are asked to begin their recall slightly before the beginning of the event, and to continue through to the point where the event ended, or the client was no longer in danger. Clients are encouraged to close their eyes and recall the memories "as vividly as possible and to picture them in your mind's eye" (Foa et al., 2019, p. 99). They are told to recall their memories in the present tense, as if the event was happening as it was being recalled, and in as much detail as possible. Imaginal exposure sessions go on for approximately 40 minutes, and once clients have finished their first recounting of the event they are asked to go back to the beginning and start recalling the memory again (Foa et al., 2019). As treatment progresses, if clients do not spontaneously recall certain details, therapists will begin to interrupt their recall of the memories to ask them specific questions about the event. Sessions are recorded via audiotape, and clients are given the audiotape to take home with them and listen to once a day. Clients are instructed to listen to the complete tape from beginning to end, without interruption and with their eyes closed. They are encouraged to visualise what they are hearing and engage with any feelings they have (Foa et al., 2019).

Cognitive Therapy for PTSD. A variation on prolonged exposure therapy and cognitive processing therapy (Resick & Schnicke, 1992), this iteration of cognitive therapy draws on Ehlers & Clark's cognitive model of PTSD (Ehlers & Clark, 2000). One of the primary goals for the therapy is to correct the patient's autobiographical memory deficit by helping them "develop a coherent narrative account which starts before the trauma begins, ends after the patient is safe again and places the series of events during the trauma in context, in sequence, and in the past" (Ehlers et al., 2005, p. 416). To achieve this goal, the therapy can use a combination of a) writing a detailed description of the event to help the client recall details of the event, b) imaginal exposure to help with "eliciting all aspects of the memory" and combining them into a complete description,

and c) revisiting the site of the trauma to place the event in the past. Procedures are adapted to the needs of each client (Ehlers et al., 2005).

Taken together, these various therapies all assume incoherent traumatic memories play a role in the symptoms of posttraumatic stress their clients report. As such, the therapies use techniques to resolve this apparent incoherence and, in turn, promote recovery. In all of these therapies, clients repeatedly bring to mind their memory of the traumatic event with a view to process the memory and build coherence. So how effective is treatment at building coherence, and is increased coherence associated with recovery?

The Effect of Therapy on Coherence. Despite the focus on resolving supposed incoherence over the course of treatment, few studies have tested the efficacy of treatments using procedures hypothesised to target incoherence, and the subsequent effect on symptoms of distress. The handful of peer-reviewed studies that have addressed this issue using prolonged exposure therapy protocols were small (client *N*s between 14 and 77) and produced findings that differed depending on the study and the measure of incoherence (Bedard-Gilligan et al., 2017; Foa et al., 1995; Kindt et al., 2007; Mundorf & Paivio, 2011; van Minnen et al., 2002). More specifically, across all five of studies there was no consistent reduction in incoherence from pre- to post-treatment, and findings regarding the relation between incoherence and distress were inconsistent—sometimes reductions in incoherence were associated with reductions in distress, and other times not. These findings raise questions about the extent to which a reduction in incoherence occurs over the course of treatment and if such a reduction is, in fact, necessary for recovery.

The Scientific Evidence About the Coherence of Traumatic Memories

One crucial issue for those claiming incoherent traumatic memories underlie posttraumatic stress is that the scientific evidence about coherence is mixed: traumatic

memories are sometimes less coherent than their non-traumatic counterparts, sometimes similarly coherent, and other times more coherent (for reviews, see Crespo & Fernández-Lansac, 2016; O'Kearney & Perrott, 2006; Sotgiu & Mormont, 2008).

At least two possible explanations for these heterogenous findings have been proposed. First, there is little consistency in the literature in the comparison memories selected, resulting in conclusions about the coherence of traumatic memories that are based on different comparisons (Crespo & Fernández-Lansac, 2016). To address the issue of the coherence of traumatic memories, then, we must compare traumatic memories to a range of other memories, controlling for factors that might plausibly affect coherence—such as emotional intensity, valence, and importance (Read & Lindsay, 2000; Rubin, Boals, & Berntsen, 2008). Second, there is also little consistency in the literature about how to measure coherence. The scientific literature characterises coherence in a variety of different ways, leading to a variety of operational definitions of coherence (O'Kearney & Perrott, 2006). A solution to this issue would be to use a range of measures that capture a wide variety of aspects of coherence, and to determine on which measures, if any, traumatic memories differ.

Rubin and colleagues (2016) addressed both possibilities by examining 28 different measures of coherence in people's traumatic, positive, and important memories. These measures included subjective ratings made by the subjects after reporting their memories, objective ratings made by observers after reading the memory reports, and computer-scored measures taken from linguistic analyses performed on the memory reports. Across these various measures there was no evidence of consistent differences in coherence between traumatic and non-traumatic memories (Rubin et al., 2016). In fact, where differences emerged, the traumatic memories were more coherent than the comparison memories just as often as they were less coherent. Even in the subset of the

sample who met the diagnostic criteria for PTSD, traumatic memories were both more and less coherent than the positive and important memories depending on the specific measure of coherence (Rubin et al., 2016). These findings support the idea that the incoherence of traumatic memories is unlikely to be central to the emergence and maintenance of PTSD and provide evidence inconsistent with the special mechanisms accounts of PTSD. Instead, these findings provide support for a "basic mechanisms" theory of traumatic memories in which symptoms of distress are explained in terms of basic mechanisms that operate on all autobiographical memories, rather than on only memories for trauma.

The Autobiographical Memory Theory of PTSD

In contrast to "special mechanisms" accounts of PTSD, Rubin and colleagues proposed a theory of PTSD that drew on more recent developments in research on emotion, cognition, and autobiographical memory more generally (Rubin, Berntsen, & Bohni, 2008). According to the autobiographical memory theory, three interrelated factors predict people's responses to traumatic events. First, people are more likely to develop PTSD if their memory for the event is a) frequently recalled, b) infused with emotions, and c) tied closely to their identity and life story. These three properties increase the ease with which the memory comes to mind—a key feature of PTSD. Second, people are more likely to remember a traumatic event in these ways if they have a general tendency towards a) frequently bringing memories to mind, b) experiencing events as emotionally intense (regardless of their valence), and c) tying negative events closely to their identities and life stories. Third, people are more likely to develop PTSD if they struggle to regulate their emotions when the memory of their traumatic event comes to mind (Rubin, Berntsen, & Bohni, 2008).

On their own, each of these factors is not adequate to predispose someone to PTSD. In fact, frequently recalling an emotional and central memory for a traumatic experience is a normal response to trauma. Instead, the theory emphasises the importance of both the memory for the event and the person experiencing the event when accounting for the development of PTSD (Rubin, Berntsen, & Bohni, 2008). Several studies have tested and provided support for this theory, suggesting special mechanisms accounts of traumatic memories are not necessary (and are in fact insufficient) for explaining the development of PTSD (Robinaugh & McNally, 2011; Rubin, Boals, & Berntsen, 2008; Rubin et al., 2011). Instead, PTSD can be explained by the combination of these basic psychological mechanisms.

Despite these developments in our understanding of PTSD and traumatic memories, disagreement regarding the coherence of traumatic memories persists in the scientific literature (see, for example, McNally et al., 2022). In this thesis, I attempt to reconcile these discrepant views by drawing on principles from basic cognitive psychology.

Overview of the Thesis

Paper One

It is possible that coherence is not a stable property of memory but is instead, to some extent, an attribution made in the moment depending on the way in which a memory was constructed at retrieval. For example, recall that under the self-memory system model of autobiographical memory, the current goals of the working self determine which pieces of autobiographical knowledge can be retrieved and how those pieces of knowledge are constructed to form an autobiographical memory (Conway, 2005; Conway & Pleydell-Pearce, 2000). These memories, then, will be constructed in different ways at different instances of retrieval depending on the working self's goals at

the time. These differences in construction might have flow on effects for the coherence of that memory at that instance of retrieval.

This idea is also consistent with Rubin's dimensional model of autobiographical memory in which narrative might be an added component during construction of an autobiographical memory but is not necessarily an inherent property of a memory (Rubin, 2005, 2006, 2019; Rubin & Umanath, 2015). The type of narrative structure used to construct an autobiographical memory might differ, then, depending on the purpose for which that memory is being recalled. Again, these differences in narrative structure could plausibly affect the coherence of that memory in the moment.

To the extent that autobiographical memories are constructed in the moment based on schemas and situational demands, could judgements of coherence be pushed around by simple variations in these demands? And would these manipulations affect judgements about the coherence of both traumatic and non-traumatic memories? These are the questions we address in our first paper. Before conducting the experiments proper, we first conducted a pilot experiment to a) test our manipulation of retrieval task, and b) determine how many subjects we needed to include to have a sufficiently large sample of people rating traumatic memories. The details of this pilot experiment can be found in Appendix A.

In this paper, published in the *Journal of Applied Research in Memory and Cognition,* we reported two experiments in which we asked people to recall memories in varying ways. In the first experiment, we asked people to recall a memory that was either positive, negative, important, or traumatic. We then manipulated the conditions under which people retrieved their memory, before asking them to rate its coherence. We found that when people were asked lots of questions about their memory, they then rated that memory as less coherent. In contrast, when people recalled their memory from beginning

to end, as if telling a story, they then rated that memory as more coherent. These findings persisted regardless of whether the memories were traumatic or non-traumatic and suggest the ways in which people recall memories might produce changes in how coherent those memories feel, in turn contributing to mixed evidence about coherence.

In the second experiment, we replicated these findings in a group of people experiencing distress about a traumatic event, and found these findings persisted regardless of the severity of people's posttraumatic stress. Taken together, these findings suggest the way we retrieve a memory has downstream consequences for judgements we make about that memory. Furthermore, these findings are at odds with the idea that coherence is a stable property of a specific memory, and instead suggest coherence can be the product of attributions, manipulated in the moment.

Paper Two

To date, most of the research on the coherence of traumatic and non-traumatic memories has been done by asking people to recall memories for several different types of experiences from their lives—for example, the memories of their most positive and most negative life events (Porter & Birt, 2001; Porter & Peace, 2007; Rubin, 2011; Rubin et al., 2011, 2016). This reliance on evidence from people's autobiographical memories presents at least three issues that hinder our interpretation of the existing findings. First, we have no verifiable account of what happened during the events, so we cannot determine loss of information, accuracy, or distortion. Second, these autobiographical memories might be for events that happened at varying points in people's lives. We are then left with the challenge of controlling for possible effects of the passage on time of memory coherence, such as added opportunity for memory rehearsal and differences in degree of memory decay depending on how old the memory is (Ebbinghaus, 1913; Meeter et al., 2005; Skowronski & Walker, 2004). Third, we are unable to account for

aspects of the events themselves that could plausibly affect memory coherence. For example, are people recalling memories of events that unfolded in a similarly structured and organised manner? Tackling these issues is crucial to establish the existence of causal relationships between different types of events and the coherence of people's subsequent memories for those events; information that would advance our understanding of the relative coherence of traumatic and non-traumatic memories.

Therefore, in our second paper, published in *Clinical Psychological Science,* we reported the creation of a new set of materials for use in the "trauma film paradigm"—a well-established paradigm in which subjects watch a film depicting a traumatic event or compilation of events, and later answer various questions about the event and how it has affected them (Holmes & Bourne, 2008; James et al., 2016). After establishing that our films a) induced positive or negative affect as expected, b) depicted events of different valence and emotional intensity that were equivalently easy to follow, and c) were not familiar to most people, we then used these materials to examine the coherence of people's memories for the various traumatic and non-traumatic films. We found that regardless of the type of film people watched, their memories were not only coherent, but equivalently coherent—a finding that provides some evidence against the idea that people's memories for trauma are processed via a special mechanism, and buttresses findings from the autobiographical memory literature.

Paper Three

The psychological literature is yet to provide consensus on the extent to which people's traumatic memories cohere over the course of treatment for PTSD. In the most thorough peer-reviewed investigation of the issue to date, Bedard-Gilligan and colleagues (2017) examined the coherence of traumatic and non-traumatic memories before and after a 10-week course of prolonged exposure therapy. There was no evidence to suggest

people's memories were more coherent after treatment than they were before, even among those who showed the greatest reduction in symptoms of PTSD (Bedard-Gilligan et al., 2017). It would appear, then, that prolonged exposure therapy does not operate to increase the coherence of people's traumatic memories, and that recovery from PTSD does not require the resolution of a feeling of incoherence (c.f. Brewin et al., 1996, 2010; Foa et al., 1995; Foa & Riggs, 1993; Foa & Rothbaum, 1998).

But this conclusion neglects to consider the effect of the passage of time on memory coherence. That is, as time passes, memories decay: we forget details of events over time (Ebbinghaus, 1913; Meeter et al., 2005). Perhaps as these details are lost, a memory feels less coherent. Any study that seeks to quantify the effect of repeated recall on memory coherence, then, needs to account for any decay that might happen over time if a memory were not repeatedly recalled. One way to account for this decay would be to consider the coherence of both a memory that is repeatedly recalled over the course of an experiment, and a memory that is not. Therefore, our third paper, a manuscript in preparation for submission, seeks to examine the pattern of memory coherence over repeated recalls, compared to memories that are not repeatedly recalled.

Furthermore, it is possible that simple cognitive mechanisms might be responsible for any change in coherence that occurs over the course of repeated recall. More specifically, the coherence of a memory might change over repeated reproductions of that memory as the memory undergoes both reconsolidation, and levelling and sharpening (Allport & Postman, 1945; Bartlett, 1932; Nader et al., 2000). For example, although the aims of both levelling and sharpening are to create a narrative that is easier to understand, as Bartlett observed, a side effect is that the narrative becomes more coherent (Bartlett, 1932). As people repeatedly retrieve a memory, then, and engage in levelling and sharpening, the coherence of that memory might plausibly change. Additionally, as

people repeatedly retrieve a memory, it is possible that any extra details recalled while the memory is in a labile state will be incorporated into the memory as that memory undergoes reconsolidation (Lee et al., 2017). These added details, in turn, might alter the coherence of that memory. Crucially, these cognitive mechanisms would plausibly operate to affect the coherence of traumatic and non-traumatic memories alike (Alberini & LeDoux, 2013).

Therefore, in the experiment reported in this manuscript we asked people to watch two films portraying traumatic or various non-traumatic events, and then to repeatedly describe the memory of one of those films over the course of five days, reporting the coherence of their memories for both films at the beginning and end of the experiment. We then completed linguistic analyses on each of these descriptions to attain computer-scored measures of memory coherence. We also asked people to report the symptoms of distress they experienced about each film over the final two days of the experiment, which allowed us to examine the extent to which memory coherence was associated with distress.

Before conducting the experiment proper we ran two pilot studies to a) examine the coherence of people's autobiographical memories over repeated recall, and b) determine how best to order people's ratings of their symptomatology of PTSD. The details of these pilot experiments can be found in Appendix D.

As detailed in the manuscript, our experiment proper produced a pattern of data consistent with the idea that repeatedly recalling a memory prevented a subjective loss of coherence that occurred for control memories. Furthermore, a similar pattern emerged regardless of whether that repeatedly-recalled memory was traumatic or non-traumatic. There was no evidence that the traumatic memories were subjectively incoherent. When we examined computer-scored measures of coherence, we found similar a pattern. Across

18 measures, traumatic memories were a mix: sometimes more, sometimes less, and sometimes equivalently coherent to their non-traumatic counterparts. We found little evidence of traumatic memories operating differently over repeated recall compared to non-traumatic memories. Finally, there was no evidence of a relation between the coherence of people's traumatic memories and their symptoms of posttraumatic stress. These findings have implications for models of PTSD, and the development and delivery of cognitive therapies for PTSD.

Meta-Analyses

Finally, we followed recommendations from Cumming (2014) and conducted two mini meta-analyses to obtain more precise estimates of our findings about a) the relative coherence of traumatic and non-traumatic memories, and b) the relation between the coherence of a traumatic memory and the symptoms of posttraumatic stress associated with that memory. The first meta-analysis supported the conclusion that across our experiments there was no meaningful difference in the coherence of traumatic memories and their various non-traumatic counterparts. The second meta-analysis supported the conclusion that across our experiments there was no evidence of a relation between coherence and symptomatology.

Chapter 2: Judgments of Memory Coherence Depend on the Conditions Under Which a Memory is Retrieved, Regardless of Reported PTSD Symptoms

Paper published in the *Journal of Applied Research in Memory and Cognition*:

Taylor, A., Jordan, K., Zajac, R., Takarangi, M. K. T., & Garry, M. (2020). Judgments of memory coherence depend on the conditions under which a memory is retrieved, regardless of reported PTSD symptoms. *Journal of Applied Research in Memory and Cognition, 9*(3), 396-409. https://doi.org/10.1016/j.jarmac.2020.07.003

Abstract

People's memories for traumatic experiences supposedly lack "coherence." Yet scientific evidence does not conclusively support these claims—instead, the evidence is mixed. We hypothesized this mixed evidence occurs because coherence is not a stable property of an individual memory, and is sensitive to momentary attributions depending on how that memory is retrieved. In Experiment 1 we asked 1640 people to bring to mind a memory that was either positive, negative, important, or traumatic, and then to either simply describe that event, or answer many specific questions about it. Those who answered specific questions rated their memories as less coherent than those who provided descriptions. In Experiment 2 with 886 people, these effects persisted regardless of people's reported PTSD symptoms. Across both experiments, we found no evidence to suggest traumatic memories feel incoherent. Our results suggest coherence is sensitive to momentary attributions, depending on how a memory is retrieved.

Keywords: Attributions, Autobiographical memory, Memory coherence, PTSD, Traumatic memories

General Audience Summary

In popular culture, traumatic memories are often portrayed as remembered in pieces, fragmented and jumbled—or in other words, as "incoherent." One idea, based on the way clients sometimes discuss their memories in therapy, is that our brains have a special process for encoding traumatic memories. Incoherent memories are a by-product of this suggested process, and are said to be prominent in people with posttraumatic stress disorder (PTSD). But evidence from scientific studies does not conclusively support the existence of this special process, and instead is mixed—sometimes people's traumatic memories are more coherent than their non-traumatic memories, and other times less. We wondered whether one reason this mixed evidence exists is because coherence is not a property of an individual memory, but is instead a judgment people make in the moment depending on their experience recalling that memory—for example, how easy it felt to recall. We addressed this possibility in two experiments by varying the way in which people recalled a traumatic or non-traumatic memory. We found that when people were asked lots of questions about their memory, they then rated that memory as less coherent. In contrast, when people recalled their memory from beginning to end, as if telling a story, they then rated that memory as more coherent. These findings persisted regardless of whether the memories were traumatic or non-traumatic, and regardless of people's post-traumatic distress. Our findings suggest the ways people recall memories might produce changes in how coherent those memories feel, in turn contributing to the mixed evidence about coherence. Our findings also provide some reason to question whether incoherence underlies distress—and therefore should be targeted in therapy to reduce distress—or is instead a by-product of situations in which people recall a memory in a way that produces feelings of incoherence.

Judgments of Memory Coherence Depend on the Conditions Under Which a Memory is Retrieved, Regardless of Reported PTSD Symptoms

Pervasive throughout popular culture is the idea that memories for traumatic events are special. In TV dramas like *The Sinner* and *Criminal Minds*, acclaimed movies such as *Waltz with Bashir*, and even in *The Simpsons*, traumatic memories are remembered in pieces—fragmented and jumbled. Traumatic memories are therefore portrayed, in a word, as "incoherent"—lacking the completeness and storyline we associate with other memories (Rubin et al., 2016).

This portrayal in popular culture likely reflects the general public's belief that traumatic memories differ from memories for other events (Ost, Easton, Hope, French, & Wright, 2017; Wake, Green, & Zajac, 2019). But it is not just the general public who hold this belief: a non-trivial percentage of mental health professionals do, too (Ost et al., 2017). This subset of professionals claim traumatic memories are incoherent, and therefore promote distress, due to impoverished encoding (e.g. Brewin, Dalgleish, & Joseph, 1996; Foa & Riggs, 1993; van der Kolk & Fisler, 1995). This putative encoding mechanism accounts for the "amnesia" part of Criterion D for posttraumatic stress disorder (PTSD): an "inability to recall key features" of the traumatic event (American Psychiatric Association, 2013).

But the scientific literature does not conclusively support this incoherence view of traumatic memories. Instead, the evidence is mixed. Traumatic memories are sometimes less coherent than their non-traumatic counterparts, but other times more coherent (for reviews, see Crespo & Fernández-Lansac, 2016; O'Kearney & Perrott, 2006). Even among people who meet the diagnostic criteria for PTSD, traumatic memories can be, and feel, more—or less—coherent than other emotional memories, depending on how coherence is measured (Rubin et al., 2016).

This mixed evidence about the incoherence of traumatic memories is not a new issue and has prompted various explanations, including two influential accounts suggesting this mixed evidence is driven by methodology. Consider first the "compared to what?" explanation (Read & Lindsay, 2000; Rubin, Boals, & Berntsen, 2008). According to this account, it is only when we compare traumatic memories to a battery of other memories—controlling for emotional intensity, valence, and importance—that we can begin to understand the extent to which traumatic memories are truly incoherent. But across various studies, there is little consistency in the comparison memories selected, meaning conclusions about the incoherence of traumatic memories are made on the basis of varying comparisons (Crespo & Fernández-Lansac, 2016). It is possible, then, that the variation in addressing the "compared to what?" issue contributes to the mixed findings in the scientific literature.

Consider second the "operational definition" explanation. According to this account, the scientific literature is characterized by varying conceptualizations of incoherence, which in turn lead to varying operational definitions (O'Kearney & Perrott, 2006). A promising solution to this issue is to avoid picking and choosing which aspects of incoherence one is interested in, and instead use measures sensitive to multiple aspects. Rubin et al. (2016) took exactly this approach, concluding that across these multiple measures there is no evidence of consistent differences between traumatic and non-traumatic memories. It is likely, then, these differing conceptualizations and operational definitions of incoherence also contribute to the mixed evidence in the literature.

There is a third explanation, one that has not yet featured in the literature. According to this third account, coherence is not a property of a specific memory, but instead is sensitive to attributions people make in the moment, depending on the conditions under which they retrieve that memory. After all, the conditions under which

people are asked to retrieve a memory can produce systematic changes in the way that memory is recalled (for a review, see Marsh, 2007). For example, in one study, people asked to recall an event for entertainment purposes provided more exaggerated retellings than people asked to provide an accurate retelling of the same event (Dudukovic, Marsh, & Tversky, 2004). Furthermore, a large body of work fits with the idea that people come to see feelings as information, informing their momentary attributions (Schwarz, 2012). In one study, when people were asked to list birds from memory, those who were in a positive mood were satisfied with their lists, and therefore stopped making the list earlier, than those who were in a negative mood (Martin, Ward, Achee, & Wyer, 1993). In another study, people rated food additives as more dangerous when the additive felt more difficult to pronounce than they did when the additive felt easier to pronounce (Song & Schwarz, 2009). It is possible, then, that people's judgments of memory coherence are, too, the product of momentary attributions.

There are several ways in which such a process could unfold. Suppose, for example, you are asked to recall an experience from your past, and to provide a description of what happened during that experience. Being asked to freely describe a memory affords people the opportunity to draw on schemas in order to temporarily structure that memory into a coherent story (Campoy & Baddeley, 2008; Tversky & Marsh, 2000). In this case, retrieving a memory for the purpose of providing a description might produce a feeling of coherence, inflating subsequent judgments of coherence, relative to people who do not retrieve the memory in that way. In short, the very act of retrieving a memory for a particular purpose could lead people to recall the memory in a way that produces a feeling of more or less coherence.

Take another example, in which you are asked to answer many questions about one experience from your past, and few questions about another experience. There is

evidence to suggest that, in this situation, people might make attributions about coherence based on the subjective feeling of ease—or fluency—that accompanies the retrieval of each of those memories (for reviews, see Alter & Oppenheimer, 2009; Weingarten & Hutchinson, 2018). Consistent across the psychological literature is the finding that a feeling of fluent retrieval is associated with positive judgments, whereas a feeling of disfluent retrieval is associated with negative judgments. These findings extend to the attributions people make about the quality of their memories. For example, in one study, people asked to recall many aspects of what happened on a recent New Year's Eve rated their memory as poorer than did people asked to recall few aspects (Echterhoff & Hirst, 2006). Clearly, fluency affects people's momentary evaluative judgments, and could shape people's judgments of coherence.[4]

Now imagine you are unable to answer some of the specific questions you're asked about a past experience. It is plausible that this experience of failure would lead you to make attributions about your memory for that experience (Belli, Winkielman, Read, Schwarz, & Lynn, 1998). This approach is similar to the way in which victims of trauma are often asked to retrieve their memories of their traumatic experiences. People might be asked myriad specific questions about their traumatic experiences—perhaps as part of a forensic or therapeutic interview—and encouraged to remember particular aspects in as much detail as possible (Fisher & Geiselman, 1992; Foa & Rothbaum, 1998; Westera, Gentle, Powell, & Zajac, in press). It is likely that over the course of this questioning, people's attention is drawn to parts of the experience they cannot recall. In

[4] In an earlier version of this paper, our theoretical framework was solely about the effects of retrieval fluency on judgments of coherence. But two anonymous reviewers raised alternative accounts of our findings. This feedback led us to broaden the scope of our theoretical framework, and to consider other mechanisms by which retrieval conditions might affect judgments of coherence. We thank the reviewers for their comments.

turn, then, people might make attributions about why they could not recall that information—that their memories, for example, are incoherent.

There is a rationale, then, for hypothesizing a role for coherence as a momentary attribution in explaining the mixed findings about traumatic memory incoherence—an explanation not yet featured in the literature. To what extent do people make momentary attributions about the coherence of their memories, depending on the conditions under which they retrieve those memories? To answer this question, in Experiment 1, we asked people to bring to mind a memory that was either positive, negative, important, or traumatic. We manipulated the conditions under which people retrieved their memory, before asking them to rate its coherence. In Experiment 2, we replicated our findings in a group of people experiencing distress about a traumatic experience.

If people's ratings of coherence depend on the conditions under which a memory is retrieved, it would suggest coherence is not a property of a specific memory, but the product of a momentary attribution. Such a finding would have both theoretical and practical implications. For example, it would suggest an alternative account of the mixed findings in the scientific literature with respect to the incoherence of traumatic memories. Furthermore, there has been some suggestion that the incoherence of traumatic memories is caused by increased amygdala activity and reduced hippocampus activity at encoding (Bisby, Burgess, & Brewin, 2020; Brewin, Gregory, Lipton, & Burgess, 2010). These changes in neural activity, then, lead to the insufficient binding of details. Because the resulting incoherence is thought to promote distress in the form of involuntary negative memories, some therapeutic treatments for PTSD target—and attempt to resolve—incoherence (see, for example, prolonged exposure therapy; Foa & Rothbaum, 1998). Then in therapy, as the memory is "rescripted," coherence increases (Brewin et al., 2010; see also, for a review, Brewin, 2014). But these ideas are predicated on the notion that the

coherence of a specific memory is a property of the memory itself. That is, there is no consideration of the possibility that feelings of coherence could be produced in the moment, by attributions that depend on retrieval conditions.

Experiment 1

Method

We pre-registered both of our experiments (see the Open Practices section) and posted our pre-registrations, materials, and data here: https://osf.io/jzbr7. We also report comparatively minor, but useful, information in the Supplementary Materials in Appendix B.

Subjects

We recruited subjects on Amazon's Mechanical Turk (MTurk) using the TurkPrime platform (Litman, Robinson, & Abberbock, 2017). We used the "Between ANOVA One-way or Two-way" Shiny R Web App to calculate "sample size for power correcting for uncertainty and publication bias" (Version 1.0.0; Anderson, Kelley, & Maxwell, 2017), which resulted in our target sample size of 1632 useable responses. We collected data, in batches, from a total of 2026 MTurk workers, and after exclusions (see Results) retained 1640 for analyses (M_{age} = 38.63, SD_{age} = 13.13; 29% identified as men, 70% identified as women, and 1% identified as gender diverse). Of these subjects, 94% reported English was their first language. Subjects received $0.70 for participating in the study.

Design

We used a 4 (memory type: positive, negative, important, traumatic) x 3 (retrieval task: describe then rate, rate only, probing then rate) between-subjects design. We report the number of subjects in each of these 12 cells, after exclusions, in the Supplementary Materials (range: 105–159).

Procedure

The experiment was approved by the University of Waikato's School of Psychology Research and Ethics Committee under the delegated authority of the University's Human Research Ethics Committee. The experiment was conducted in accordance with the ethical provisions of the World Medical Association Declaration of Helsinki. The experiment consisted of three phases, administered online using Qualtrics (https://www.qualtrics.com/).

Phase 1. Subjects nominated, in random order, memories of their most positive, negative, important, and traumatic events—all of which were to have occurred at least one month earlier. We chose these kinds of momentous memories to ensure we were addressing the "compared to what?" issue (Read & Lindsay, 2000; Rubin et al., 2008). We used positive memories because they should be comparable to traumatic memories in emotional intensity, but oppositely valenced (Rubin et al., 2008; Rubin, 2011). We used important memories because they should have had a comparable impact to traumatic memories on subjects' lives, but be less emotionally intense (Rubin et al., 2008; Rubin, 2011). Finally, we included an additional negative memory comparison group to address the extent to which memories for objectively traumatic events (per DSM-5; American Psychiatric Association, 2013) differ in coherence from memories for non-traumatic negative events, while being comparable in terms of negative valence and emotional intensity.

We adapted our instructional cues from Berntsen, Rubin, & Siegler (2011). To cue for positive memories, we asked subjects to produce a short title for the memory that "now makes you feel most positive." To cue for negative memories, we asked subjects to produce a short title for the memory that "now makes you feel most negative." To cue for important memories, we asked subjects to produce a short title for the memory of the

event that "now feels like your most important life event.” Finally, to tap into subjects’ history of trauma, we asked them to complete the Traumatic Life Events Questionnaire (TLEQ; Kubany et al., 2000). For each subject, Qualtrics then fed back the set of traumatic events the subject reported having experienced, and asked the subject to select and produce a short title for the event from that list that "now bothers you the most” (Berntsen, Willert, & Rubin, 2003). After each subject had produced a title for all of these types of memories, Qualtrics selected one of the four memories at random, piped it back to the subject, and asked them to “keep in mind the following memory” for the duration of the survey.

Phase 2. Next, subjects were randomly assigned to complete one of three retrieval tasks. We asked one-third of subjects to describe everything they could remember about their nominated event, and then to rate, using the Autobiographical Memory Questionnaire, the coherence of their memory (AMQ; Rubin, Schrauf, & Greenberg, 2003). We asked another third of subjects to complete a series of specific probing questions about their nominated event, presented in a random order, before rating coherence (see Table 1). We asked the final third to simply rate coherence with no additional retrieval task.

During pilot testing we discovered that subjects asked to complete a series of probing questions dropped out of the experiment at a higher rate than subjects asked to describe their memory, or simply rate coherence. More specifically, 52% of probing-then-rate subjects dropped out compared to 17% of describe-then-rate and 4% of rate-only subjects. In some ways, we considered this disproportionate rate to be evidence of a successful manipulation of retrieval conditions. Indeed, when we compare the amount of time taken to complete the experiment, “probing then rate” subjects took much longer than subjects completing the other two retrieval tasks ($M_{\text{probing-then-rate}}$ = 25.42 minutes, *SD*

Table 1

Probing Questions used in Experiments 1 and 2

- On what day of the week did the event happen?
- At what time of day did the event happen?
- What was the date? Provide the exact date in mm/dd/yyyy form as accurately as you can.
- Where did the event happen? Describe the setting where the event occurred in enough detail that someone else would be able to imagine it accurately.
- What was the weather like during the event?
- Picture the event in your mind and describe all the details you can see. Keep in mind that these details—and the relative positions of the details—might change over the course of the event.
- What were you doing just before the event? Describe what you did in enough detail that someone else would be able to imagine it accurately.
- What did you do just after the event? Describe what you did in enough detail that someone else would be able to imagine it accurately.
- Who was there? Note both the people involved and anyone else who experienced the event.
- What did you say during the event? Tell us the exact words you used at the time.
- What did the other people around you say during the event? Tell us the exact words they used at the time. If you were alone throughout, please write N/A.
- What actions did you take during the event? Describe everything you did throughout the event.
- What actions did the other people around you take during the event? Describe everything they did throughout the event, one at a time. If you were alone throughout, please write N/A.
- What were you thinking? Describe the thoughts you had at the time of the event.
- What were the other people around you thinking? If you were alone throughout, please write N/A.
- What were you wearing at the time of the event? Describe your entire outfit.
- What were the other people around you wearing at the time of the event? Describe their entire outfits, one at a time. If you were alone throughout, please write N/A.
- How did you feel? Describe the emotions you felt over the course of the event.
- How did the other people around you feel? Describe the emotions they felt over the course of the event. If you were alone throughout, please write N/A.
- What colors could you see at the time of the event?
- What flavors could you taste at the time of the event?
- What scents could you smell at the time of the event?
- What sounds could you hear at the time of the event?
- What other sensations could you feel during the event? Describe these sensations in as much detail as possible.

= 13.84, range = 7–82; $M_{\text{describe-then-rate}}$ = 12.30, *SD* = 8.10, range = 3–56; $M_{\text{rate-only}}$ = 7.99, *SD* = 5.08, range = 2–42). Nonetheless, this dropout rate was a threat to internal validity. Therefore, we changed our instructions such that people assigned to the "probing then rate" retrieval condition were told the following:

> In the next part of this study you will answer a series of open-ended questions. We really appreciate your participation in this study. To thank you, if you complete the study under the conditions outlined earlier, and answer all questions to the best of your ability, we will pay you a bonus of $0.30, bringing your total rate for participation to $1.00.

Subjects made ratings about 21 AMQ items (see the Supplementary Materials in Appendix B, as well as one attention check item we embedded in the AMQ. In line with our pre-registration, we created a coherence sum variable using the following three items from the AMQ, identified as measures of subjective coherence by Rubin et al. (2016): the *story* item, "My memory comes to me in words or in pictures as a coherent story or episode and not as an isolated fact, observation, or scene" (1 = *not at all*, 7 = *completely*), the *pieces* item, "This memory comes in pieces, with bits missing (1 = *not at all*, 7 = *completely*; reverse coded)," and the *setting* item, "As I remember the event, I can recall the setting where it occurred" (1 = *not at all*, 7 = *as if it were happening now*). Because the setting item does not clearly fit with our conceptualization of coherence—a memory that has a clear storyline and is not recalled in bits and pieces—we pre-registered our main analysis both including and not including setting in the sum variable. We found no differences in the pattern of results. Therefore, to ensure our main coherence measure was in line with our conceptualization of coherence, we report the results here without the setting item.

Phase 3. In the final phase of the experiment, subjects first classified their memory on the Cultural Life Script, a list of prototypical life events shared by a culture (CLS; Rubin, Berntsen, & Hutson, 2009; because these data are not central to our research question, we report them in the Supplementary Materials. Next, subjects rated "how much effort did it require to recall details from this event?" (1 = *no effort*, 5 = *a lot of effort*), and answered a yes/no question designed to capture Criterion A traumas: "Did the event involve actual or threatened death, serious injury, or sexual violence to yourself or others?" (American Psychiatric Association, 2013). All subjects then reported the extent to which they "tried to structure the memory for this event into a story" (1 = *not at all*, 5 = *completely*), allowing us to isolate the extent to which describe-then-rate subjects were especially inclined to create a narrative structure while completing their description. Finally, probing-then-rate and rate-only subjects freely described their memory, so we could exclude from analyses those with nonsensical responses.[5]

Results

Recall our primary aim was to determine the extent to which people make momentary attributions about the coherence of their memories, depending on the conditions under which they retrieve those memories. Before addressing this aim, we excluded 308 subjects on the basis of pre-registered exclusion criteria. Of these 308, 63 subjects provided nonsensical descriptions of their event or nonsensical responses to the probing questions; 136 subjects nominated either (a) a negative memory they rated below the midpoint of the negativity scale, (b) a positive memory they rated below the midpoint of the positivity scale, or (c) an important memory they rated below the midpoint of the significance scale; in addition, 173 subjects failed our attention check. We excluded some

[5] As pre-registered, we also used these descriptions to examine the objective coherence of subjects' memories. Because these analyses are not central to our research question, we report them in the Supplementary Materials.

subjects on the basis of more than one of these criteria. In addition to these pre-registered criteria, we also excluded another 78 subjects who did not provide a description of their nominated event[6]. We did not pre-register this criterion, because we did not anticipate it would happen. In total, then, we excluded data from 386 subjects, or 19%, leaving 1640 useable data points. We report the number of subjects in each cell, after exclusions, in the Supplementary Materials (range: 105–159).

We then carried out manipulation checks to ensure that subjects' nominated memories were, in fact, positive, negative, important, or traumatic: they were. We also checked that the bonus awarded to probing-then-rate subjects reduced the drop-out rate: it did. We report further information about these data in the Supplementary Materials.

Word Count

Subjects described their memories using a mean of 128 words (*Mdn* = 94, *SD* = 138.16, range = 5–2429), but there was no evidence to suggest subjects used more words depending on the type of memory they described, nor the way they retrieved that memory, $F(11, 1628) = 0.84$, $p = 0.595$.

Criterion A

Of subjects who were assigned to rate a traumatic memory, 65% answered yes to the Criterion A question, compared to 34% of "negative" subjects, 13% of "important" subjects, and only 5% of "positive" subjects. Although it might seem odd that 5% of subjects' positive memories could be classified as Criterion A traumas, a closer look suggests they were reported as positive because of the way subjects' friends rallied around them at the time, for example, or because of the relief that the event could have

[6] Probing-then-rate subjects were more likely to be excluded for not providing a description than their describe-then-rate and rate-only counterparts. Those excluded on this basis typically indicated they were not prepared to describe the event after having already answered all of the probing questions. For example, one subject wrote, "I have already written the details in bits in the previous section."

been much worse. We did not pre-register that we would restrict traumatic memories to Criterion A events only, nor that we would remove Criterion A events from positive, negative, or important memories. But we analyzed the data both with and without these exclusions, and the patterns were similar. Therefore, in line with our pre-registration, we report the data below without restricting the traumatic memories to Criterion A events, and without removing Criterion A events from the positive, negative, or important memories.

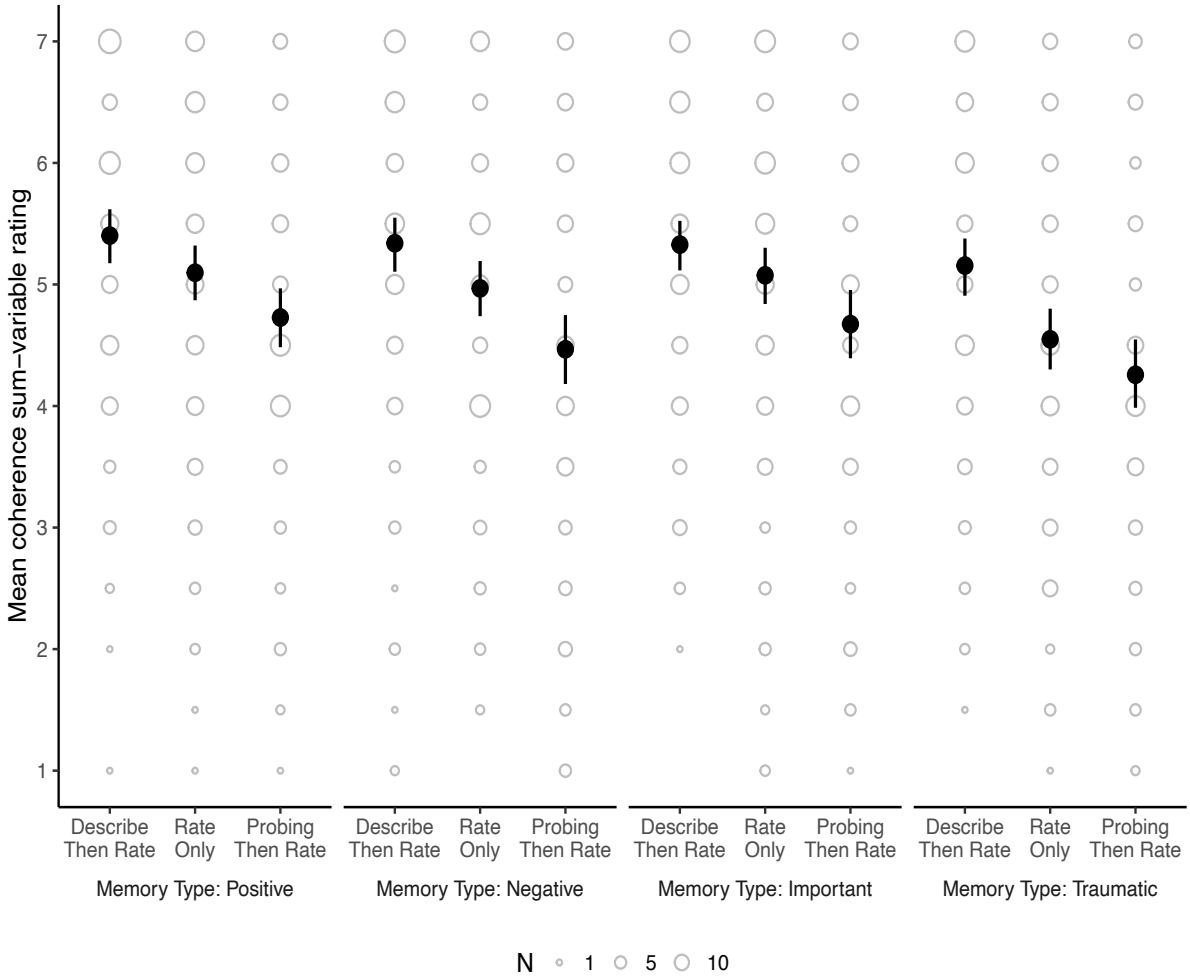
Judgments of Coherence

We now turn to our primary research question: To what extent do people make momentary attributions about the coherence of their memories, depending on the conditions under which they retrieve those memories? To answer this question, we determined the extent to which subjects' ratings of coherence differed depending on the type of memory they were assigned to rate and the way they retrieved that memory. We first calculated subjects' scores on our coherence sum variable, and then calculated the mean coherence sum variable score and 95% CI around it for each possible combination of type of memory and retrieval task. We display these data in Figure 1. The y-axis of Figure 1 represents mean coherence sum variable rating, with higher bars indicating greater coherence.

Figure 1 reveals three important findings. First, regardless of the type of memory they nominated and the way they retrieved that memory, subjects rated their memories as relatively coherent. Second, and in line with our predictions, subjects rated their memories as more, or less, coherent based on the way they retrieved their memory. More specifically, describe-then-rate subjects rated their memories as more coherent than their rate-only counterparts. Furthermore, probing-then-rate subjects rated their memories as less coherent than their rate-only counterparts. Third, although traumatic memories were

Figure 1

Coherence Sum Variable Ratings for Subjects' Positive, Negative, Important, and Traumatic Memories, Split by Retrieval Task



Note. Black points represent the mean for each group. Error bars represent 95% confidence intervals of the cell means. The size of the gray points represents the number of subjects whose mean coherence sum variable rating fell at that point on the scale.

rated as relatively coherent, they were consistently rated as less coherent than their positive, negative, and important counterparts. These results suggest that manipulating the way people retrieve a memory can influence how coherent they judge that memory to be,

and that traumatic memories are judged as slightly less coherent than their non-traumatic counterparts.[7]

Put another way, there was no Memory Type (positive, negative, important, traumatic) x Retrieval Task (describe then rate, rate only, probing then rate) interaction on ratings of coherence, $F(6, 1633) = 0.53$, $p = 0.788$. There was an effect of retrieval task on coherence, $F(2, 1637) = 37.25$, $p < .001$. Post-hoc Tukey comparisons revealed significant differences in coherence ratings comparing all combinations of retrieval tasks. More specifically, describe-then-rate subjects rated their memories as more coherent than both their rate-only counterparts, $M_{diff} = 0.38$, 95% CI [0.18, 0.58], and their probing-then-rate counterparts, $M_{diff} = 0.77$, 95% CI [0.56, 0.99]. Probing-then-rate subjects also rated their memories as less coherent than their rate-only counterparts, $M_{diff} = -0.39$, 95% CI [-0.58, -0.18]. There was also an effect of memory type on coherence, $F(3, 1636) = 6.44$, $p < .001$. Post-hoc Tukey comparisons revealed that subjects who reported traumatic memories rated those memories as less coherent than subjects who reported positive, negative, and important memories: $M_{diff} = 0.42$, 95% CI [0.15, 0.69], $M_{diff} = 0.27$, 95% CI [0.00, 0.54], and $M_{diff} = 0.37$, 95% CI [0.11, 0.64], respectively.

Effect of Retrieval Task. What is it about manipulating the retrieval task that affected people's judgments of coherence? Recall we proposed two possible mechanisms by which the probing questions might lead people to rate their memories as less coherent than their counterparts who did not answer probing questions. The first possibility is that probing questions might disrupt retrieval fluency, in turn leading people to make the

[7] Manipulating retrieval task also produced systematic changes in people's other AMQ ratings. Those who were asked specific questions reported lower ratings in response to most AMQ items than did those who were asked to describe their memory. These differences were smaller than the differences in coherence ratings, however, suggesting the specific retrieval tasks used in the experiments reported here produced targeted changes to judgments in coherence. We report people's AMQ ratings split by retrieval task in the Supplementary Materials (Table S2).

attribution that their memory is incoherent. To address this possibility, we asked subjects to report how much effort the tasks required; effort is a marker of disfluency (Alter & Oppenheimer, 2009). We found that probing-then-rate subjects reported putting in more effort to recall details of their event than all other subjects (see Table 2): compared to rate-only, M_{diff} = 0.85, 95% CI [0.69, 1.02]; compared to describe-then-rate M_{diff} = 0.65, 95% CI [0.48, 0.81]. These results provide some support for the hypothesis that asking people many specific questions about their events leads them to experience less fluency when retrieving a memory. These findings fit with the idea that probing-then-rate subjects misattributed a feeling of disfluency to the quality of their memories, leading them to judge those memories as less coherent than other subjects.

But there is at least a second possibility. Recall, we hypothesized that in the process of answering probing questions, people's attention would be drawn to parts of an experience they cannot recall. The greater this failure to recall, the more people might

Table 2

Ratings by Retrieval Task for Experiments 1 and 2

		Describe then rate			Rate only			Probing then rate		
Experiment	Rating	*M*	95% CI	*SD*	*M*	95% CI	*SD*	*M*	95% CI	*SD*
1[a]	Effort	1.83	[1.73, 1.92]	1.13	1.62	[1.54, 1.70]	0.96	2.47	[2.35, 2.60]	1.40
1[b]	Story	3.36	[3.26, 3.46]	1.22	3.03	[2.92, 3.14]	1.34	3.36	[3.25, 3.47]	1.22
2	Effort	1.91	[1.77, 2.05]	1.23	1.68	[1.57, 1.80]	1.02	2.67	[2.51, 2.83]	1.38
2[c]	Story	3.43	[3.28, 3.57]	1.27	2.84	[2.70, 2.98]	1.24	3.01	[2.87, 3.15]	1.19

[a] "How much effort did it require to recall details from this event?" (1 = *no effort,* 5 = *a lot of effort*)

[b] "While completing this study, I tried to structure the memory for this event into a story." (1 = *not at all*, 5 = *completely*)

[c] "While completing this study, I structured the memory for this event into a story." (1 = *not at all*, 5 = *completely*)

make the attribution that, for example, their memories are missing important pieces—a component of coherence. To address this possibility, we focused on the 486 probing-then-rate subjects and reviewed their responses to each of the 24 probing questions. We counted the number of times that each subject answered the 24 questions by saying "I don't know" (or a similar response that suggested the subject could not remember the detail in question). We found that as subjects reported more memory failures in response to the probing questions, they reduced their ratings of coherence; $r(484) = -0.31$, $p < .001$, 95% CI [-0.39, -0.23]. This finding suggests probing questions might lead people to experience a feeling of failure when attempting to recall details of a memory, in turn reducing their judgments about the coherence of that memory.

So far, we have shown data in support of at least two possible routes by which manipulating retrieval task might have affected people's judgments of coherence. We now turn to the "flipside" of this experiment. Recall, we hypothesized that describing a memory allows people to structure the memory into a narrative—one of the components of a coherent memory—which might, in turn, cause people to boost their judgments of coherence. To measure the extent to which subjects who described their memories used the description as an opportunity to structure the memory into a narrative, we asked everyone to indicate the extent to which they tried to structure the event into a storyline while completing the study. We found that describe-then-rate subjects tried just as hard to structure their memory into a story as probing-then-rate subjects, but slightly harder than rate-only subjects (see Table 2): $M_{diff} = 0.01$, 95% CI [-0.18, 0.19] and $M_{diff} = 0.33$, 95% CI [0.16, 0.51], respectively. This pattern suggests that asking people to describe their memories does not encourage them to form a narrative storyline any more so than asking people many questions about an event. It is possible, however, that these data are an artefact of the way we worded this question such that trying to structure an event into a

storyline does not mean success in doing so. Perhaps although probing-then-rate subjects put a similar amount of effort into structuring their memory into a story as describe-then-rate subjects, those who described the event succeeded and those who answered probing questions did not. We address this possibility in our second experiment.

Effect of Memory Type. We now turn to the effect of memory type on coherence. Recall that subjects who nominated traumatic memories rated their memories as less coherent than subjects who reported positive, negative, and important memories. Why? One possibility is that traumatic memories were rated as less coherent than positive and important memories simply because people rehearse their traumatic memories less frequently (Berntsen, Hoyle, & Rubin, 2019). Perhaps the unpleasant nature of these memories leads people to avoid thinking about them and talking about them with other people, resulting in fewer opportunities to structure the memory into a narrative, and fill in any gaps.

To address this possibility, we deviate from our pre-registration for an exploratory analysis of rehearsal ratings by memory type. We created a conscious rehearsal sum variable using two AMQ items: the *talked about* item, "Since it happened, I have talked about this event," and the *thought about* item. Both items were rated on a 7-point Likert-type scale (1 = *not at all*, 7 = *as often as any event in my life*). We found that subjects who nominated traumatic memories reported rehearsing their memories less than subjects who nominated important and positive—but not negative—memories (see Table 3): M_{diff} = 1.05, 95% CI [0.77, 1.33], M_{diff} = 0.95, 95% CI [0.67, 1.23], and M_{diff} = 0.16 [-0.12, 0.45], respectively. Furthermore, an exploratory analysis of the relation between conscious rehearsal and judgments of coherence suggests that the more people rehearse their memories, the more coherent those memories feel; $r(1638) = .20$, $p < .001$, 95% CI [0.15, 0.25].

Table 3

Conscious Rehearsal ratings by Memory Type

Memory Type	*M*	95% CI	*SD*
Positive	5.25	[5.10, 5.40]	1.55
Negative	4.47	[4.31, 4.63]	1.59
Important	5.36	[5.22, 5.49]	1.40
Traumatic	4.30	[4.14, 4.47]	1.63

Note. 1 = *not at all*, 7 = *as often as any event in my life*

These results provide some support for our hypothesis that traumatic memories feel slightly less coherent than other memories due to less frequent rehearsal. Therefore, although traumatic memories were rated as slightly less coherent than their positive, negative, and important counterparts, this finding might be due in part to people not rehearsing their traumatic memories as often. Moreover, although traumatic memories were consistently rated as less coherent than their non-traumatic counterparts, they were still rated as relatively coherent. Therefore, our findings do not support the idea that traumatic memories are incoherent.

Discussion

Taken together, our findings suggest coherence is sensitive to attributions that depend on retrieval conditions. We found no evidence to suggest traumatic memories are uniquely incoherent. It is possible, however, that people experiencing distress about an event might have more difficulty answering probing questions about that event than those not experiencing distress, which could exacerbate a feeling of incoherence (Brewin, 2007). We addressed this possibility in a second experiment.

Experiment 2

Method

Subjects

As in Experiment 1, we collected data in batches, excluding as we went, from a total of 1176 MTurk workers. After exclusions, which we detail in the Results, we retained 886 people for analysis (M_{age} = 37.87, *SD* = 18.13; 36% identified as men, 63% identified as women, and < 1% identified as gender diverse). Of these subjects, 96% reported English was their first language. We gave people $1.30 for participating, with a $0.70 bonus for people who answered the probing questions. The smaller sample size compared to Experiment 1 is attributable to the simpler design.

Design

We used a 2 (PTSD symptoms: below threshold, above threshold) x 3 (retrieval task: describe then rate, rate only, probing then rate) between-subjects design. We report the number of subjects in each of these 6 cells, after exclusions, in the Supplementary Materials (below-threshold range = 191–209; above-threshold range = 92–100).

Procedure

The procedure for Experiment 2 was identical to that used in Experiment 1, with the following four exceptions.

First, people did not nominate a positive, negative, or important event. Instead, everyone completed the TLEQ and provided a short title for the TLEQ event they reported having experienced that "now bothers you the most." We then asked everyone to keep that memory in mind for the duration of the experiment. Consistent with the findings from Experiment 1, 12% of the total sample reported never having experienced any of the TLEQ traumas (12.43% of describe-then-rate subjects, 11.21% of rate-only subjects, and

12.16% of probing-then-rate subjects). These subjects were instead asked to "bring to mind the memory of the event that now bothers you the most."[8]

Second, we measured the severity of people's PTSD symptoms in response to their nominated traumatic event by asking everyone to complete the revised Impact of Event Scale (IES-R; Weiss & Marmar, 1997). Qualtrics then calculated each person's scores on the IES-R and—on the basis of those scores and an established threshold in the literature (see Creamer, Bell, & Failla, 2003)—classified everyone into one of two groups: people who were *at or above the threshold* of 33, suggesting they were at high risk for meeting the diagnostic criteria for PTSD (n = 290, M = 49.81, SD = 13.34), and those who were *below threshold* (n = 596, M = 13.71, SD = 10.10). Within each of these threshold classifications, we randomly assigned people to complete one of the three retrieval tasks from Experiment 1.

Third, we included only those AMQ items that were relevant to our hypothesis, and reduced the items comprising the coherence sum variable to two items (the *story* item, "My memory comes to me in words or in pictures as a coherent story or episode and not as an isolated fact, observation, or scene," and the *pieces* item, "This memory comes in pieces, with bits missing"—reverse coded; see Supplementary Materials). Unlike Experiment 1, subjects rated the "story" and "pieces" items first, and in a random order, before then rating the remaining AMQ items, which were also presented in a random order.

Fourth, recall that in Experiment 1 both probing-then-rate and describe-then-rate subjects reported a similar (and moderate) level of "trying" to structure their memory into a story. We earlier raised the possibility that "trying" does not necessarily mean "doing"

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[8]As with Experiment 1, we used subjects' descriptions of their nominated events to examine the objective coherence of their memories, and report these analyses in the Supplementary Materials.

(see also Yoda, as quoted in Lucas & Kershner, 1980). Therefore, a critic could rightly observe that we still do not have evidence for our hypothesis that describing a memory helps people to structure the memory into a narrative, in turn boosting coherence for our describe-then-rate subjects. In line with this possibility, we changed the wording of that item to "while completing this study, I structured the memory for this event into a story."

Results

Recall the purpose of Experiment 2 was to answer the following question: to what extent do probing questions produce an exacerbated feeling of incoherence among people experiencing distress?

Before addressing this question, we first excluded data from 135 subjects on the basis of our pre-registered exclusion criteria. Of these subjects, 110 nominated a memory they rated below the midpoint of the negativity scale, 36 failed the attention check built into our ratings questionnaire, and one subject provided a nonsensical event description. Some subjects were excluded on the basis of more than one of these criteria. We also excluded data from a further 35 subjects who did not provide a description of their nominated event.

Consistent with the findings from Experiment 1, only 12% of the remaining sample (n = 120) reported having never experienced a TLEQ event. Because there were not enough subjects in this group to provide any interpretable information when split into our three retrieval conditions, and these data are not pertinent to our research question, we also excluded data from these subjects. In total, we excluded data from 290 subjects, leaving 886 useable data points.

Next, we examined the extent to which subjects' ratings of valence and intensity depended on their reported PTSD symptoms. In line with previous findings, above-threshold subjects rated their memories as more negative and more intense than below-

threshold subjects (Rubin et al., 2008). But regardless of symptom severity, the memories were rated as reasonably negative and intense. That is, for negativity, $M_{\text{above-threshold}}$ = 6.27, SD = 0.99, 95% CI [6.15, 6.38], $M_{\text{below-threshold}}$ = 6.02, SD = 1.04, 95% CI [5.93, 6.10], M_{diff} = 0.25 [0.11, 0.39]; for intensity, $M_{\text{above-threshold}}$ = 5.98, SD = 1.25, 95% CI [5.84, 6.13], $M_{\text{below threshold}}$ = 4.86, SD = 1.61, 95% CI [4.73, 4.99], M_{diff} = 1.13, 95% CI [0.93, 1.32].

Word Count

Subjects described their memories using a mean of 153 words (*Mdn* = 112, *SD* = 137.39, range: 4–1355). There was no evidence to suggest subjects used more words depending on the way they retrieved their memory, nor their reported PTSD symptoms, $F(5, 880) = 1.49$, $p = 0.190$.

Criterion A

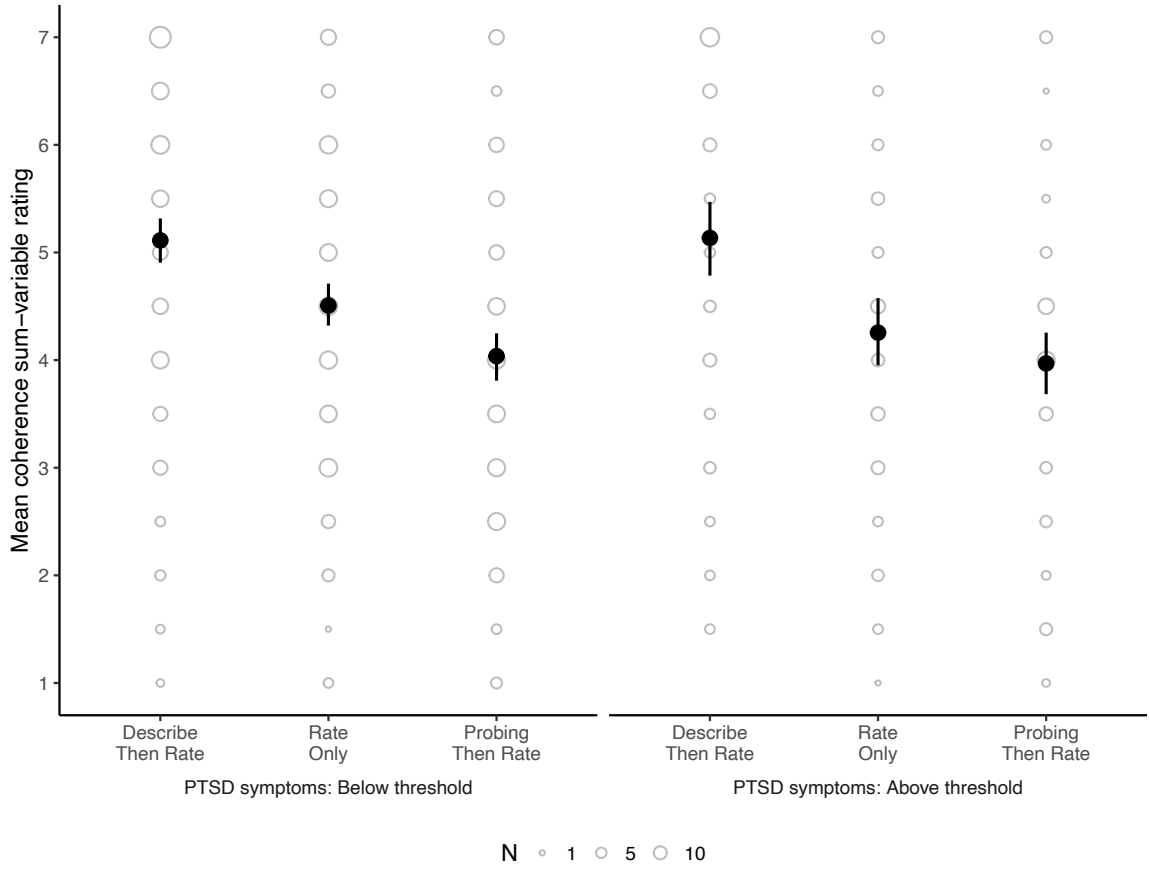
Sixty-nine percent of subjects said yes when asked, “Did the event involve actual or threatened death, serious injury, or sexual violence to yourself or others?”; we classified those events as Criterion A traumas. These findings are in line with those of Experiment 1. The patterns of results we report below do not change when we restrict the analysis to memories of Criterion A events. Therefore, to maximize the interpretability of our results and maintain random assignment, we report the data below for all 886 subjects, regardless of their response to the Criterion A question.

Judgments of Coherence

We now turn to our primary question: Among people experiencing distress, do probing questions exacerbate a feeling of incoherence? To answer this question, we first determined the extent to which subjects’ ratings of coherence differed depending on the conditions under which they retrieved their memory and the severity of their reported PTSD symptoms. We display these data in Figure 2.

Figure 2

Coherence Sum Variable Ratings for Subjects Below and at or Above the IES-R Threshold of 33, Split by Retrieval Task



Note. Black points represent the mean for each group. Error bars represent 95% confidence intervals of the cell means. The size of the gray points represents the number of subjects whose mean coherence sum variable rating fell at that point on the scale.

As Figure 2 shows, we replicated the patterns from Experiment 1. First, regardless of the way they retrieved their memory, subjects rated their memories as relatively coherent. Second, subjects rated their memories as more, or less, coherent based on the way they retrieved those memories. Again, describe-then-rate subjects rated

their memories as more coherent than their rate-only counterparts, and probing-then-rate subjects rated their memories as less coherent than their rate-only counterparts.

Figure 2 also reveals two additional findings. First, we did not find evidence to support the claim that traumatic memories are incoherent among people still experiencing distress. Regardless of the severity of their reported PTSD symptoms, subjects rated their traumatic memories as similarly coherent. Second, we found no evidence to suggest that, when asked probing questions, subjects who reported a high severity of PTSD symptoms reduced their judgments of coherence any more than those who reported a low severity of PTSD symptoms.

In statistical terms, we see there was no PTSD Symptoms x Retrieval Task interaction on ratings of coherence, $F(2, 883) = 0.52$, $p = 0.593$. There was, however, an effect of retrieval task on coherence, $F(2, 883) = 35.08$, $p < .001$. Post-hoc Tukey comparisons revealed significant differences in coherence ratings comparing all combinations of retrieval task. More specifically, describe-then-rate subjects rated their memories as more coherent than both their rate-only ($M_{diff} = 0.69$, 95% CI [0.39, 0.99]) and probing-then-rate counterparts ($M_{diff} = 1.11$, 95% CI [0.80, 1.41]). Probing-then-rate subjects also rated their memories as less coherent than their rate-only counterparts, $M_{diff} = -0.42$, 95% CI [-0.72, -0.12]. There was, however, no effect of PTSD symptom severity on coherence, $F(1, 884) = 0.78$, $p = 0.377$. Furthermore, when we deviated from our pre-registration to conduct an exploratory analysis of the relationship between reported PTSD symptoms and judgments of coherence, we did not find good evidence to suggest that judgments of coherence varied with PTSD symptoms, $r(884) = .04$, $p = .229$, 95% CI [-.03, 0.10].

Recall that in Experiment 1, we found some evidence to suggest answering probing questions might have disrupted subjects' feelings of retrieval fluency, leading

probing-then-rate subjects to rate their memories as less coherent than did their describe-then-rate and rate-only counterparts. Consistent with this finding, we again found that probing-then-rate subjects rated the recall process as more effortful than the other subjects (see Table 2); M_{diff} = 0.99, 95% CI [0.75, 1.22], and M_{diff} = 0.76, 95% CI [0.52, 1.00], respectively. These results provide further support for the idea that asking people many specific questions about their events might lead them to misattribute a feeling of disfluency to the quality of their memories, which in turn lowers their judgments about the coherence of those memories.

In Experiment 1, we also found evidence to suggest that probing questions might have led people to experience a feeling of failure when attempting to recall details of a memory, in turn reducing their judgments of coherence. We replicated this finding in Experiment 2, such that as subjects reported more memory failures in response to the probing questions, they reduced their ratings of coherence; $r(287) = -0.31$, 95% CI [-0.41, -0.21]. This finding provides further support for the hypothesis that probing questions might lead to momentary attributions about memory coherence, by way of a feeling of failure.

We now turn to the hypothesis that describing a memory helps people structure their memory into a storyline, which in turn boosts judgments of coherence. In Experiment 1, we addressed this hypothesis by asking subjects to report the extent to which they tried to structure the memory into a storyline, but inadvertently conflated "trying" with "doing." Therefore, we reworded that item accordingly. To what extent, then, did describe-then-rate subjects report actually structuring their memory into a storyline? The answer is that "describe then rate" subjects reported being more successful than both probing-then-rate and rate-only subjects (see Table 2): M_{diff} = 0.42, 95% CI [0.18, 0.66], and M_{diff} = 0.59, 95% CI [0.35, 0.82], respectively. This pattern suggests

that, as we hypothesized, asking people to describe a memory helps them to form a storyline for that memory, which in turn boosts judgments of coherence.

Taken together, these findings suggest that when people retrieve a memory, their judgments about the coherence of that memory are sensitive to momentary attributions made during recall, and depend on the conditions under which that memory is retrieved. We found no evidence to support the claim that traumatic memories feel incoherent among people currently experiencing distress, nor did we find evidence to suggest probing questions exacerbate a feeling of incoherence among people experiencing distress.

Discussion

Our results provide further evidence that coherence is sensitive to attributions made in the moment during recall, depending on the conditions under which a memory is retrieved. We found no evidence to suggest traumatic memories are especially incoherent, even among people experiencing distress.

General Discussion

Across two experiments and 2526 subjects, we found evidence that judgments of coherence are affected by the way people retrieve a memory. Specifically, people who answered probing questions rated their memory as less coherent than did those who simply rated it, who in turn reported less coherence than did those who described their memory. These effects persisted regardless of the extent to which people reported experiencing distress. We found no evidence to suggest traumatic memories are uniquely incoherent, even among people experiencing distress. Instead, our findings suggest judgments of coherence are sensitive to attributions people make in the moment during retrieval, depending on the conditions under which a memory is retrieved.

Our findings fit with a growing literature suggesting at least three ways that people treat feelings as information across a variety of tasks (Schwarz, 2012). First, probing questions reduced people's judgments of coherence. This finding fits with the idea that feelings of disfluency affect subsequent judgments in predictable ways (Alter & Oppenheimer, 2009; Weingarten & Hutchinson, 2018).

Second, probing questions were also accompanied by "I don't know" responses; as the number of these memory failures increased, people's ratings of coherence decreased. This pattern fits with two bodies of literature in which experiences of failure lead people to make attributions. For example, an experience of failure while attempting to recall a piece of information leads people to make the attribution that they will be less likely to be able to recall that information in the future (Finn & Metcalfe, 2008; King, Zechmeister, & Shaughnessy, 1980). Likewise, an experience of failure as an eyewitness (driven by feedback that the person one picked in a lineup is, in fact, not the suspect) leads people to make attributions about their abilities as eyewitnesses (Douglass & Steblay, 2006). This pattern is also reminiscent of a finding from another related literature in which people are asked to recall either many events from their childhoods (a difficult task) or few events (an easy task; Belli et al., 1998). In this literature, people asked to recall many events rate their memory for childhood as less complete than those asked to recall few—an effect partially driven by the memory failures of those asked to recall many events. Of course, because we did not manipulate the number of probing questions subjects answered, our analyses are correlational, and we therefore cannot be sure that experiences of memory failures among those answering probing questions are responsible for the subsequent reduction in coherence ratings.

Third, describing a memory led people to structure that memory into a story, in turn boosting their judgments of coherence. This finding fits with literature suggesting

people spontaneously organize information into stories, and doing so provides a mnemonic benefit (Bower & Clark, 1969; Campoy & Baddeley, 2008; Tulving, 1962). This finding also fits with another literature suggesting that "effort after meaning" helps develop a "coherent mental representation" (Zaromb & Roediger, 2009, p. 448; Bartlett, 1932).

The finding that PTSD symptoms did not vary with judgments of coherence is at odds with the idea that traumatic amnesia contributes to the development of PTSD (American Psychiatric Association, 2013; Foa & Riggs, 1993). Instead, our findings fit with the body of literature questioning the validity of the traumatic amnesia criterion for PTSD. For example, McNally (2009) has criticized the same criterion as predicated on the assumption that people encode into memory nearly every aspect of events. In addition, patients with PTSD rarely report experiencing traumatic amnesia, and the criterion is poorly correlated with the other PTSD criteria (Breslau, Reboussin, Anthony, & Storr, 2005; Yufik and Simms, 2010). Taken together, then, these findings add to a growing body of work questioning the value of the traumatic amnesia criterion for PTSD.

Collectively, our findings suggest a new account for the literature's mixed findings on the coherence of traumatic memories. Our data suggest coherence is not a property of a specific memory, but instead is sensitive to attributions people make in the moment depending on the conditions under which they retrieve that memory. This new account is consistent with earlier work in which people's ratings of coherence over a two-week interval were reasonably unstable (Rubin, Schrauf, & Greenberg, 2004). Our findings suggest momentous memories in general feel more or less coherent depending on the way those memories are retrieved. Still, even in the most difficult retrieval conditions, we found no data to suggest traumatic memories feel all that incoherent.

A critic might protest that in asking people to provide momentous memories, we were tapping into memories that were already coherent. For example, people might have brought to mind memories of events they have long thought about and talked about—events that now have a storyline (Berntsen et al., 2019). To the extent this criticism is valid, it is difficult to interpret the absolute levels of subjects' reported coherence. Our methods, then, might overestimate levels of memory coherence in the wider population. But regardless of the absolute levels of memory coherence, what is important here are the relatively similar levels of coherence for traumatic and non-traumatic memories.

Our results raise interesting questions worthy of future research. For example, we asked people only one set of probing questions—yet in forensic and clinical settings, people are often questioned repeatedly about the same events. What is the trajectory of coherence as a result of repeated questioning? When people cannot recall details over a series of interviews, would coherence steadily reduce by making those memory gaps salient? Or would coherence steadily increase by providing the opportunity and methods for people to fill in those gaps as a result of imagining, inferring, and so on (Garry, Manning, Loftus, & Sherman, 1996; Rubin et al., 2016)? Furthermore, how do retrieval conditions affect judgments of memory coherence after a longer delay? In the experiments reported here, people made coherence ratings shortly after recalling their memories. But the conditions under which someone retrieves a memory can have long-term memorial consequences (Tversky & Marsh, 2000). Perhaps, then, retrieval conditions not only produce systematic changes in momentary attributions of memory coherence, but also longer-term memorial changes.

Our findings have both theoretical and practical implications. Theoretically, our findings suggest the way we retrieve a memory has downstream consequences for judgments we make about that memory. These findings are at odds with the idea that

coherence is a property of a specific memory (Brewin et al., 2010), and instead suggest coherence can be the product of attributions, manipulated in the moment. Our findings also provide support for the idea that traumatic memories are not processed using a special mechanism (Bedard-Gilligan, Zoellner, & Feeny, 2017; cf. Brewin, 2007).

Practically, our findings suggest the way in which people retrieve their memories might affect their judgments about the coherence of those memories. Our findings, therefore, have practical implications for both clinical and forensic settings on at least two fronts. First, one goal of prolonged exposure therapy is to help clients develop a coherent memory of their trauma, so they can begin to recover (Foa & Riggs, 1993; Foa & Rothbaum, 1998). But if, across 886 people, we found no plausible evidence that people's judgments of coherence vary with their PTSD symptoms, perhaps it is time to ask if helping clients develop a coherent memory is truly crucial to recovery (see also, Bedard-Gilligan et al., 2017). Second, mental health professionals and forensic interviewers should be cognizant of the finding that coherence is not a stable property of memory—which means that when people are questioned, they might well make attributions about coherence on the basis of their feelings during retrieval.

Author contributions

All authors developed the study concepts and contributed to study design. A. Taylor and K. Jordan performed data collection and coding. A. Taylor performed analysis, and interpretation under the supervision of M. Garry and R. Zajac. M. Garry and R. Zajac wrote the funding proposal and are PIs; M. K. T. Takarangi is an AI. A. Taylor and M. Garry drafted the manuscript, and R. Zajac, K. Jordan, and M. K. T. Takarangi provided critical revisions. All authors approved the final version of the manuscript for submission.

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Data availability

All numerical data have been made available via the Open Science Framework and can be accessed at https://osf.io/jzbr7. This link will be password-protected, with passwords available to qualified professionals on request. We are unable to share descriptions of memories or answers to probing questions, as these data might contain identifying information.

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Chapter 3: Evidence From the Trauma-Film Paradigm That Traumatic and Nontraumatic Memories are Statistically Equivalent on Coherence.

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Abstract

There is contention in the scientific literature about the coherence of people's memories for trauma: Sometimes, traumatic memories are rated as less coherent than nontraumatic memories; other times, they "look" very similar. But several methodological challenges invite counter-explanations that hinder the interpretation of these findings. We set out to address these challenges by adopting a trauma-film-paradigm approach to examine the coherence of traumatic and nontraumatic memories. We developed a new set of materials for the trauma-film paradigm and then used these materials to examine the relative coherence of traumatic and various nontraumatic memories. We found that traumatic memories were not only fairly coherent but also statistically equivalent to their nontraumatic counterparts. Our hope is that scientists use these materials in experiments that complement the existing autobiographical-memory literature and allow for a greater understanding of the relation between memory and dysfunction.

Keywords: trauma-film paradigm, memory coherence, traumatic memories, PTSD, equivalence testing, open data, preregistered

Evidence From the Trauma-Film Paradigm That Traumatic and Nontraumatic Memories are Statistically Equivalent on Coherence

Memories for traumatic experiences are sometimes thought to be fragmented and jumbled and to come to mind in pieces; these impairments are said to be a consequence of the unique way in which people encode their memories for traumatic experiences (Brewin, 2016; van der Kolk & Fisler, 1995). Furthermore, traumatic experiences are thought to promote dissociation, which results in gaps in people's memories for trauma (Janet, 1889/1973; for a review, see Dalenberg et al., 2012; but see also, Bedard-Gilligan & Zoellner, 2012; Lynn et al., 2012). This lack of "coherence" is, in turn, said to contribute to the development and maintenance of posttraumatic stress disorder (PTSD; e.g., Brewin et al., 1996, 2010; Foa & Riggs, 1993; van der Kolk & Fisler, 1995). Related issues are evident in the criteria for PTSD as the "inability to recall important aspects of the trauma" found in the fifth edition of the *Diagnostic and Statistical Manual of Mental Disorders* (*DSM-5*; American Psychiatric Association, 2013; see McNally, 2009). Victims of trauma, then, might be driven to search for details that fill in the gaps in their memories—perhaps in therapy with a practitioner who encourages this approach with a view to resolve incoherence and, in turn, promote recovery (Belli et al., 1998; Foa & Rothbaum, 1998; Lindsay & Read, 1994; Poole et al., 1995).

The coherence of traumatic memories, then, has far-reaching implications, so it is important that understanding of this issue be grounded in solid scientific evidence. At first glance, it might seem the best way to gather this evidence would be to ask people about their traumatic and nontraumatic memories and then determine the extent to which these memories "look like" each other with respect to coherence. Some scientific studies have taken exactly this approach and found evidence that people's autobiographical traumatic memories are much like their nontraumatic counterparts on at least 28 different

measures of coherence (Rubin, 2011; Rubin et al., 2011, 2016). Furthermore, coherence does not seem to vary with posttraumatic stress. For example, in one recent study of more than 2,500 people, ratings of a memory's coherence did not vary with self-reported symptoms of PTSD and instead depended on the way in which that memory was retrieved (Taylor et al., 2020).

But at least three methodological challenges hinder interpretation of the findings in the existing literature and provide counter-explanations for these findings. First, what kind of memories should one select as the nontraumatic counterparts (Read & Lindsay, 2000; Rubin et al., 2008)? Using a carefully selected combination of counterpart memories would allow comparison with traumatic memories on a number of dimensions—such as valence and emotional intensity (Rubin et al., 2008). But few studies have taken this comprehensive approach (for review, see Crespo & Fernández-Lansac, 2016; O'Kearney & Perrott, 2006; but cf. Rubin et al., 2016; Taylor et al., 2020). Second, how reliable are the memories that people nominate? After all, there is evidence to suggest people struggle to remember what memory they nominated as their most positive or most negative memory just 2 days earlier (Taylor et al., 2017). Because it is reasonable to expect a third of the "most negative" memories to be of events that would meet the *DSM-5* criteria for traumatic stressors (American Psychiatric Association, 2013; Rubin et al., 2008), these data suggest caution in assuming that today's nominated traumatic, negative, or positive memory would also be tomorrow's. Unreliability, of course, injects threats into the interpretation and generalizability of findings, which makes it difficult to interpret findings from studies using these prompts to gather evidence about traumatic memories. And third, what actually happened during the events scientists ask people to recall? Typically, this information is not available, and so loss of detail, accuracy, or distortion cannot be determined.

Clearly, tackling these methodological challenges is necessary to establish the existence of causal relationships between different event types and memory coherence and, in turn, to advance the understanding of the relative coherence of traumatic and nontraumatic memories. Carefully controlled experiments can do just that (for a similar point, see Nielsen et al., 2020). One such experimental approach is the well-established trauma-film paradigm (Butler et al., 1995; Holmes et al., 2004; Horowitz, 1969; Lazarus et al., 1962; for reviews, see Holmes & Bourne, 2008; James et al., 2016). In experiments adopting this paradigm, subjects watch a film depicting a traumatic event or compilation of events and later answer various questions about the event and how it has affected them. The paradigm has been deployed to address the effect of various cognitive processes on the development of posttraumatic stress symptoms and the efficacy of laboratory interventions and therapeutic treatments aimed at reducing people's distress after a real-world traumatic experience (Hagenaars & Arntz, 2012; Halligan et al., 2002; Holmes et al., 2010). One typical finding, across more than 100 experiments, is that subjects tend to report a similar combination of PTSD-like symptoms as victims of genuine trauma do—albeit to a lesser extent and for a shorter duration (for reviews, see Holmes & Bourne, 2008; James et al., 2016). This body of experimental literature, then, suggests that using the trauma-film paradigm would be a fruitful means to address the issue of the coherence of traumatic and nontraumatic memories.

But the paradigm's utility to address this issue hinges on the use of key nontraumatic counterpart films that, in combination, permit comparisons with traumatic memories on important dimensions. To date, however, we have been unable to find any experimental work using the trauma-film paradigm in which these key counterpart films are properly normed for variables that could plausibly affect coherence to allow for these crucial comparisons. To address this puzzling and important gap in the literature, here we

report on the creation of a new set of materials that includes two films of traumatic events, two films of negative events, two films of positive events, and two films of neutral events. We included negative events because they should, like the traumatic events, induce negative affect; but they should differ from the traumatic events in that they are not traumatic stressors, as listed in the *DSM-5* diagnostic criteria for PTSD, because they do not contain exposure to "actual or threatened death, serious injury, or sexual violence" (American Psychiatric Association, 2013, p. 271). We included positive events because they should, like the traumatic events, be emotionally intense; but they should differ from the traumatic events in that they induce positive—as opposed to negative—affect (Rubin et al., 2008, 2016). Finally, we included neutral events because they are neither emotionally intense nor valenced and should induce a neutral affective state (Schaefer et al., 2010).

We aimed to create materials that

1. Induce expected positive or negative affect in viewers. More specifically, we expected the traumatic and negative films to increase negative affect. This increase would not necessarily be to the same extent because events satisfying Criterion A are assumed to be rare and severe enough to produce distress in the majority of people experiencing such an event (American Psychiatric Association, 2013; McNally, 2009). Therefore, it is possible the traumatic films would produce a greater increase in negative affect than the negative films. In addition, although it seems intuitive to expect the traumatic and negative films to decrease positive affect, positive and negative affect are distinctive constructs and not simply the opposite of one another (Watson et al., 1988; Watson & Tellegen, 1985; but see also, Crawford & Henry, 2004). An increase in negative affect, then, might not be accompanied by a decrease

in positive affect. We did not make a prediction about the effect of positive films on negative affect for the same reason—but did expect the positive films to increase positive affect. Finally, we expected the neutral films to produce only slight changes in subjects' reported positive and negative affect.

2. Can be considered equivalent with respect to *storyness*, or how easy viewers think it is to follow the events depicted. Prior work suggests that aspects of films such as clarity, cohesiveness, coherence, understanding, and comprehensibility could have downstream consequences for the coherence of memory (Segovia et al., 2016). Therefore, the negative, positive, and neutral films should be equivalent to the traumatic films on storyness.
3. Have low rates of previous exposure. Because people's previous exposure to a film could plausibly affect both the coherence of their memories and induced affect, we aimed to create a set of films that many people had not already seen.

Meeting these criteria would put us in a position to examine the issue of coherence in people's traumatic memories compared with nontraumatic memories in an experimental design that addresses long-standing counter-explanations for findings in the literature. This design would allow for comparisons of traumatic memories to negative, neutral, and positive memories and establish the causality required to advance the understanding of the relative coherence of traumatic and nontraumatic memories. Using this approach and drawing on the existing scientific evidence about the coherence of autobiographical traumatic memories, we hypothesized not only that people's memories of the traumatic films would be coherent but also that such coherence would be "equivalent" (Lakens, 2017) to that of memories for nontraumatic films. Such a finding

would be not only an interesting use of these materials per se but also an important contribution to the literature.

Experiment 1

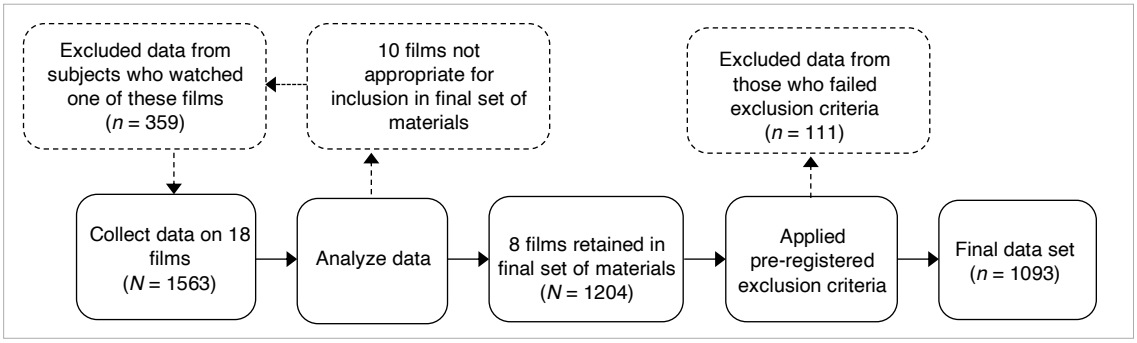
Method

We followed a preregistered plan (https://osf.io/yzk69/) to collect and analyze data in increments, replacing films with alternatives as we went if subjects were not responding to the films the way we expected (see Fig. 1). More specifically, we replaced a film if (a) subjects' responses suggested that film was not inducing the expected type of affect (i.e., traumatic and negative films were not inducing negative affect or positive films were not inducing positive affect), (b) subjects' responses suggested they found it much easier or much more difficult to follow the event depicted by that film compared with the events depicted by the other films (i.e., the films had unequal levels of storyness), and (c) more than 20% of subjects reported they had previously watched that film. The films detailed in the procedure pertain to our final set of materials for the trauma-film paradigm.

We adopted an "equivalence testing" approach, which allowed us to assess support for the null hypothesis that the films depict events that are equivalent on storyness and, subsequently, memory coherence (Lakens, 2017). When using an

Figure 1

Data Collection and Exclusion Processes



equivalence-testing approach, experimenters set a "smallest effect size of interest"—the minimum size effect they would consider large enough to be meaningful. A significant equivalence test, therefore, suggests the observed effect is smaller than that of interest and the observations can be considered equivalent. Our preregistration, numerical data, materials, and additional analyses are available on the project's OSF page (https://osf.io/uqtn6); supplemental information is available in Appendix C.

Subjects

We recruited subjects from the United States and Canada on Amazon's Mechanical Turk (MTurk) using the CloudResearch platform (https://www.cloudresearch.com/; Litman et al., 2017). Sample size was a result of calculations for the smallest effect size of interest (Lakens, 2017). We set our smallest effect size of interest at a raw difference of 0.5 on a 7-point Likert scale (D. Lakens, personal communication, June 27, 2019). According to previous research with similar Likert-scale items, a raw difference of 0.5 equates to a Cohen's *d* of approximately 0.4, which, in turn, required a sample size of 136 per film for an equivalence test with 90% power. We therefore set our target sample size at 136 subjects for each of the eight films, which totaled 1,088 usable responses required in the final data set. As we describe below, our preregistration specified a rolling analysis of batched data, replacing films as necessary. We collected data, in batches, from a total of 1,563 MTurk workers and ultimately retained 1,093 subjects for analysis (mean age = 41.23 years, *SD* = 13.57; 62% women, 37% men, < 1% gender diverse; 95% reported English was their first language). Subjects received $0.40 for participating in the study.

Design

We used a between-subjects design with one factor (type of film) and four levels: traumatic, negative, positive, and neutral.

Procedure

All experiments reported here were approved by the University of Otago Human Ethics Committee, with reciprocal notification to the University of Waikato. The experiment was conducted in accordance with the ethical provisions of the World Medical Association Declaration of Helsinki. Before subjects provided their consent to participate in the study, they were informed that the film they would be asked to watch "may contain emotional content, such as a road traffic accident or stillbirth" and that "it is possible that some people may experience distress or discomfort when watching these videos." We therefore asked that if anyone thought they might be adversely affected by viewing any of these films, they should not participate in the study. The experiment proper consisted of five phases, administered online using Qualtrics (https://www.qualtrics.com/).

Phase 1. First, we measured subjects' baseline positive and negative affect by asking them to complete the Positive and Negative Affect Schedule (PANAS; Watson et al., 1988). The PANAS consists of 20 affect-related words; for each word, subjects indicated the extent to which they "feel this way right now" on a 5-point Likert-type scale (1 = *very slightly or not at all*, 5 = *extremely*). Both the positive and negative subscales have good internal reliability; Cronbach's αs are between .84 and .90, depending on the subscale and specific instructions (Watson et al., 1988). In the current study, Cronbach's α for both subscales ranged from .90 to .93.

Phase 2. Next, to verify that subjects' audio was working, they first listened to an audio clip prompting them to "select the number 9 below." Subjects could not progress through to the next phase until they passed this sound-check. Subjects were then randomly assigned to watch one of eight films—two of these films depicted traumatic events, two depicted positive events, two depicted negative events, and two depicted

neutral events.[9] Each film was edited to be approximately 3 min long (*M* = 2 min 56s, range = 2 min 26s to 3 min 15s). Subjects were asked to "please pay attention and watch the entire video once only. When the video ends, click the 'next' button to progress through the survey." We locked out the "next button" for the duration of subjects' assigned film.

The first of the two traumatic films (Traumatic Variant 1) depicted a graphic car accident and was adapted from its prior use in experiments making use of the trauma-film paradigm (Strange & Takarangi, 2012). The second film (Traumatic Variant 2) depicted a pregnant woman finding out her baby no longer had a heartbeat and then giving birth to a stillborn child.

The first of the two negative films (Negative Variant 1) depicted a young boy with a facial disfigurement overhearing his peers making fun of him. The second film (Negative Variant 2) depicted a teenage boy discovering a storage locker containing gifts from his mother for all the big occasions to come in his life in case she did not survive cancer to be there with him for those occasions.

The first of the two positive films (Positive Variant 1) depicted a woman's gold-medal-winning ice-skating routine at the Olympics and was adapted from prior work in which it has induced positive emotions (Green et al., 2016; Gruber et al., 2008). The second film (Positive Variant 2) depicted travelers receiving Christmas gifts from an airline after a flight on Christmas Eve.

The first of the two neutral films (Neutral Variant 1) depicted a man talking about the history of parquet flooring and how a parquet floor is made. The second film (Neutral Variant 2) was an instructional film on how to use chopsticks.

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[9] Additional information about these films is reported at OSF (https://osf.io/gkbja/).

Phase 3. After watching their assigned film, subjects again completed the PANAS. Then, to measure the extent to which they found it easy to follow the event depicted by the film—that is, the film's storyness—we asked subjects the following five questions: (a) "How clear did you find the story in the video?" (clarity); (b) "How cohesive did you find the story in the video?" (cohesiveness); (c) "How coherent did you find the story in the video?" (coherence); (d) "How well do you believe you understood what happened in the story?" (understanding); (e) "Did the story in the video make sense to you?" (comprehensibility). These items were drawn from Segovia et al. (2016), appeared in a random order, and were rated on a 7-point Likert-type scale (1 = *not at all*, 7 = *completely*). We created a sum variable of storyness by taking subjects' mean ratings of these five items, and we report findings about the individual items on OSF (https://osf.io/gkbja/) and in the supplemental information.

Phase 4. Subjects then completed a Sudoku puzzle for 5 min, after which we asked them to rate the coherence of their memory for the film they watched. We measured coherence using two items from the Autobiographical Memory Questionnaire (Rubin et al., 2003, 2004): the story item, "My memory comes to me in words or in pictures as a coherent story or episode and not as an isolated fact, observation, or scene" (1 = *not at all*, 7 = *completely*), and the pieces item, "This memory comes in pieces, with bits missing" (1 = *not at all*, 7 = *completely*; reverse-coded). These items appeared in a random order, and we created a memory coherence sum variable by taking subjects' mean ratings of these two items (see also Taylor et al., 2020).

We then asked subjects some additional questions about their assigned film, such as how traumatic and how pleasant they found it, the extent to which they found the film personally relevant, and how much attention they paid to the film. Because these data are not related to our aims and showed nothing unexpected, we report them on OSF

(https://osf.io/gkbja/) and in the supplemental information. We also embedded an attention-check item in this section of the survey so we could exclude subjects who were not paying attention. To determine whether subjects had actually watched and paid attention to the film, we asked them to "take your time and report everything that you can remember about the video. Make sure to leave nothing out. Start at the beginning and give a complete description of the entire video." To measure previous exposure, we asked subjects whether they had seen the film before.

Phase 5. Finally, subjects provided some information about basic demographics (age, gender, first language, and primary language) and answered some questions about the extent to which they complied with our instructions throughout the experiment (e.g., "Did you watch the entire video, from start to finish (*yes/no*)?").

Results

Our primary aim was to develop a set of materials for the trauma-film paradigm that would (a) induce expected positive or negative affect, (b) be considered equivalent on storyness, and (c) have low rates of previous exposure.

Before we address this aim, recall that we preregistered that we would replace films during the course of data collection if (a) subjects' responses suggested that film was not inducing the expected affect, (b) subjects' responses suggested the film was different from the other films on storyness, or (c) more than 20% of subjects reported they had previously watched that film. These criteria led us to replace 10 films over the course of data collection and to exclude the associated data from the 359 subjects who had watched one of those films (more information about these films is available on OSF: https://osf.io/gkbja/) and in the supplemental information.

We then applied our preregistered exclusion criteria to the data from the 1,204 subjects who watched one of the final eight films and excluded data from 111 subjects.

Of these 111, 18 subjects failed the attention check, 15 subjects reported failing to complete the experiment in one session, four subjects reported not watching the entire film, 15 subjects reported watching the film more than once, 15 subjects reported not being able to hear the audio in the film, and 66 subjects provided descriptions of their assigned film that were nonsensical or inaccurate or that suggested they had missed a key aspect of the film (note that some subjects fell into more than one of these categories). Applying these exclusion criteria resulted in a final data set of 1,093 subjects.

As preregistered, we conducted our primary analyses both with and without subjects who said they had previously watched the film. We found no differences in the pattern of results. Therefore, to maximize power and maintain random assignment, we report the results here including those subjects who reported previously watching that film. In typical laboratory use, we would expect each variant of film type to be counterbalanced. Therefore, throughout the Results, we report our findings in a manner consistent with this counterbalancing by collapsing findings across the variants of each type of film.[10]

Criterion 1: Induce Expected Positive or Negative Affect

We turn to our primary aim and begin by addressing the first of our three primary criteria: To what extent did the set of materials for the trauma-film paradigm induce expected positive or negative affect? To answer this question, we first calculated each subject's prefilm and postfilm PANAS scores (Watson et al., 1988). We then determined that there were no prefilm differences in subjects' PANAS scores depending on which film they watched. To maximize the ease of data interpretation, we report these pre-film

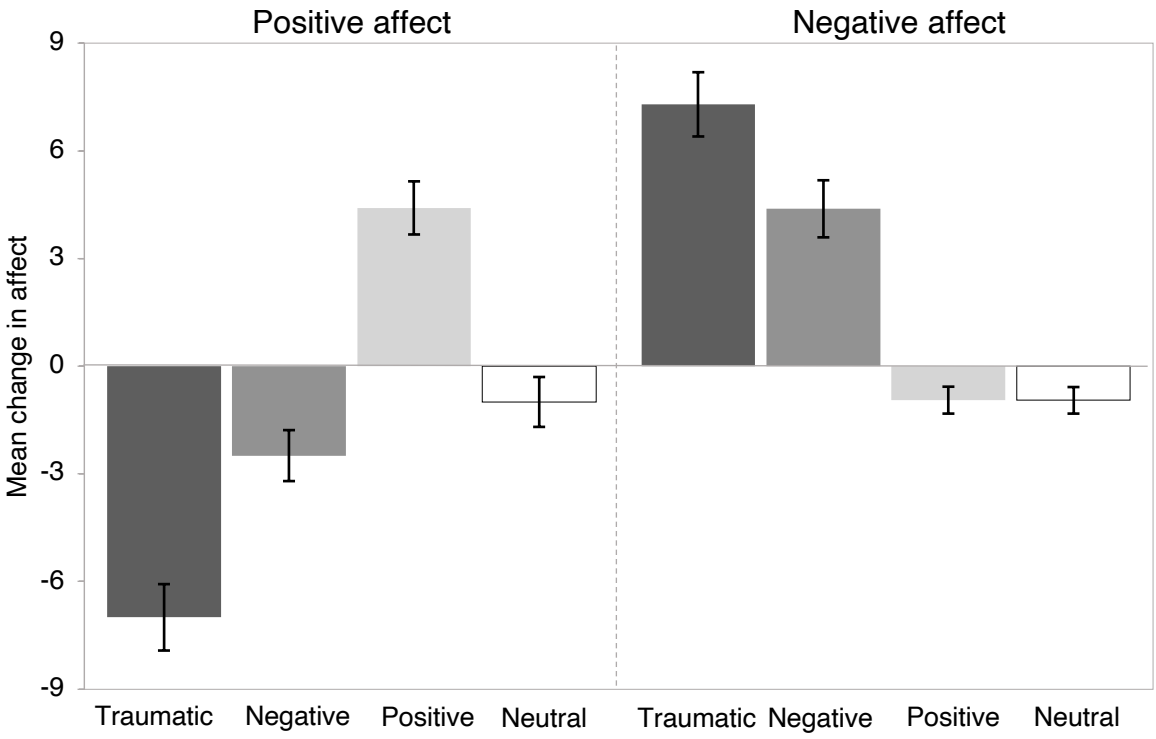
[10] For all analyses reported here, we report the data for each variant of type of film at OSF (https://osf.io/gkbja/) and in the supplemental information.

scores on OSF (https://osf.io/gkbja/) and instead consider here subjects' mean change in both positive and negative affect.[11]

We present these data in Figure 2. The *y*-axis represents subjects' mean change in affect; bars above the baseline represent an increase in affect, and bars below the baseline represent a decrease in affect. The *x*-axis shows the type of film subjects watched. The graph shows subjects' change in positive affect (left) and negative affect (right). To the degree that our set of materials induced expected affect, we should observe several patterns in Figure 2. Starting on the left side of the graph in Figure 2, first, subjects who watched a positive film should report increased positive affect, and that is what Figure 2 shows. Second, subjects who watched a neutral film should report only slight changes in

Figure 2

Subjects' Mean Change in Positive and Negative Affect



Note. Error bars represent 95% confidence intervals of cell means.

[11] We also report subjects' change for each individual PANAS item at OSF (https://osf.io/gkbja/) and in the supplemental information.

their positive affect; here, that translates to the slight decrease in positive affect that Figure 2 shows. Third, recall that because positive and negative affect are two separate constructs, we made no particular predictions about the effect of the traumatic and negative films on positive affect. Nonetheless, Figure 2 shows that both traumatic and negative films decreased positive affect.

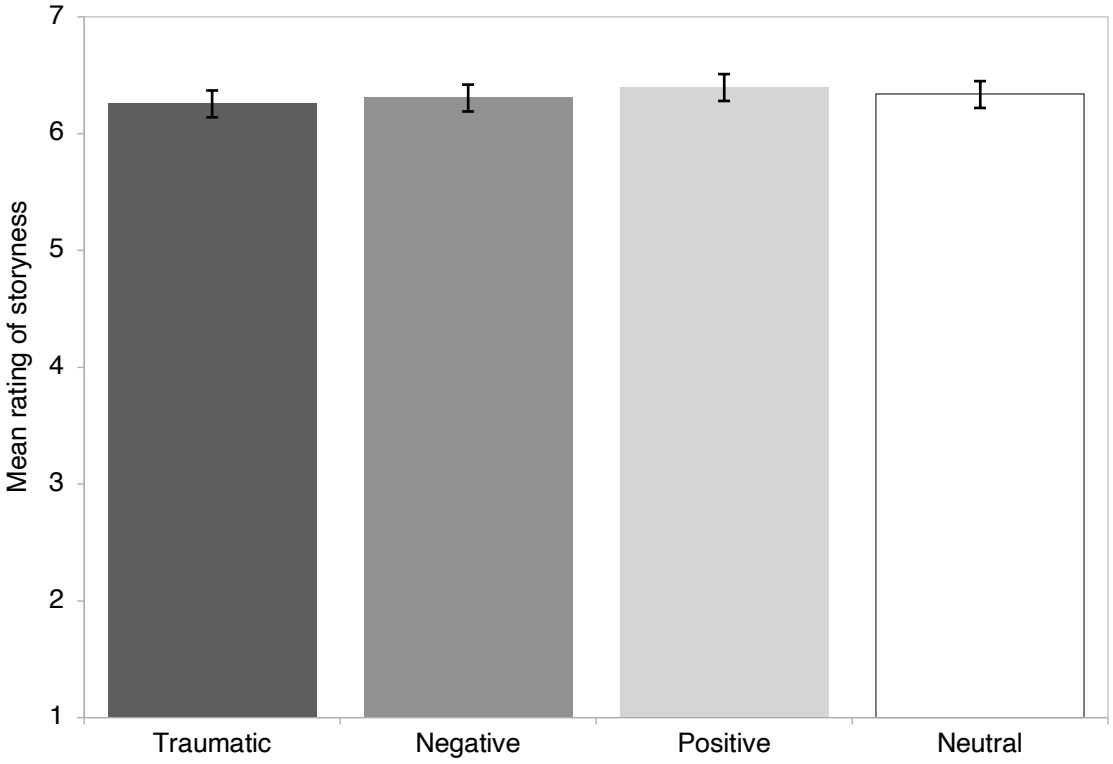
On the right side of the graph in Figure 2, first, subjects who watched a negative or a traumatic film should report increased negative affect, and that is what Figure 2 shows. Second, subjects who watched a neutral film should report only slight changes in their negative affect; here that translates to the slight decrease in negative affect shown. And third, again we made no specific prediction about the effect of positive films on negative affect; as Figure 2 shows, they produced a small decrease. Taken together, then, these films satisfy our criteria of inducing expected positive or negative affect. But a closer look at Figure 2 shows that the traumatic films increased subjects' negative affect to a greater degree than the negative films. We return to this finding in the General Discussion.

Criterion 2: Can be Considered Equivalent on Storyness

We now turn to the second of our three primary criteria: To what extent did the different types of films depict events that could be considered equivalent on storyness? Recall that after watching their assigned film, subjects answered five questions designed to measure that film's storyness (Segovia et al., 2016). We then created a sum variable with these questions, calculated subjects' mean storyness ratings, and classified them according to type of film; we present these data in Figure 3. To the degree that this set of materials could be considered equivalent on storyness, Figure 3 should depict this equivalence, and it does. Furthermore, when we conducted an equivalence test on

Figure 3

Subjects' Mean Ratings of Storyness as a Function of Film Type



Note. Error bars represent 95% confidence intervals of cell means.

subjects' mean ratings of storyness, we found that the traumatic films were equivalent to the negative, positive, and neutral films on this measure (negative films: mean difference = −0.05, 95% confidence interval [CI] = [−0.25, 0.14], $p < .001$; positive films: mean difference = −0.15, 95% CI = [−0.34, 0.05], $p < .001$; neutral films: mean difference = −0.09, 95% CI = [−0.28, 0.11], $p < .001$). Considered together, then, these data suggest this package of materials for the trauma-film paradigm can be considered equivalent on storyness.

Criterion 3: Have low Rates of Previous Exposure

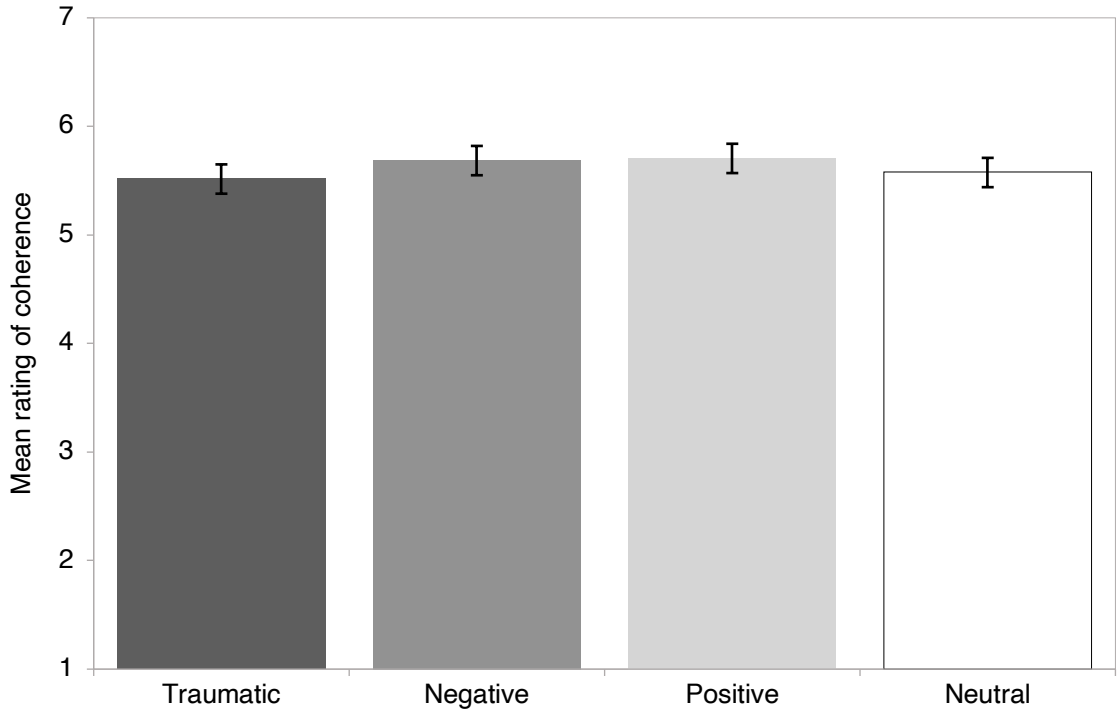
We now turn to our final criterion: To what extent did the films have low rates of previous exposure? To answer this question, we calculated the percentage of subjects who answered yes to the question asking whether they had seen the film before and classified their responses according to type of film. We found low rates of reported previous exposure: 4.03% for a traumatic film, 6.59% for a negative film, 4.40% for a positive film, and 2.19% for a neutral film. In short, these materials for the trauma-film paradigm are unlikely to be familiar to participants.

Coherence of Memory

Taken together, our findings suggest these novel films for the trauma-film paradigm induce expected positive or negative affect, can be considered equivalent on storyness, and have low rates of previous exposure. Therefore, we now ask how coherent people's traumatic and nontraumatic memories were. To answer this question, we calculated subjects' mean rating of the two-item memory coherence sum variable and classified that variable by type of film. Results are displayed in Figure 4. As Figure 4 shows, subjects rated their memories as fairly coherent across all types of films. More to the point, traumatic memories were rated as just as coherent as their negative, positive, and neutral counterparts. We then conducted equivalence tests to determine the degree to which coherence was equivalent for traumatic and nontraumatic memories. These equivalence tests showed the coherence of subjects' memories for the traumatic films was equivalent to that of memories for the negative, positive, and neutral films (negative film: mean difference = −0.17, 95% CI = [−0.41, 0.07], $p < .001$; positive film: mean difference = −0.20, 95% CI = [−0.44, 0.05], $p < .001$; neutral film: mean difference = −0.06, 95% CI = [−0.30, 0.18], $p < .001$). Considered as a whole, these results do not fit with the idea that traumatic memories are incoherent or less coherent than their

Figure 4

Subjects' Mean Ratings of Memory Coherence



Note. Error bars represent 95% confidence intervals of cell means.

nontraumatic counterparts (see also Rubin et al., 2016; Taylor et al., 2020; but cf. Brewin, 2007, 2014; Sachschal et al., 2019).

A critic might suggest an obvious counter-explanation: that because of the short delay between watching the films and making coherence ratings, perhaps incoherence did not have time to emerge; likewise, perhaps symptoms of posttraumatic stress did not have time to emerge. Put another way, to the extent that incoherence is related to people's distress (Brewin, 2014; cf. Rubin et al., 2016), maybe the reason we did not detect evidence of incoherent traumatic memories is simply because the films were not good analogues for real-life traumatic events and the symptoms they often produce. To address this counter-explanation, we briefly report findings from an additional experiment.

Experiment 2

This experiment was originally conducted as a pilot experiment for a related study using some of the materials reported in this article. The a priori aim of this preregistered[12] experiment was to examine how coherent people's memories for analogue traumatic and positive events are after a 5-min delay compared with a 48-hr delay. A full treatment of the method for this experiment appears in the supplemental information in Appendix C and we report here only the details of this experiment relevant to the counter-explanation we seek to address.

Method

Subjects

We recruited subjects on MTurk using the CloudResearch platform. We collected data from a total of 246 MTurk workers and ultimately retained 229 subjects for analysis (mean age = 40.70 years, *SD* = 12.35; 63% women, 37% men; 93% reported that English was their first language). Subjects received $1.30 for participating in the experiment ($0.30 for the first session and $1 after completion of the second session).

Design

We manipulated film type (traumatic, positive) between subjects.

Procedure

This pilot experiment consisted of two sessions, separated by approximately 48 hr (*M* = 49.61 hr, *SD* = 6.69, *Mdn* = 47.8).

Session 1. As in Experiment 1, subjects completed a soundcheck before being randomly assigned to watch either a traumatic film (*n* = 113) or a positive film (*n* = 116).

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[12] The preregistration is available at OSF (https://osf.io/5fs4p/).

We counterbalanced which of the variants of film type reported in Experiment 1 subjects watched.

Session 2. Subjects made ratings about the coherence of their memories for their assigned film on four items: the story and pieces items (as in Experiment 1) and two new items piloted in this experiment to capture a broader range of aspects of memory coherence—the fragments item ("As I remember the event, my memory comes to me as a collection of disconnected fragments") and the jumbled item ("As I remember the event, my memory feels jumbled, with parts of the event coming to me out of order"). All items were rated on a 7-point Likert-type scale (1 = *not at all*, 7 = *completely*). We created a memory-coherence sum variable by reverse-coding the pieces, fragments, and jumbled items and calculating subjects' mean rating of the four items. The sum variable had a Cronbach's α of .87.

Then, we used a version of the revised Impact of Event Scale (IES-R; Weiss & Marmar, 1997) to ask subjects about the symptoms of posttraumatic stress they had experienced since watching the film. We adapted the IES-R instructions for use over a 2-day delay rather than the typical 7 days. Although a 7-day delay is more common in the literature on the trauma-film paradigm, shorter delays are reasonably common; more to the point, the literature shows symptoms emerge over a shorter delay (e.g., Strange & Takarangi, 2012).

Results

We report here three relevant findings from this experiment. First, regardless of the type of film they watched, subjects' memories were fairly coherent (traumatic film: M = 4.90, 95% CI = [4.66, 5.15]; positive film: M = 4.87, 95% CI = [4.60, 5.14]). In a replication of our findings from Experiment 1, subjects rated their memories for traumatic

and positive films as equivalent on coherence (*p*s < .01). Again, there was no evidence that subjects' traumatic memories were incoherent.

Second, subjects reported symptoms of posttraumatic stress about the traumatic films. The mean total score on the IES-R was 11.70 (95% CI = [9.40, 14.00]). This mean total score is comparable with those typically found in the literature after similar delays (e.g., Porcheret et al., 2015; Short et al., 2020). Each subscale revealed symptoms (Intrusions: $M = 0.5$, 95% CI = [0.38, 0.62]; Avoidance: $M = 0.77$, 95% CI = [0.63, 0.91]; Hyper-arousal: $M = 0.26$, 95% CI = [0.16, 0.36]). Put another way, the films produced a titrated "dose" of symptoms, in line with recommendations that trauma films should be sufficiently distressing to produce symptoms and model trauma without generating a high degree of stress (James et al., 2016).

And finally, there was no evidence of a relation between coherence and symptoms; $r(111) = -.07$, 95% CI = [−0.25, 0.12], $p = .46$. We report this correlation with two caveats: First, the correlation itself was not preregistered, and second, in typical research scenarios, stable correlations warrant at least 260 people (Schönbrodt & Perugini, 2013, 2018). Taken together, these data provide some evidence against the idea that we did not detect incoherence because the films were not "traumatic enough" to produce symptoms.

General Discussion

Our aims here were twofold. First, we aimed to develop a set of materials for use in experimental work involving the trauma-film paradigm. Second, we then aimed to use these materials to address the relative coherence of people's memories for traumatic and nontraumatic events. The data suggest we accomplished both aims. The films induced positive or negative affect as expected and depicted events of different valence and emotional intensity that were equivalently easy to follow and were not familiar to the vast

majority of our sample. We then used these materials to address the question of coherence in people's traumatic memories compared with nontraumatic memories and found that regardless of the type of film they watched, not only were people's memories fairly coherent—but also traumatic and nontraumatic memories were equivalently coherent.

Recall that we found that the traumatic films increased subjects' negative affect to a greater degree than did the negative films. We did not necessarily expect these films to produce changes in negative affect to the same degree given the assumed distressing nature of events that satisfy the *DSM-5* criterion for objective traumas (American Psychiatric Association, 2013). Nonetheless, it is important to consider whether this finding might have produced the hypothesized pattern of equivalent coherence we found for traumatic and nontraumatic memories. In short, the finding would have worked against our hypothesis. To understand why, let us assume that as events become more distressing, memories become more incoherent (Brewin, 2014; Metcalfe et al., 2019; but see Rubin et al., 2016; Taylor et al., 2020). If this assumption was correct, the finding that traumatic films generated greater negative affect than negative films would have led us to expect traumatic-film memories to be more incoherent than negative-film memories. On the contrary, our hypothesis was that coherence would be equivalent. Therefore, if anything, the materials probably worked against our hypothesis rather than unwittingly feeding it.

Our findings extend the understanding of the relative coherence of traumatic and nontraumatic memories and have implications for several different literatures. First, they provide some evidence against the idea that people's memories for trauma are processed via a special mechanism different from that of memories for other, nontraumatic events (Brewin, 2016; van der Kolk & Fisler, 1995). Accordingly, our findings are inconsistent with theories that suggest that incoherent memories arise because of the way people

process and encode traumatic experiences or because traumatic experiences promote dissociation that inhibits memory encoding and integration (regarding processing and encoding, Brewin et al., 1996, 2010; Ehlers & Clark, 2000; Horowitz, 1976; van der Kolk & Fisler, 1995; regarding dissociation, Janet, 1889/1973; for a review, see Dalenberg et al., 2012).

Instead, our findings build on earlier work demonstrating that traumatic memories are not incoherent even among people with PTSD or otherwise reporting symptoms of posttraumatic stress (Rubin et al., 2016; Taylor et al., 2020). Taken together, this body of work raises important questions about the extent to which recovery from trauma is truly dependent on the resolution of incoherence (see also Bedard-Gilligan et al., 2017). Moreover, one must consider the possibility that encouraging victims of trauma to search for missing details in an attempt to resolve incoherence might actually be doing more harm than good. After all, in searching for lost memories, people can come to remember things that never happened at all—sometimes with disastrous consequences (Lindsay & Read, 1994; Loftus, 1993).

Second, our findings buttress the autobiographical memory literature by bringing experimental rigor to address long-standing counter-explanations. That is, the experimental design afforded comparisons of the coherence of traumatic and various nontraumatic events—comparable on important factors that could plausibly affect coherence—after the same delay. Even when accounting for these important factors, traumatic memories and nontraumatic memories were equivalently coherent.

If we and others have found no evidence of incoherence in traumatic memories, why do reports of incoherence persist? Here, we draw on two different literatures to provide possible explanations. First, there is evidence from the clinical literature that people who report a greater degree of dissociation believe their memories of analogue

traumas are less coherent than do people who report less dissociation. But the actual quality of these memories does not vary (Kindt & van den Hout, 2003; for a review, see Bedard-Gilligan & Zoellner, 2012). Perhaps, then, reports of incoherence are driven by a subjective sense of incoherence among victims of trauma who are otherwise prone to dissociation. Second, McNally (2021) raised the possibility that on college campuses, reports of incoherence in victims of sexual trauma are a direct result of the alcohol consumption that often accompanies or is adjacent to those traumatic experiences. More specifically, the rapid consumption of alcohol can interfere with both the encoding and consolidation of memories, which results in blackouts in which people have no memory for entire experiences, or aspects of experiences, they had while under the influence (Wetherill & Fromme, 2016; White, 2003). In these instances, then, traumatic memories are likely to be incoherent—not because of a special mechanism but because of the deleterious effects of alcohol on memory.

We did not collect data on subjects' racial/ethnic identification or their income, education, or socioeconomic status. We acknowledge that the absence of these data limits our ability to generalize the findings. What we do know, however, is how these characteristics are distributed in other samples from MTurk (Litman & Robinson, 2020; Moss et al., 2020). The race and ethnicity of MTurk workers is fairly representative of the population of the United States, but African Americans are somewhat underrepresented on MTurk (9.1% vs. 13.4%). Household income of MTurk workers is also fairly well matched to that of the U.S population, but MTurk workers are underrepresented at the top annual household income level of more than $150,000 (4.92% vs. 15.47%). Taken together, then, and assuming our sample is representative of MTurk workers, it seems likely that the demographic makeup of our sample is in step with that of the U.S. population. Nonetheless, we cannot verify this proposition and also would urge caution in

generalizing these findings to populations that are markedly different from those in the United States.

Although here we have addressed the question of coherence, our hope is that these materials assist scientists to tackle broader questions, too. For instance, the materials could be helpful in experiments requiring induction of positive or negative affect—and because the negative and positive films induced similar levels of negative and positive affect, respectively, they allow for experimental comparisons of events that induce affect to similar and opposite degrees. In addition, because the films provide a verifiable source of what happened during each event, they could be helpful in examining the accuracy of memories for traumatic and nontraumatic events. Finally, our unplanned—and therefore exploratory—analyses suggest a basis for speculating that these films produced minor differences between men and women on some measures (see the additional analyses reported on OSF; https://osf.io/ gkbja/). But on the crucial issue, which is whether the patterns of affect, storyness, and memory coherence are similar, the additional analyses give us no reason to conclude the films are fit for use with only men or only women. Nonetheless, it is our hope the wider scientific community will help bring clarity with planned analyses.

It bears emphasizing, however, that we are not suggesting the trauma-film paradigm should replace the valuable work of scientists who adopt other paradigms to study autobiographical memories or mental-health professionals making crucial clinical observations. Of course, the trauma-film paradigm has its own inherent limitations (for a review, see Marks et al., 2018). Most obviously, it is unsuitable for addressing the effects of any mechanism in which autobiography is paramount. Consider, for example, the autobiographical memory theory of PTSD (Rubin et al., 2011). This theory outlines factors that predict people's level of stress after a traumatic event: (a) the extent to which

they regulate their emotions at the time of memory recall; (b) individual tendencies toward intense emotions, rumination, and how important events are to people's life stories; and (c) three cognitive and affective mechanisms that operate on traumatic and nontraumatic memories alike—the emotional intensity of the memory for the event, the frequency with which the memory is rehearsed, and the extent to which the event becomes integrated into the life story. Obviously, the trauma-film paradigm is unsuitable for examining the importance or integration of a trauma to people's life stories (Berntsen et al., 2003; Berntsen & Rubin, 2007; Brewin, 2016; Broadbridge, 2018). But the paradigm is well suited for bringing an experimental approach to address factors relevant to the autobiographical memory theory of PTSD, such as emotion-regulation strategies, rumination, emotional intensity of the memory, and rehearsal. Our hope, then, is that the field will continue to see this suite of methodological approaches as complementary (Bahrick, 1991; Conway, 1991; Marks et al., 2018; Tulving, 1991). Ultimately, the reward is a better understanding of memory and dysfunction.

Author Contributions

All of the authors developed the study concepts and contributed to study design. A. Taylor performed data collection, analysis, and interpretation under the supervision of M. Garry and R. Zajac. M. Garry and R. Zajac wrote the funding proposal and are principal investigators; M. K. T. Takarangi is an associate investigator. A. Taylor and M. Garry drafted the manuscript, and R. Zajac and M. K. T. Takarangi provided critical revisions. All of the authors approved the final manuscript for submission.

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Open Practices

All data, coded and stripped of possible identifiable information, have been made publicly available via OSF and can be accessed at https://osf.io/uqtn6. The design and analysis plans for the experiments were preregistered at OSF and can be accessed at https://osf.io/7mjte. Because the materials contain repurposed commercial media, they are available at OSF in a password-protected folder, and access is available to qualified professionals on request. This article has received badges for Open Data and Preregistration. More information about the Open Practices badges can be found at https://www.psychologicalscience.org/publications/badges.

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Chapter 4: The Coherence of Analogue Traumatic and Non-Traumatic Memories Over Repeated Recall

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Abstract

In therapy, clients sometimes repeatedly recall their traumatic memories with a goal of resolving the incoherence said to underlie distress. But the existing literature does not tell us the extent to which people's memories for traumatic and non-traumatic memories cohere over the course of repeated recall, compared with similar memories not repeatedly recalled. We asked people to watch two films portraying traumatic or non-traumatic events, and then to repeatedly describe their memory for one of those films over five days. Data were consistent with the idea that repeatedly recalling a memory prevented a subjective loss of coherence that occurred for control memories, regardless of valence. There was little evidence of uniquely incoherent traumatic memories, nor a relation between the coherence of people's traumatic memories and their symptoms of posttraumatic stress disorder (PTSD). These findings have implications for models of PTSD, and the development and delivery of cognitive therapies for PTSD.

Keywords: trauma-film paradigm, memory coherence, traumatic memories, PTSD, equivalence testing, repeated recall, preregistered

The Coherence of Analogue Traumatic and Non-Traumatic Memories Over Repeated Recall

People's memories for their traumatic experiences are consistently portrayed as fragmented, jumbled, and recalled in bits and pieces. Put simply, compared to our memories for non-traumatic experiences, traumatic memories are thought to be "incoherent" as a result of the supposedly unique way these memories are encoded (e.g. Brewin et al., 1996; Brewin et al., 2010; Ehlers & Clark, 2000; Foa & Riggs, 1993; van der Kolk & Fisler, 1995; but see also Rubin et al., 2016). This incoherence is said to resolve, often over the course of therapy, as "dissociative amnesia" fades and people develop a coherent narrative of the experience (Brewin, 2016; Foa et al., 1995).

In fact, some therapeutic procedures explicitly target incoherent traumatic memories to help clients resolve incoherence (Ehlers et al., 2005; Foa & Rothbaum, 1998; Grand, 2013; Shapiro, 2009; van der Kolk & Fisler, 1995). For example, in one prevalent form of treatment for PTSD—prolonged exposure therapy (Foa & Rothbaum, 1998)—clients repeatedly recall the memory of their traumatic experience in a particular way to habituate to it. One of the justifications for this "imaginal exposure" is "processing and organizing the memory" (Foa et al., 2007, p. 64). Similarly, one of the main goals of trauma-focused cognitive therapy for PTSD is for the client to develop a "coherent narrative account" that "places the series of events during the trauma in context, in sequence, and in the past" (Ehlers & Clark, 2005, p. 416). To achieve this coherent account, clients might a) repeatedly recall the memory, b) write a detailed account of the memory, and/or c) revisit the site of the trauma.

Likewise, eye movement desensitization and reprocessing therapy (EMDR)—in which clients recall a traumatic memory while completing lateral eye movements—is said to help clients engage in "appropriate information processing," with therapists directed to

identify targets to "gain access to the dysfunctional memory networks" (F. Shapiro, 2018, p. 47). Therapists work to facilitate access to supposedly obstructed memories (F. Shapiro, 2018). In EMDR protocols for recent traumatic experiences, traumatic memories are assumed to be incoherent, stored "as a fragmented experience that has not yet consolidated" (E. Shapiro, 2009, p. 142). Clients recall aspects of the event in chronological order and clinicians work to process each aspect to "facilitate integration and consolidation" (E. Shapiro, 2009, p. 142).[13] Offshoots of EMDR, such as Brainspotting (Grand, 2013), inherit these same assumptions. Brainspotting is said to help with the "opening up" of painful memories (Grand, 2013, p. 113). Some clients are described as having "the veil of dissociation lifted in front of their eyes" as they supposedly begin to remember aspects of the trauma they believed they could not remember due to loss of consciousness (Grand, 2013, p. 117). Clearly, the idea that repeated recall turns incoherent traumatic memories into coherent traumatic memories has wide-reaching, and sometimes dangerous, implications.

But there is little in the way of good scientific evidence to support this idea that incoherence resolves over successful therapy. First, this idea stems from the assumption that encoding mechanisms operate in different ways on traumatic memories versus non-traumatic memories (e.g. Brewin et al., 1996; Ehlers & Clark, 2000). These different operations are said to produce incomplete encoding of traumatic experiences, and the subsequent incoherence in turn exacerbates posttraumatic stress with the disorganised fragments coming to mind involuntarily (Brewin et al., 1996; Foa & Riggs, 1993; van der Kolk & Fisler, 1995). Indeed, the notion that traumatic memories are special on a number of dimensions is one of several widely-held beliefs about traumatic memories that are

[13] Here, "consolidated" does not seem to refer to memory consolidation in the neuroscientific manner, but instead to the linking of aspects of the event into "an integrated whole"; a process F. Shapiro suggests, in the case of trauma, happens over two to three months (F. Shapiro, 2018, p. 222).

shared by the general public and practicing clinicians (Ost et al., 2017; Patihis et al., 2014; see also Hayne et al., 2006; Lynn et al., 2014; Mangiulli et al., 2021). The research, however, does not support this belief—a growing body of scientific evidence suggests traumatic memories are not only coherent (e.g. Halligan et al., 2003; Jones et al., 2007; Rubin et al., 2016; Taylor et al., 2020), but can be considered statistically equivalent on coherence to their non-traumatic counterparts (Taylor et al., 2021). These findings call the existence of a special mechanism into question.

Second, only a handful of published studies have attempted to measure changes in the coherence of adults' traumatic memories over the course of therapies targeting incoherence—most featuring small samples (*N*s = 14–77; Bedard-Gilligan et al., 2017; Foa et al., 1995; Kindt et al., 2007; Mundorf & Paivio, 2011; van Minnen et al., 2002). Across these studies, there was no consistent change in coherence from pre- to post-treatment; three found no significant change in coherence (Bedard-Gilligan et al., 2017; Kindt et al., 2007; Mundorf & Paivio, 2011), and the remaining two found patterns that differed depending on the measure (Foa et al., 1995; van Minnen et al., 2002). There were also no consistent findings regarding the relation between coherence and posttraumatic stress; sometimes increases in coherence were associated with reductions in symptoms of posttraumatic stress, other times the opposite, and other times still there was no relation. These studies support the idea that the benefits of therapy do not arise from an increase in the coherence of people's traumatic memories. Indeed, in the most thorough peer-reviewed investigation of the issue to date, there was no evidence to suggest people's traumatic memories were more coherent after 10 sessions of prolonged exposure therapy, and the associated repeated recall, than they were before—even among those who showed the greatest reduction in symptoms of PTSD (Bedard-Gilligan et al., 2017).

It is tempting to conclude these techniques do not give rise to increases in coherence, but such a conclusion is premature: any statement of causality requires a carefully-controlled experiment (for a similar point, see Nielsen et al., 2020; Taylor et al., 2021). In this case, the design of such an experiment must permit two crucial comparisons. First, we must determine what happens to memories that are recalled repeatedly compared with similar memories that are not, to account for the loss of information that happens over time (Ebbinghaus, 1885/1913; Meeter et al., 2005). This comparison does not exist in the literature. Second, we must determine the extent to which whatever effects we see for traumatic memories are unique to traumatic memories. Such a determination requires the comparison of traumatic memories to a battery of non-traumatic memories—controlling for emotional intensity, valence, and importance (Read & Lindsay, 2000; Rubin, Boals, & Berntsen, 2008; Taylor et al., 2020, 2021). Again, these comparisons do not exist in the literature. Given there is no good evidence for the idea that traumatic memories operate via some special mechanism (Rubin, Berntsen, & Bohni, 2008), it seems reasonable to speculate that whatever effects we see for traumatic memories we would also see for non-traumatic memories.

Taken together, the shortcomings of the literature mean we do not yet know the answer to these crucial questions: to what extent do people's traumatic memories cohere over the course of repeated recall, compared to memories that are not repeatedly recalled? And to what extent is the pattern of coherence of traumatic memories over repeated recall similar to that of various non-traumatic memories over repeated recall? The answers to these questions would have implications for how we are to understand the efficacy of techniques in widespread use for the treatment of PTSD. We conducted a carefully-designed experiment to answer these questions, with enough precision to detect statistical

equivalence in the coherence of traumatic versus non-traumatic memories over repeated recall.

To do so, we drew on the well-established trauma film paradigm (for reviews, see Holmes & Bourne, 2008; James et al., 2016). On the first day of the experiment, each subject watched two films portraying events of the same valence—either traumatic, negative, positive, or neutral. These films were previously normed on factors that might plausibly affect coherence (see Taylor et al., 2021). Two days later, we asked subjects to begin repeatedly recalling one of those events. Instructions for this repeated recall contained many of the key elements of those provided during prolonged exposure therapy. Repeated recall took place over the course of three sessions, spanning five days; subjects reported the coherence of their memories for those events at both the beginning and end of the experiment. We also asked subjects to report the analogue symptoms of posttraumatic stress they experienced about each event over the final two days of the experiment, to establish the extent to which any increased coherence of traumatic memories was associated with lower levels of analogue symptoms.

Method

We took an "equivalence testing" approach (Lakens, 2017) to assess support for two null hypotheses: (1) that people's memories for the traumatic films would be similarly coherent to those for the various non-traumatic films, and (2) that people's memories for the traumatic films would show a similar pattern of coherence over the course of repeated recall as those for the various non-traumatic films. In line with this approach, we pre-registered a minimum effect size we considered large enough to be meaningful (our "smallest effect size of interest"). A significant equivalence test supports the conclusion that the observed effect is too small to be meaningful, and observations can therefore be considered "equivalent." Our pre-registration, data, and additional

analyses are available on the project's ResearchBox page: https://researchbox.org/966&PEER_REVIEW_passcode=WLSFEK

Subjects

We recruited subjects on Amazon's Mechanical Turk (MTurk; https://www.mturk.com/) using the CloudResearch platform (Litman et al., 2017). We conducted a pilot study to determine our target sample size, and as a result set our target sample size at 544 useable responses (136 per valence of film) after exclusions. This sample size would allow us to detect equivalence with a smallest effect size of interest between subjects of $d = 0.4$ (a shift of approximately half a point on a seven-point Likert-type scale), with 90% power and $\alpha = .05$ (Lakens, 2017). In our prior work, we varied the way we asked people to recall traumatic and non-traumatic memories and found differences in coherence ranging from $d = 0.26$, 95% CI [0.12, 0.40] to $d = 0.53$, 95% CI [0.38, 0.68]. Therefore, it seems unlikely that a difference in the coherence of traumatic and non-traumatic memories smaller than $d = 0.4$ would be large enough to reflect an impaired ability to narrate traumatic memories (for a similar idea, see Rubin et al., 2016). As such, $d = 0.4$ seemed a reasonable smallest effect size of interest when managing resource constraints.

For the experiment proper, we collected data in batches. A total of 1076 MTurk workers completed the first session of the experiment. We progressively excluded subjects after each session of the experiment, ultimately retaining 542 for analyses (*M* age = 40.08 years, *SD* = 13.08, *Mdn* = 37; 34% men, 65% women, 1% gender diverse). Of these subjects, 93% reported English was their first language, and 93% reported English was their primary language. A complete breakdown of attrition and exclusions appears in the Supplemental Information (Appendix E). Subjects received a total of $3 for participating in all sessions of the study, with payment split across the sessions.

Design

We used a 4x2 mixed design. Valence of film (traumatic, negative, positive, neutral) was manipulated between-subjects, and whether a memory was repeatedly recalled (repeatedly-recalled, control) was manipulated within-subjects.

Procedure

The experiment was approved by the University of Otago Psychology Departmental Ethics Committee, with reciprocal notification to the University of Waikato. The experiment was conducted in accordance with the ethical provisions of the World Medical Association Declaration of Helsinki. Before subjects provided their consent, we told them:

> The videos you will be asked to watch might contain emotional content, such as a road traffic accident or stillbirth… It is possible that some people may experience distress or discomfort when watching these videos… If you are now uncomfortable or think that you might be adversely affected by viewing any of these videos, please do not participate in this study.

As Figure 1 shows, the experiment consisted of four main experimental sessions over seven days. Each session was administered online using Qualtrics. In addition to these four sessions, subjects were invited to complete optional check-in sessions on Days 4 and 6. These check-in sessions were designed to serve as experimental analogues to the homework clients do between sessions of prolonged exposure therapy (Foa & Rothbaum, 1998).[14]

Day 1

The first session of the experiment consisted of three phases.

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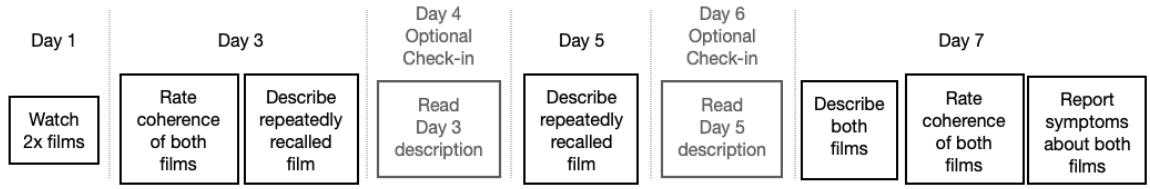
[14] We report the data about the effect of these check-in sessions in the Supplemental Information

Figure 1

Procedure



Phase 1. After providing their consent to participate in the experiment, we asked subjects to satisfy several environmental conditions while taking part in the study, such as completing the experiment in a quiet place with no distractions. Next, we verified that subjects' audio was working by asking them to listen to an audio clip directing them to "select the number nine below" from a variety of numbered options.

Phase 2. Once subjects had completed the audio check, they were randomly assigned to watch films that portrayed either traumatic, negative, positive, or neutral content. The negative films were included as counterparts to the traumatic films that also induce negative affect, but do not depict death, serious injury, or sexual violence (American Psychiatric Association, 2013). The positive films were included as counterparts to the traumatic films that are also emotionally intense, but induce positive affect instead of negative (Rubin, Boals, & Berntsen, 2008; Rubin et al., 2016). The neutral films were included as counterparts to the traumatic films that are neither emotionally intense nor valenced (Schaefer et al., 2010). We used an existing set of trauma film paradigm materials that induce positive and negative affect as expected, are equivalent on the extent to which they portray story-like events, and are unfamiliar to the majority of people (Taylor et al., 2021; https://osf.io/uqtn6/). Each film was approximately three minutes long (M = 2 min 56 s, range = 2 min 26 s to 3 min 15 s).

We counterbalanced the order in which subjects watched each of the two films of their assigned valence. Immediately after watching each film, subjects were shown a

screenshot from the film and asked to give the film "a short title (between 3 and 5 words)." We told subjects:

> Several days from now, we will use this video still plus your video title as a reminder for you, so that we can ask you questions about this particular video. So it is important this title is informative, and makes sense to you.

After providing that title, subjects then read a short filler passage of unrelated educational prose, before repeating the process for the other film (Butler et al., 2007).

Phase 3. At the end of this session, subjects completed a basic demographics questionnaire, and for exclusion purposes: a) answered questions about their compliance with instructions, and b) described the task they were asked to complete during the session. Finally, we reminded everyone we would contact them two days later with a link to the second session, which they should complete within 24 hours.

Day 3

Approximately 48 hours later (M = 50.14 hours, SD = 7.19, Mdn = 48.02), subjects completed the second session. This session consisted of three phases.

Phase 1. First, we reminded subjects about the two films they had watched on Day 1. In a counterbalanced order, we then showed them a still image of each film, accompanied by their nominated title for the film. Then, in a counterbalanced order, we returned to each combination of "still image and title" and asked subjects to rate the coherence of their memories for each film. We drew on existing literature, using four items to capture a broad range of aspects of coherence (Rubin et al., 2003, 2004; Taylor et al., 2021): (1) the story item ("as I remember the event, my memory comes to me as a coherent story"), (2) the pieces item ("as I remember the event, my memory comes to me in pieces, with bits missing"), (3) the fragments item ("as I remember the event, my memory comes to me as a collection of disconnected fragments"), and (4) the jumbled

item (“as I remember the event, my memory feels jumbled, with parts of the event coming to me out of order”). The items were presented in a random order and each was rated on a seven-point Likert-type scale from 1 (not at all) to 7 (completely). We created a coherence sum variable by reverse coding the pieces, fragments, and jumbled items, and calculating subjects’ mean rating of the four items (Taylor et al., 2021). These four items produced Cronbach’s αs ranging from .88–.89.

Phase 2. We then randomly selected one of each subject’s assigned films to be repeatedly recalled, and the other to serve as the “control” memory. Subjects saw the still image and title for the film they would subsequently repeatedly recall and read the following instructions adapted from guidelines for imaginal exposure sessions in prolonged exposure therapy (Foa et al., 2007):

> Please take your time and report everything that you can remember about the event portrayed in this video. Start your description at the very beginning of the event, and give a complete description of the entire event. We would like you to describe the event in the present tense, as if it were happening now, right here. It is important that you recall the memory of this event as vividly as possible, and picture the details of the event in your mind’s eye. Your description should contain as much detail as you can manage so that someone else who hasn’t watched the video would be able to imagine the event accurately.

Each sentence of the instruction appeared on a separate page, with a recap on the final page above a text box in which subjects were asked to write their description of their memory for that film.

Phase 3. Finally, subjects answered questions about their compliance with instructions and were reminded we would contact them two days later with a link to the

third session, which they should complete within 24 hours. We also reminded subjects we would email them the next day about the first optional check-in session.

Day 4 (Optional)

Approximately 24 hours later (*M* = 24.82 hours, *SD* = 6.00, *Mdn* = 25.23), 520 subjects completed the first optional "check-in" session. During this session, we piped back to each subject their Day 3 description of their repeatedly-recalled memory and asked them to read it "from beginning to end, without stopping," and to "visualise what you are reading… emotionally engage with any feelings you might have while reading the description." These instructions were adapted from those provided in practitioner guides regarding the homework clients do between sessions of prolonged exposure therapy (Foa et al., 2007). Finally, subjects answered questions about their compliance with instructions and described the task they were asked to complete during the check-in session.

Day 5

Approximately 48 hours after finishing the second session (*M* = 49.41 hours, *SD* = 5.63, *Mdn* = 48.10), subjects completed the third session. This session consisted of two phases.

Phase 1. First, subjects saw the still image and title of their repeatedly-recalled memory and again described their memory in a text box. To elicit these descriptions, we used the same instructions used on Day 3, with two small additions in line with guidelines for the second session of imaginal exposure in prolonged exposure therapy (Foa et al., 2007): "Please focus in detail on what you are seeing, hearing, and feeling as you recall the memory," and "please include what happened, and what you were feeling and thinking as you watched the video."

Phase 2. Finally, subjects answered questions about their compliance with instructions and were reminded we would contact them two days later with a link to the fourth and final session, which they should complete within 24 hours. We also reminded subjects we would email them the next day about the second optional check-in session.

Day 6 (Optional)

Approximately 24 hours later (M = 25.56 hours, SD = 6.63, Mdn = 24.06), 512 subjects completed the second optional "check-in" session. This check-in session was identical to the first, but for each subject we piped back their Day 5 description of their repeatedly-recalled memory, as opposed to Day 3.

Day 7

Approximately 48 hours after finishing the third session (M = 49.98 hours, SD = 6.85, Mdn = 48.37), subjects completed the fourth and final session. This session consisted of 6 phases.

Phase 1. First, we reminded subjects that seven days ago they had watched two different films. We then, in a counterbalanced order, showed them the stills and titles for each of those videos. In the subsequent phases of this session, subjects answered a number of questions about their memories for each of their assigned films. Subjects answered a given set of questions about each film in turn, before moving on to the next set of questions. We randomly assigned half of the subjects to answer any given set of questions about their repeatedly-recalled memory first, and the other half to answer about their control memory first. There was one exception to this counterbalanced rating order, which we explain in Phase 4 below.

Phase 2. Next, we asked subjects to provide a description of their memory for each of their assigned films. To elicit these descriptions, we asked people to "Please take your time and report everything that you can remember about this event, starting from the

beginning." We simplified the instructions in this session, compared to those in the previous sessions, to ensure subjects did not experience too much difficulty providing the description, which might in turn disrupt their subsequent coherence ratings (Taylor et al., 2020).

This description comprised subjects' first time recalling their control memory, but their third time recalling their repeatedly-recalled memory. We asked them to describe both memories in line with the possibility that providing the description might temporarily inflate their subsequent coherence ratings, providing a potential counter-explanation for our findings (Taylor et al., 2020). That is, if we had asked subjects to describe only their repeatedly-recalled memory in this session, they might have then rated that memory as more coherent than their control memory due to momentary attributions about coherence, rather than systematic changes in the actual coherence of the memory.

Phase 3. Subjects then completed a word search puzzle for five minutes, before rating the coherence of their memories for each of their two assigned films using the same four coherence items as on Day 3.

Phase 4. We then asked subjects to report the symptoms of posttraumatic stress they had experienced over the last two days about each film. To provide these symptom reports, subjects completed the revised Impact of Event Scale (IES-R; Weiss & Marmar, 1997), with instructions adapted for use a) with non-traumatic events (for example, "emotional life events" instead of "stressful life events", and b) over a 2-day timeframe rather than the typical 7 days. We asked all subjects to complete the adapted IES-R for both of their assigned films—even if these were non-traumatic counterpart films—to ensure the procedure was identical for everyone.

We fixed the order of these symptom reports: subjects reported the symptoms they had experienced about their repeatedly-recalled memory first, followed by their control

memory. We did this for two reasons. First, regardless of order, it would have been difficult to interpret the absolute levels of symptoms people experienced about each memory; the Day 3 and 7 descriptions, and check-in sessions, would have served as reminders of the repeatedly-recalled memory, increasing accessibility and potentially inflating their symptom reports for that memory. Second, we collected pilot data that suggested subjects' reports of symptoms about the first memory they rate anchors their reports about a subsequent memory. Because we were primarily interested in subjects' symptoms about their repeatedly-recalled memory, we fixed the order so that subjects always rated those symptoms first.

Phase 5. Next, to examine the extent to which subjects believed their memories had become more or less coherent over the course of the experiment, we asked them to complete the statement: "Over the course of this 7-day study, my memory for this event has become... (1 = more coherent, 7 = less coherent)." Subjects then rated each memory on three phenomenological aspects: reliving, emotions, and rehearsal. To measure these aspects, we asked subjects to rate each memory on eight items from the Autobiographical Memory Questionnaire (AMQ; Rubin et al., 2003, 2004). We report the AMQ data in the Supplemental Information.

Phase 6. Finally, subjects answered questions about their compliance with instructions and told us what they thought the study was about, before being debriefed.

Results and Discussion

Of the 620 subjects who completed all four main experimental sessions, we excluded 75 subjects (12%) based on pre-registered exclusion criteria. Of these 75, 56 subjects failed two or more attention checks, 16 reported they completed at least one of the sessions of the study in more than one sitting, and three provided a description of their control memory on Day 7 that either did not make sense or suggested they could not

remember the film. In addition to these pre-registered criteria, we excluded one subject who described the same memory twice on Day 7, one who did not describe their repeatedly-recalled memory on Day 5, and one who did not describe their repeatedly-recalled memory on Day 7. We did not pre-register these additional criteria because we did not anticipate the scenarios would happen. In total, then, we excluded the data from 78 subjects (13%), retaining 542 for analyses ($n_{traumatic}$ = 136, $n_{negative}$ = 136, $n_{positive}$ = 139, $n_{neutral}$ = 131).

Coherence Over Repeated Recall

We now turn to our primary research questions: to what extent do people's traumatic memories cohere over the course of repeated recall, compared to memories that are not repeatedly recalled? And to what extent is the pattern of coherence of traumatic memories over repeated recall similar to that of various non-traumatic memories over repeated recall? We addressed these questions using two approaches.

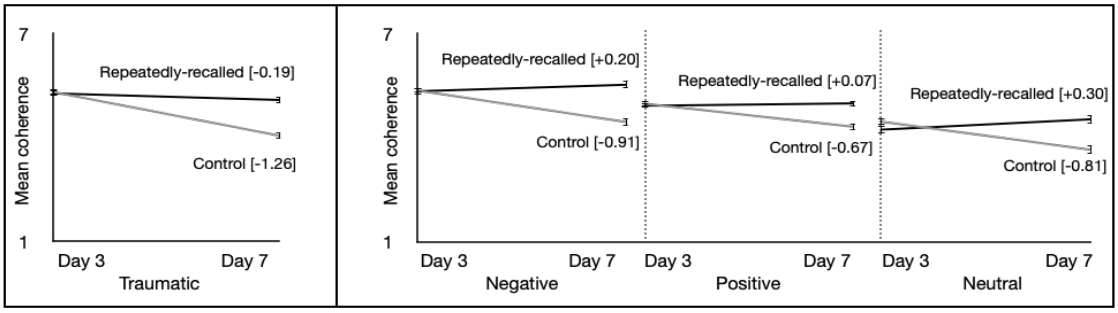
Reported Coherence

Day 3 and Day 7 Ratings of Coherence. Our first approach addressed these questions with subjects' own ratings of coherence. We created a coherence sum variable using subjects' responses to the four coherence items on Day 3; we did the same for Day 7. We then classified these ratings according to whether the ratings were about a control or repeatedly-recalled memory, and again by the film's valence. We display these data in Figure 2.

The figure shows at least four interesting patterns. First, focusing on the traumatic memories, we see that people rated their control and repeatedly-recalled memories as similarly coherent on Day 3. When we compare the traumatic memories to their non-traumatic counterparts, we see this pattern is consistent across the different types of

Figure 2

Mean Coherence Ratings of Repeatedly-Recalled and Control Traumatic and Non-Traumatic Memories



Note. Day 3 ratings of coherence were made prior to first session of recall. Error bars represent 95% Cousineau-Morey confidence intervals.

memories. Put another way, there was no evidence of a significant interaction between valence of film (traumatic, negative, positive, neutral) and whether that memory was subsequently repeatedly recalled or served as a control on Day 3 coherence ratings, F (3, 538) = 0.82, p = 0.482. As expected, there was no evidence of a significant effect of whether a memory was subsequently repeatedly recalled or served as a control, F (1, 540) = 2.68, p = .102.

Second, if we look again at the traumatic memories, we see that on Day 7 people rated their repeatedly-recalled memories as more coherent than their control memories. In addition, control memories became less coherent while repeatedly-recalled memories remained largely unchanged. That is, repeatedly recalling a traumatic memory arrested the predictable loss of information over time. This loss of information occurred across the board for the control memories. Put another way, there was no evidence of a significant interaction between valence of film type (traumatic, negative, positive, neutral) and whether a memory was repeatedly recalled or served as a control on Day 7 coherence ratings, F (3, 538) = 1.55, p = 0.200. There was an effect of repeated recall, such that

people rated their control memory as less coherent on Day 7 than their repeatedly-recalled memory, M_{diff} = -0.92, 95% CI [-1.06, -0.77], F (1, 540) = 156.13, p < .001.

Third, in absolute terms, the figure shows that traumatic memories—even control memories—were relatively coherent, as were their non-traumatic counterparts. When we conducted equivalence tests on people's Day 3 coherence ratings (collapsing across the repeatedly-recalled and control memories) we found the traumatic and negative memories were equivalent on coherence (M_{diff} = 0.04, ps <.01), but the traumatic memories were more coherent than both neutral (M_{diff} = 0.98, $p_{\text{lower bound}}$ <.001, $p_{\text{upper bound}}$ = .793) and positive films (M_{diff} = 0.35, $p_{\text{lower bound}}$ <.001, $p_{\text{upper bound}}$ = .168). On Day 7, if we focus first on the repeatedly-recalled memories, the traumatic memories were less coherent than negative memories (M_{diff} = -0.44, $p_{\text{lower bound}}$ =0.361, $p_{\text{upper bound}}$ < .001), equivalent to the positive memories (M_{diff} = 0.10, $p_{\text{lower bound}}$ < .001, $p_{\text{upper bound}}$ < .01), and more coherent than the neutral memories (M_{diff} = 0.58, $p_{\text{lower bound}}$ < .001, $p_{\text{upper bound}}$ = .690. This pattern was similar for the control memories, but here the traumatic memories were less coherent than the positive memories (negative: M_{diff} = -0.39, $p_{\text{lower bound}}$ = 0.281, $p_{\text{upper bound}}$ < .001; positive: M_{diff} = -0.25, $p_{\text{lower bound}}$ = 0.097, $p_{\text{upper bound}}$ < .001; neutral: M_{diff} = 0.44, $p_{\text{lower bound}}$ < .001, $p_{\text{upper bound}}$ = 0.37).

We suspect the lower coherence of the neutral memories reflects a difference in how engaging the neutral films were compared to the other films, and the extent to which people thought about the films between watching them on Day 1 and answering the coherence questions on Day 3. Though we do not have the data to test this possibility here, it is consistent with data from Taylor et al. (2021) in which people who watched a neutral film reported they would be less likely to think about that film over the next few days than did people who watched a traumatic, negative, or positive film (https://osf.io/wu5ek/). Taken together, these patterns of results do not support the idea

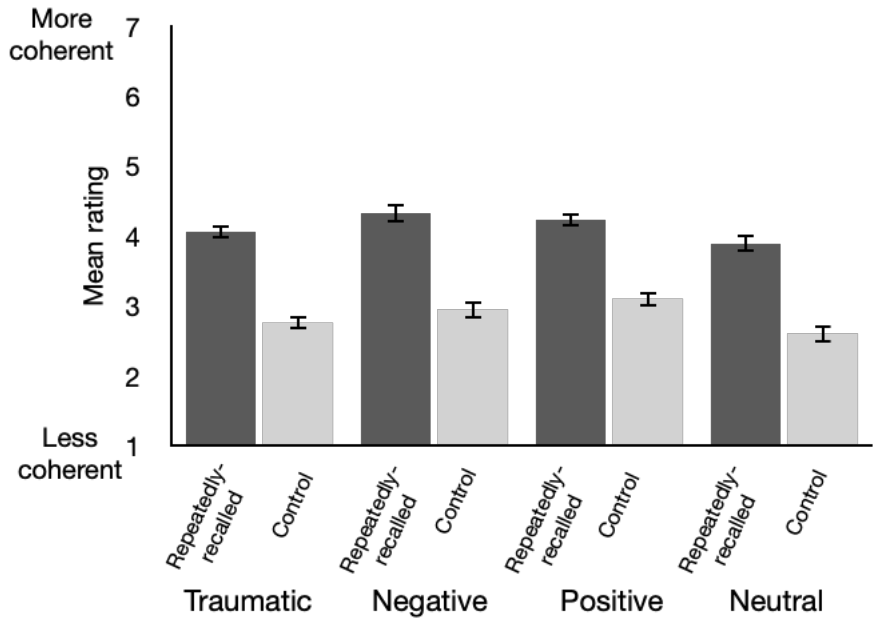
that processes involved in encoding traumatic memories result in incoherence (Brewin et al., 1996; Foa & Riggs, 1993; van der Kolk & Fisler, 1995).

Finally, when we look at the difference between Day 3 and Day 7 coherence ratings, we see that the traumatic memories lost more coherence than did negative, positive, and neutral memories (respectively, M_{diff} = -0.37, 95% CI [-0.75, 0.01], M_{diff} = -0.43, 95% CI [-0.81, -0.05], & M_{diff} = -0.47, 95% CI [-0.85, -0.09], F (3, 538) = 208.60, p < .001). It is hard to interpret this finding as consistent with the idea that traumatic memories are incoherent given they are still more coherent on Day 7 than their neutral counterparts, and statistically equivalent to their positive counterparts.

Reports of Memories Becoming More or Less Coherent. Recall on Day 7 we also asked people to rate the extent to which their memory became more, or less, coherent over the course of the experiment. To what extent, then, did people think their traumatic memories lost coherence? Figure 3 displays their responses. As the figure shows, ratings of the repeatedly-recalled memories hovered around the midpoint of the scale, suggesting people thought the coherence of these memories remained stable. But ratings of the control memories suggested people thought these memories lost coherence; a pattern consistent with the actual changes in coherence people reported. Put another way, there was no evidence of a significant interaction between the valence of the film (traumatic, negative, positive, neutral) and whether that memory was repeatedly recalled or served as a control, F (3, 538) = 0.42, p = 0.736. People reported their control memories lost more coherence than did their repeatedly-recalled memories, M_{diff} = -1.14, 95% CI [-1.30, -0.99], F (1, 540) = 200.43, p < .001. The neutral memories lost more coherence than the negative and positive memories, but not the traumatic memories (respectively, M_{diff} = -0.42, 95% CI [-0.84, 0.01], M_{diff} = -0.44, 95% CI [-0.85, 0.03], & M_{diff} = -0.21, 95% CI [-0.62, 0.21], F (3, 538) = 3.56, p < .05).

Figure 3

Mean Ratings of the Extent to Which Memories Became More or Less Coherent



Note. Ratings reverse-coded for ease of interpretation. Error bars represent 95% Cousineau-Morey Confidence Intervals

Taken together, these findings suggest repeatedly recalling a memory arrested a loss of coherence that occurred when a memory was not repeatedly recalled. A critic might say this finding is not surprising given we have known for over a century that rehearsal dampens the loss of information (Ebbinghaus, 1913; Meeter et al., 2005). But the purpose of this study was not to determine the effect of repeatedly recalling a memory in general; instead we set out to examine the claim that traumatic memories operate in fundamentally different ways than non-traumatic counterparts. And in fact, regardless of whether the memories were traumatic or non-traumatic, people rated their repeatedly-recalled memories as more coherent on Day 7 than they did their control memories. Furthermore, people's ratings of whether their memories became more or less coherent from Day 3 to Day 7 were fairly consistent with the changes in coherence they actually reported. More specifically, people reported similarly coherent repeatedly-recalled

memories on Days 3 and 7, but less coherent control memories on Day 7 than Day 3. Although people's ratings suggested the traumatic memories lost more coherence over time than the non-traumatic counterparts, people did not have the sense that these memories became any less coherent than did the non-traumatic memories.

Computer-Scored Measures of Coherence

For our second approach to addressing our primary research questions, we gathered computer-scored measures of coherence by analyzing subjects' descriptions of their memories (Rubin et al., 2016). Recall that subjects provided three descriptions of their repeatedly-recalled memory—one each on Days 3, 5, and 7—and one description of their control memory on Day 7. To determine the coherence of these memory descriptions, we then analyzed them using Linguistic Inquiry Word Count (LIWC; Pennebaker et al., 2015) and Coh-Metrix (Graesser et al., 2004). These analyses provided data on 18 computer-scored measures of memory coherence; five from LIWC and 13 from Coh-Metrix. With the exception of two LIWC measures (nonfluencies and filler words), higher scores suggest a more coherent memory (for information about each measure, see the Supplemental Information on ResearchBox). The analyses resulted in 72 datapoints for each subject (18 computer-scored measures for each of their four descriptions). We then classified the data according to both the valence of the memory and description it was gathered from, and calculated mean coherence for each of the 18 measures (see Table 1, Table 3, & Table S2).

Day 3 Coherence. To what extent could the Day 3 traumatic memories be considered equivalent to their non-traumatic counterparts with respect to coherence? To address this question, we conducted a series of equivalence tests on each computer-scored measure of coherence gathered from the Day 3 descriptions, with a smallest effect size of interest of $d = 0.4$. We display these data in Table 1. As the table shows, there was no

Table 1

Computer-Scored Measures of Coherence Gathered From Subjects' Day 3 Descriptions of Their Repeatedly-Recalled Memories

	Traumatic	Negative			Positive			Neutral		
Measure	***M* [95% CI]**	***M* [95% CI]**	***M*$_{diff}$ [95% CI]**	***p*$_{max}$**	***M* [95% CI]**	***M*$_{diff}$ [95% CI]**	***p*$_{max}$**	***M* [95% CI]**	***M*$_{diff}$ [95% CI]**	***p*$_{max}$**
Cognitive processes	6.19 [5.71, 6.66]	7.88 [7.42, 8.35]	-1.70 [-2.70, -0.69]	.962	6.36 [5.78, 6.93]	-0.17 [-1.17, 0.83]	<.01*	8.37 [7.71, 9.03]	-2.18 [-3.20, 1.17]	.984
Insight	1.27 [1.06, 1.48]	2.45 [2.19, 2.72]	-1.19 [-1.62, -0.75]	1	1.21 [0.94, 1.49]	0.06 [-0.38, 0.49]	<.01*	0.63 [0.45, 0.82]	0.64 [0.19, 1.08]	.831
Causation	0.79 [0.65, 0.92]	1.19 [0.99, 1.38]	-0.40 [-0.89, 0.09]	.891	0.38 [0.27, 0.48]	0.41 [-0.07, 0.90]	.899	4.00 [3.53, 4.48]	-3.22 [-3.71, -2.72]	1
Nonfluencies	0.11 [0.05, 0.18]	0.10 [0.05, 0.15]	0.02 [-0.11, 0.14]	<.001*	0.14 [0.03, 0.25]	-0.03 [-0.16, 0.10]	<.01*	0.01 [0.00, 0.03]	0.10 [-0.03, 0.28]	.481
Fillers	0.01 [0,00, 0.03]	0.01 [0.00, 0.02]	0.00 [-0.02, 0.02]	<.01*	0	0.01 [-0.01, 0.03]	N/A	0	0.01 [-0.01, 0.03]	N/A
Narrativity	0.65 [0.53, 0.76]	1.16 [1.04, 1.27]	-0.51 [-0.76, -0.26]	.984	0.37 [0.23, 0.50]	0.28 [0.03, 0.53]	.417	0.17 [0.00, 0.35]	0.47 [0.22, 0.72]	.891
Syntactic ease	-0.31 [-0.48, -0.16]	-0.76 [-0.91, -0.61]	0.44 [0.14, 0.74]	.640	-0.15 [-0.31, 0.02]	-0.17 [-0.47, 0.13]	.027*	-0.09 [-0.27, 0.10]	-0.23 [-0.54, 0.07]	.081
Word concreteness	2.50 [2.32, 2.68]	1.92 [1.80, 2.04]	0.58 [0.25, 0.91]	.916	1.71 [1.53, 1.89]	0.79 [0.47, 1.12]	.999	1.54 [1.31, 1.76]	0.96 [0.63, 1.29]	1
Referential cohesion	1.19 [0.99, 1.40]	0.78 [0.61, 0.94]	0.42 [0.06, 0.78]	.410	0.14 [-0.03, 0.30]	1.06 [0.70, 1.68]	1	1.45 [1.21, 1.70]	-0.26 [-0.62, 0.11]	.043*
Deep cohesion	-0.43 [-0.60, -0.26]	0.04 [-0.14, 0.22]	-0.47 [-0.86, -0.08]	.551	0.50 [0.25, 0.74]	-0.93 [-1.32, -0.54]	.998	0.14 [-0.11, 0.38]	-0.57 [-0.96, -0.17]	.671
Content word overlap	0.14 [0.13, 0.16]	0.15 [0.14, 0.16]	-0.01 [-0.03, 0.02]	.037*	0.12 [0.11, 0.13]	0.02 [0.00, 0.05]	.454	0.17 [0.16, 0.19]	-0.03 [-0.05, 0.00]	0.110
Latent semantic analysis	0.27 [0.26, 0.29]	0.20 [0.19, 0.22]	0.05 [0.03, 0.08]	.954	0.19 [0.18, 0.21]	0.04 [0.01, 0.07]	.740	0.31 [0.28, 0.33]	-0.02 [-0.05, -0.01]	.002*
Temporal connectives	22.73 [20.05, 25.41]	18.81 [16.69, 20.93]	3.92 [-1.13, 8.97]	.144	25.15 [22.09, 28.20]	-2.42 [-7.44, 2.61]	.010*	25.06 [22.04, 28.09]	-2.34 [-7.43, 2.76]	.010*
Extended temporal connectives	6.30 [4.95, 7.65]	14.80 [12.78, 16.80]	-8.49 [-12.42, -4.56]	.998	17.20 [14.52, 19.87]	-10.89 [-14.81, -6.98]	1	5.13 [2.86, 7.41]	1.17 [-2.80, 5.14]	.016*
Causal connectives	12.47 [10.79, 14.14]	20.90 [18.68, 23.12]	-8.44 [-13.63, -3.24]	.956	28.16 [24.40, 31.93]	-15.70 [-20.86, -10.53]	1	18.98 [15.85, 22.11]	-6.52 [-11.76, -1.27]	.6
Adversative connectives	9.63 [7.91, 11.35]	6.31 [5.00, 7.61]	3.32 [0.57, 6.07]	.433	4.11 [2.85, 5.37]	5.52 [2.78, 8.25]	.971	6.49 [4.84, 8.14]	3.14 [0.36, 5.91]	.004*
Logical connectives	25.03 [22.31, 27.75]	31.40 [28.72, 34.08]	-6.37 [-9.26, 1.68]	.384	35.19 [32.02, 38.37]	-10.16 [-15.63, -4.69]	.932	32.18 [28.83, 35.52]	-7.14 [-12.70, -1.59]	.527
Additive connectives	50.33 [46.78, 53.88]	48.53 [45.45, 51.62]	1.80 [-5.15, 8.75]	.011	50.28 [46.58, 53.97]	0.06 [-6.86, 6.97]	<.001*	50.06 [45.37, 54.75]	0.28 [-6.74, 7.29]	<.001*

Note. M_{diff} = traumatic – comparison memory. p_{max} represents highest p-value in equivalence test comparing traumatic and comparison memory. We did not correct for multiple comparisons because these tests were pre-registered, but applying a correction does not affect the pattern of results in any specific way: fewer comparisons can be considered equivalent, but the pattern of coherence remains mixed with traumatic memories sometimes more and other times less coherent than the non-traumatic counterparts.

* significant equivalence test; traumatic and comparison memory can be considered equivalent on the computer-scored measure of coherence

particular pattern of coherence on Day 3. If we first compare the traumatic and negative memories, we see these memories were equivalent on coherence for four of the 18 measures. Of the 14 that were not equivalent, traumatic memories were more coherent on six, and less coherent on eight. If we then compare the traumatic and positive memories, we see these memories were equivalent on coherence for six of the 18 measures. Of the 12 that were not equivalent, traumatic memories were more coherent on seven, and less coherent on five. Finally, if we compare the traumatic and neutral memories, we see these memories were equivalent on coherence for six of the 18 measures. Of the 12 that were not equivalent, the traumatic memories were more coherent on three, and less coherent on nine. Taken together, these data suggest no particular pattern: compared to their non-traumatic counterparts, traumatic memories were more coherent, less coherent, or similar on coherence, depending on the measure.

Change in Coherence. We then calculated the change in each computer-scored measure of coherence by taking the difference between the scores gathered from the Day 7 and Day 3 descriptions of the repeatedly-recalled memories (see Table 2). Positive change scores suggest a description was more coherent on Day 7 than Day 3, and negative change scores suggest the opposite (again with the exception of the nonfluencies and filler words measures from LIWC). As the table shows, for most measures, the magnitude of change in coherence was not statistically different from zero. More specifically, traumatic memories were essentially unchanged on all 18 measures. Negative memories became more coherent on one and were essentially unchanged on the remaining 17. The positive memories became more coherent on one, less coherent on one, and were essentially unchanged on the remaining 16. And the neutral memories became more coherent on one and were essentially unchanged on the remaining 17.

Table 2

Change in Computer-Scored Measures of Coherence in Subjects' Repeatedly-Recalled Memories From Day 3 to Day 7

	Traumatic		Negative				Positive				Neutral			
Measure	***M* [95% CI]**	***p***	***M* [95% CI]**	***p***	M_{diff} **[95% CI]**	p_{max}	***M* [95% CI]**	***p***	M_{diff} **[95% CI]**	p_{max}	***M* [95% CI]**	***p***	M_{diff} **[95% CI]**	p_{max}
Cognitive processes	0.08 [-0.53, 0.69]	.794	-0.46 [-0.96, 0.03]	.066	0.54 [-0.71, 1.79]	<.05*	-0.85 [-1.49, -0.22]	.009†	0.93 [-0.31, 2.17]	.147	0.35 [-0.57, 1.28]	.451	-0.27 [-1.54, 0.99]	<.01*
Insight	0.01 [-0.22, 0.24]	.915	0.04 [-0.21, 0.29]	.731	-0.03 [-0.57, 0.51]	<.001*	-0.10 [-0.42, 0.23]	.561	0.11 [-0.43, 0.65]	<.01*	0.55 [0.19, 0.92]	.003†	-0.54 [-1.09, 0.01]	.233
Causation	-0.05 [-0.20, 0.10]	.521	0.02 [-0.16, 0.21]	.803	-0.07 [-0.69, 0.54]	<.01*	0.04 [-0.10, 0.18]	.539	-0.09 [-0.70, 0.52]	<.01*	0.24 [-0.39, 0.87]	.449	-0.29 [-0.91, 0.33]	<.01*
Nonfluencies	-0.03 [-0.11, 0.06]	.534	0.01 [-0.05, 0.07]	.665	-0.04 [-0.22, 0.14]	<.05*	0.06 [-0.10, 0.21]	.477	-0.08 [-0.26, 0.09]	<.01*	0.01 [-0.03, 0.04]	.686	-0.03 [-0.21, 0.14]	<.01*
Fillers	-0.01 [-0.03, 0.01]	.211	0.00 [-0.01, 0.00]	.171	-0.01 [-0.03, 0.02]	<.01*	0	N/A	-0.01 [-0.04, 0.02]	N/A	0.01 [-0.01, 0.04]	.319	-0.02 [-0.05, 0.01]	<.05*
Narrativity	0.02 [-0.08, 0.11]	.608	0.03 [-0.08, 0.14]	.602	-0.01 [-0.26, 0.25]	.008*	-0.11 [-0.27, 0.05]	.164	0.13 [-0.12, 0.39]	.047*	0.02 [-0.17, 0.20]	.867	0.01 [-0.25, 0.27]	<.001*
Syntactic ease	0.15 [-0.02, 0.32]	.085	0.13 [0.00, 0.26]	.042†	0.02 [-0.31, 0.35]	.002*	0.06 [-0.13, 0.26]	.509	0.09 [-0.24, 0.41]	.007*	0.09 [-0.13, 0.30]	.418	0.06 [-0.27, 0.39]	.004*
Word concreteness	-0.02 [-0.17, 0.13]	.767	-0.01 [-0.16, 0.15]	.945	-0.02 [-0.38, 0.35]	.01*	0.11 [-0.13, 0.34]	.365	-0.13 [-0.50, 0.24]	.012*	0.19 [-0.05, 0.44]	.114	-0.22 [-0.59, 0.15]	.054
Referential cohesion	-0.11 [-0.29, 0.08]	.27	-0.02 [-0.17, 0.14]	.803	-0.09 [-0.47, 0.30]	.017*	0.07 [-0.15, 0.30]	.516	-0.18 [-0.56, 0.20]	.015*	-0.10 [-0.36, 0.15]	.423	0.00 [-0.39, 0.38]	<.001*
Deep cohesion	0.15 [-0.01, 0.31]	.066	-0.03 [-0.24, 0.19]	.801	0.18 [-0.28, 0.64]	.064	0.03 [-0.26, 0.31]	.857	0.13 [-0.33, 0.58]	.008*	0.11 [-0.20, 0.41]	.487	0.04 [-0.42, 0.51]	.002*
Content word overlap	0.00 [-0.02, 0.01]	.456	0.00 [-0.01, 0.01]	.973	0.00 [-0.03, 0.02]	.035*	0.00 [-0.01, 0.02]	.884	-0.01 [-0.03, 0.02]	.015*	0.00 [-0.02, 0.02]	.977	-0.01 [-0.03, 0.02]	<.001*
Latent semantic analysis	-0.01 [-0.03, 0.00]	.166	-0.01 [-0.03, 0.00]	.164	0.00 [-0.04, 0.04]	.01*	0.00 [-0.02. 0.02]	.808	-0.01 [-0.05, 0.03]	.017*	-0.03 [-0.06, 0.00]	.057	0.02 [-0.06, 0.02]	.005*
Temporal connectives	1.95 [-0.78, 4.69]	.161	-0.21 [-2.48, 2.07]	.857	2.16 [-3.42, 7.75]	.002*	1.80 [-1.36, 4.96]	.263	0.16 [-5.40, 5.71]	<.001*	1.05 [-2.77, 4.87]	.587	0.90 [-4.74, 6.54]	<.001*
Extended temporal connectives	0.33 [-1.36, 2.01]	.701	0.22 [-2.20, 2.64]	.86	0.11 [-4.13, 4.36]	.002*	-1.69 [-4.83, 1.46]	.29	2.01 [-2.21, 6.24]	.008*	-1.44 [-2.95, 0.07]	.061	1.77 [-2.51, 6.06]	.09
Causal connectives	0.35 [-1.67, 2.36]	.735	0.27 [-2.55, 3.09]	.852	.08 [-5.69, 5.85]	.004*	-1.44 [-5.35, 2.47]	.468	1.78 [-3.96, 7.53]	.01*	2.01 [-1.44, 5.47]	.252	-1.67 [-7.49, 4.16]	.009*
Adversative connectives	-0.52 [-2.40, 1.37]	.59	-0.23 [-1.69, 1.22]	.749	-0.28 [-3.56, 3.00]	.002*	0.91 [-0.84, 2.66]	.307	-1.42 [-4.69, 1.84]	.008*	-1.56 [-3.57, 0.45]	.127	1.05 [-2.26, 4.36]	.004*
Logical connectives	2.50 [-0.36, 5.36]	.086	-0.93 [-4.33, 2.47]	.589	3.43 [-3.21, 10.07]	.057*	3.39 [-0.54, 7.33]	.09	-0.89 [-7.50, 5.71]	.003*	-0.14 [-4.27, 3.98]	.945	2.64 [-4.06, 9.34]	.02*
Additive connectives	0.58 [-3.26, 4.41]	.766	2.44 [-0.80, 5.68]	.139	-1.86 [-9.59, 5.88]	.021*	5.13 [0.45, 9.82]	.032	-4.55 [-12.25, 3.14]	.034*	2.68 [-2.20, 7.56]	.289	-2.10 [-9.91, 5.70]	.005*

Note. M_{diff} = traumatic – comparison memory. *p* represents p-value from t-test comparing change in computer-scored measure to zero. p_{max} represents highest p-value in equivalence test comparing traumatic and comparison memory. We did not correct for multiple comparisons because these equivalence tests were pre-registered, but applying a correction does not affect the pattern of results in any specific way: fewer comparisons can be considered equivalent, but the pattern of coherence remains mixed with traumatic memories sometimes gaining more, gaining less, losing more or losing less coherence than the non-traumatic counterparts.

† represents change significantly different from zero. * represents significant equivalence test; change of traumatic and comparison memory can be considered equivalent on the computer-scored measure of coherence

To what extent could the change in coherence of the traumatic memories be considered equivalent to that of the non-traumatic memories? To address this question, we conducted a series of equivalence tests on the change in each computer-scored measure of coherence, with a smallest effect size of interest of $d = 0.4$. We display these data in Table 2. As the table shows, for the majority of measures, the change in coherence of traumatic memories was equivalent to that of the non-traumatic counterparts. If we first compare the traumatic and negative memories, we see the change in these memories was equivalent on 16 of the 18 measures. Of the two that were not equivalent, the traumatic memories gained more coherence than the negative counterparts on both. If we then compare the traumatic and positive memories, we see the change in memories was equivalent on 16 of the 18 measures. Of the two that were not equivalent, the traumatic memories gained more coherence than the positive counterparts on both. Finally, if we compare the traumatic and neutral memories, we see the change in these memories was equivalent on 15 of the 18 measures. Of the three measures that were not equivalent, the traumatic memories gained more coherence than the neutral counterparts on one, gained less coherence on one, and lost more coherence on one. Taken together, these data suggest traumatic and non-traumatic memories show a similar change in computer-scored measures of coherence over the course of repeated recall.

Day 7 Coherence. How did repeated recall affect the coherence of traumatic and non-traumatic memories? To address this issue, for each computer-scored measure of coherence we calculated the mean difference between people's repeatedly-recalled and control memories on Day 7. We classified these data according to the valence of the memory and report them in Table 3. As the table shows, for the majority of measures there was no evidence of a significant difference in the coherence of the repeatedly-recalled and control memories on Day 7. If we look first at the traumatic memories, we

Table 3

Computer-Scored Measures of Coherence Gathered From Subjects' Day 7 Descriptions of Their Repeatedly-Recalled and Control Memories

	Traumatic		Negative		Positive		Neutral	
Measure	M_{diff} **[95% CI]**	***p***	M_{diff} **[95% CI]**	***p***	M_{diff} **[95% CI]**	***p***	M_{diff} **[95% CI]**	***p***
Cognitive processes	-0.44 [-1.41, 0.53]	.234	-1.11 [-2.08, -0.14]	.003†	-1.64 [-2.68, -0.59]	<.001†	-2.16 [-3.41, -0.91]	<.001†
Insight	-0.39 [-0.91, 0.13]	.050†	0.01 [-0.51, 0.53]	.953	-0.31 [-0.86, 0.23]	.151	-0.61 [-1.17, -0.05]	.008†
Causation	0.01 [-0.39, 0.41]	.916	-0.15 [-0.55, 0.24]	.403	-0.06 [-0.32, 0.19]	.44	-0.43 [-1.15, 0.29]	.251
Nonfluencies	0.01 [-0.1, 0.1]	.985	0.08 [-0.03, 0.18]	.022†	0.05 [-0.11, 0.22]	.405	-0.02 [-0.12, 0.07]	.492
Fillers	-0.01 [-0.03, 0.02]	.319	-0.01 [-0,03, 0.02]	.35	0 [-0.02, 0.02]	N/A	0.01 [-0.02, 0.04]	.319
Narrativity	0.06 [-0.11, 0.24]	.477	-0.05 [-0.21, 0.10]	.478	-0.22 [-0.43, 0.01]	.037†	-0.08 [-0.32, 0.15]	.493
Syntactic ease	-0.04 [-0.22, 0.14]	.685	0.17 [0.02, 0.32]	.027†	0.26 [0.06, 0.45]	.010†	0.01 [-0.23, 0.24]	.951
Word concreteness	0.26 [0.00, 0.52]	.048†	0.02 [-0.20, 0.25]	.83	0.14 [-0.16, 0.44]	.361	0.29 [-0.03, 0.62]	.079
Referential cohesion	0.38 [0.07, 0.70]	.016†	-0.16 [-0.37, 0.05]	.137	-0.23 [-0.50, 0.05]	.107	0.20 [-0.12, 0.52]	.220
Deep cohesion	0.17 [-0.10, 0.43]	.217	0.05 [-0.20, 0.31]	.677	-0.15 [-0.60, 0.31]	.524	-0.10 [-0.54, 0.34]	.654
Content word overlap	-0.02 [-0.04, 0.00]	.044†	0.00 [-0.01, 0.01]	.875	0.01 [-0.01, 0.02]	.590	-0.03 [-0.05, 0.01]	.009†
Latent semantic analysis	0.01 [-0.01, 0.04]	.212	-0.01 [-0.03, 0.01]	.356	-0.01 [-0.03, 0.02]	.559	0.04 [0.01. 0.08]	.022†
Temporal connectives	2.97 [-0.51, 6.45]	.093†	2.13 [-0.56, 4.82]	.119	0.55 [-3.55, 4.64]	.792	1.03 [-4.17, 6.22]	.697
Extended temporal connectives	-1.50 [-3.52, 0.52]	.144	-2.79 [-6.12, 0.54]	.100	2.81 [-1.68, 7.29]	.218	1.18 [-1.08, 3.44]	.303
Causal connectives	-0.80 [-3.60, 2.00]	.575	0.69 [-2.38, 3.75]	.658	6.39 [-0.72, 13.5]	.078	4.71 [-1.98, 11.40]	.166
Adversative connectives	-1.32 [-4.06, 1.42]	.342	-2.02 [-4.11, 0.07]	.058	0.34 [-1.78, 2.46]	.751	-2.14 [-4.51, 0.23]	.077
Logical connectives	1.57 [-2.54, 5.68]	.452	0.60 [-3.43, 4.63]	.768	0.49 [-5.28, 6.27]	.866	-3.03 [-8.50, 2.44]	.275
Additive connectives	2.97 [-2.20, 8.14]	.258	-1.83 [-5.41, 1.75]	.315	-11.21 [-17.46, 4.96]	<.001†	-1.36 [-9.81, 6.10]	.720

Note. M_{diff} = repeatedly-recalled – control memory. *p* represents p-value from t-test comparing change in computer-scored measure to zero. We did not correct for multiple comparisons because these tests were pre-registered, but applying a correction does not affect our interpretation of these findings: instead, fewer comparisons are significant and there is even less evidence to suggest that repeatedly recalling a memory had an effect on computer-scored measures of coherence, regardless of the type of memory being recalled.

† represents significant difference between repeatedly-recalled and control memory

see the repeatedly-recalled and control memories differed on four of the 18 measures. Of the four measures that differed, the repeatedly-recalled memories were more coherent on two, and less coherent on two. If we then look at the negative memories, we see the repeatedly-recalled and control memories differed on three of the 18 measures. Of the three that differed, the repeatedly-recalled memories were more coherent on one and less coherent on two. Next, if we look at the positive memories, we see the repeatedly-recalled and control memories differed on four of the 18. Of the four that differed, the repeatedly-recalled memories were more coherent on one and less coherent on three. And finally, if we look at the neutral memories, we see that the repeatedly-recalled and control memories differed on four of the 18 measures. Of the four that differed, the repeatedly-recalled memories were more coherent on one and less coherent on three. There was little evidence, then, to suggest that repeatedly recalling a memory had an effect on computer-scored measures of coherence, regardless of the type of memory being recalled.

Taken together, these findings about computer-scored measures of coherence are consistent with those about the coherence reported by subjects. On Day 3, the traumatic memories were either more coherent, less coherent, or equivalent on coherence compared to the non-traumatic memories. For the majority of the computer-scored measures of coherence, the traumatic and non-traumatic memories showed an equivalent change over repeated recall; in the few instances of differences, there was little evidence to support the idea that the traumatic memories became incoherent, or less coherent than the non-traumatic counterparts. Furthermore, repeatedly recalling the memory had little effect on these computer-scored measures of coherence, regardless of the valence of the memory.

Symptoms of Posttraumatic Stress

We also had a secondary interest: to establish the extent to which people who reported more coherent traumatic memories tended to experience less current

posttraumatic stress. Recall there has been suggestion in the literature that the traumatic memories of people with PTSD are particularly incoherent but that this incoherence resolves over successful therapy (Brewin, 2016; Foa et al., 1995; van der Kolk & Fisler, 2005). To the extent this suggestion is true then symptoms and coherence should vary together such that the greater the increase in coherence the fewer symptoms of posttraumatic stress would be reported on Day 7. To address this issue, we turned to the subset of subjects who watched traumatic films and measured their posttraumatic stress by calculating their mean total Impact of Event Scale score (Weiss & Marmar, 1997) for their repeatedly-recalled memory.

In the mean, subjects reported a non-trivial degree of posttraumatic stress about their repeatedly-recalled memory (M = 12.35, 95% CI [10.53, 14.18], Mdn = 9). This finding replicates a finding from previous work using these materials (Taylor et al., 2020). People reported a diverse range of symptoms, across all subscales to varying degrees (Intrusions: M = 0.56, 95% CI = [0.46, 0.66]; Avoidance: M = 0.81, 95% CI = [0.69, 0.93]; Hyper-arousal: M = 0.23, 95% CI = [0.16, 0.30]). There was no evidence to suggest the symptoms people reported about a traumatic film on Day 7 were associated with the change in coherence they reported (r = -0.03, 95% CI [-0.19, 0.14], p = .76).[15] Having said that, there was a restricted range in the change in coherence ratings; therefore, we also examined the relation between coherence ratings on Day 7 and symptoms of posttraumatic stress, and found a similar pattern (r = 0.00, 95% CI [-0.17, 0.17], p = .98). We report these correlations with the caveat that correlations tend to stabilize at roughly 260 people (Schönbrodt & Perugini, 2013, 2018). Taken together,

[15] When we inspected the IES-R data, we found they were non-normal. Therefore, for both correlations reported here we also calculated Spearman's correlations and the findings were similar; respectively, r = -0.02, p = .83, and r = 0.00, p = .97

these findings do not fit with the idea that the coherence of people's traumatic memories is associated with their symptoms of posttraumatic stress.

Discussion

We asked 542 people to watch two films depicting either traumatic, negative, positive, or neutral events. We then asked them to recall one of those films using many of the same instructions used in imaginal exposure two, four, and six days later, and to rate the coherence of their memories for both films on Days 3 and 7. We also asked people to report their symptoms of posttraumatic stress about each memory on Day 7. We found a pattern of data consistent with the idea that when people repeatedly recalled a memory, it prevented the subjective loss of coherence they reported for their control memories. This pattern emerged regardless of whether that repeatedly-recalled memory was traumatic or non-traumatic. Moreover, there was no evidence that traumatic memories were, in absolute terms, incoherent. In additional analyses, computer-scored measures of coherence produced a similar pattern. Across 18 computer-scored measures, traumatic memories were a mix: sometimes more coherent, sometimes less coherent, and sometimes equivalent to their non-traumatic counterparts. We found little evidence that traumatic memories behaved differently over repeated recall compared to non-traumatic memories. Finally, there was no evidence of a link between the coherence of people's traumatic memories and their symptoms of posttraumatic stress.

At first glance, our findings are reminiscent of retrieval-induced forgetting (Anderson, 2003; Anderson et al., 1994; Barnier et al., 2010; Hauer & Wessel, 2006). In this literature, repeatedly recalling some pieces of information, but not others, results in a poorer memory of the non-recalled information. But our findings are not easily explained by this literature. More specifically, in a retrieval-induced forgetting paradigm, the repeatedly-recalled (for example, "apple") and non-recalled information ("banana") is

retrieved with the same experimenter-provided cue ("fruit"). Repeatedly recalling some of the information strengthens that information's association with the cue. Later, when the cue is presented, the repeatedly-recalled and non-recalled information compete for retrieval; access to the non-recalled information is impaired due to the stronger association between the repeatedly-recalled information and the cue (Anderson, 2003; Anderson et al., 1994). But in our experiment, the repeatedly-recalled and control memories were retrieved with different subject-generated and experimenter-provided cues. More specifically, after watching a film, people nominated a title for their memory of that film and were presented with a still image taken from the film. Each time people were asked to recall or make ratings about a specific memory, they were cued using this still image and title combination. These different cues meant the repeatedly-recalled and control films never competed for retrieval. It seems unlikely, then, that the lost coherence of the control memories is an artefact of repeatedly recalling another memory.

Taken together, our findings are inconsistent with the idea that traumatic memories operate by way of mechanisms that result in impoverished encoding and, as a result, incoherence (c.f. Brewin et al., 1996; Brewin et al., 2010; Foa & Riggs, 1993; van der Kolk & Fisler, 1995). More specifically, when we look at the traumatic memories in isolation, people rated these memories above the scale mid-point on coherence, regardless of whether they were making the ratings on Day 3 or 7. Even the memories we did not prompt people to recall were still subjectively coherent on Day 7. Furthermore, when we compared the traumatic and non-traumatic memories, there was no particular pattern—and certainly no evidence that the traumatic memories were consistently less coherent than their non-traumatic counterparts. Instead, our findings fit a "basic mechanisms" explanation of traumatic memories, in line with prior work (Rubin, Boals, & Berntsen, 2008; Rubin, Berntsen, & Bohni, 2008). In short, our data support the idea that the

coherence of traumatic memories can be explained by basic psychological mechanisms as opposed to hypothetical mechanisms allegedly unique to traumatic events (see also Rubin et al., 2011).

There was no consistent evidence to suggest the traumatic memories demonstrated a different trajectory of coherence over repeated recall than the non-traumatic memories. More specifically, when we looked at the difference between reported coherence on Days 3 and 7, we found people who watched traumatic films reported their memories lost more coherence than did those who watched non-traumatic films. When we looked at the extent to which people reported their memories became more or less coherent over the experiment, their reports about traumatic memories did not differ from those about the non-traumatic counterparts. Furthermore, when we examined the computer-scored measures of coherence, there were few differences; and where these differences existed, more often than not they were in the direction of a greater gain in coherence for traumatic memories. Taken together, these findings—in combination with the finding that on Day 7 the traumatic memories were still equivalent to the positive memories and more coherent than the neutral memories—mean we are not inclined to make too much of any one pattern.

Recall that people's symptoms of posttraumatic stress did not vary with either coherence on Day 7 or the change in coherence over time. This finding is puzzling given the emphasis in clinical literature that developing a coherent narrative is associated with a reduction in these kinds of symptoms (Ehlers et al., 2005; Foa & Rothbaum, 1998; van der Kolk & Fisler, 1995). Of course, this experiment was conducted in laboratory; not a clinical setting using trained therapists and following therapeutic protocols, though we incorporated these techniques into our experiment where possible. Nevertheless, our findings fit with prior work suggesting repeatedly recalling memories in therapy does not

increase coherence, even in those who show reduction in symptoms (Bedard-Gilligan et al., 2017). Taking this method out of therapy and into the laboratory allowed us the experimental control required to extend previous findings by showing what happens to memories when they are, versus are not, repeatedly recalled.

It is important, then, to consider what might be happening over repeated recalls to prevent the sense of lost coherence the memory might otherwise incur. After all, every session of repeated recall provides an opportunity for the so-called "double-edged sword of memory retrieval" to take hold (Roediger & Abel, 2022). On the one hand, then, repeated recall might serve to prevent or slow decay of the memory or even facilitate later retrieval (Carrier & Pashler, 1992; Ebbinghaus, 1885/1913; Meeter et al., 2005; Roediger & Karpicke, 2006). But on the other hand, it also leaves the memory vulnerable to reconstruction, making it possible for erroneous details to become consolidated into the memory (Alberini & LeDoux, 2013; Loftus et al., 1978; Roediger et al., 1996).

This reconstructive nature of memory is specifically exploited in imagery rescripting therapy; a treatment in which clients are encouraged to reconstruct the memory of their trauma with a more positive outcome (Hackmann, 1998). Incorporating this additional, more positive, material into the memory is seen as a way to update intrusive traumatic images, in turn helping to resolve symptoms of posttraumatic stress. But might this "rescripting" be happening unintentionally in therapies in which clients repeatedly recall their memories? We know, for example, that when people repeatedly imagine an event, they can develop the illusion of remembering more (Garry et al., 1996; Garry & Wade, 2005; Mazzoni & Memon, 2003). Moreover, recent evidence suggests people who complete lateral eye movements while recalling traumatic memories (as happens in EMDR) are more prone to incorporating inaccurate information into their memories than those who do not complete these eye movements (Houben et al., 2018;

Houben et al., 2020; but see also, Calvillo & Emami, 2019; Kenchel et al., 2022; van Schie & Leer, 2019). It seems possible, then, that people might display "imagination inflation" over the course of repeated recall in therapy, manufacturing thoughts, images, and feelings that make sense and fill in their memories. Further research is required to determine precisely how memories change over the course of repeated recall, and the types of details that are omitted or added at each recall.

Our findings, in conjunction with related research (Bedard-Gilligan et al., 2017; Rubin et al., 2016), have implications for the development and delivery of therapies for PTSD. More specifically, in some forms of therapy (for example, trauma-focused cognitive therapy for PTSD, Ehlers et al., 2005; Prolonged exposure therapy, Foa & Rothbaum, 1998), clinicians are encouraged to justify the use of repeated recall over the course of treatment in terms of the effect that recall will have on coherence and, as a result, symptomatology (Foa et al., 2007). But the recent literature does not fit with the idea incoherent memories develop into coherent memories over repeated recalls, and that this coherence is a key component for recovery. While we do not contest the efficacy of this "imaginal exposure" in treating PTSD, the collective findings on the coherence of traumatic memories and its relation with recovery make it prudent to revisit that justification.

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Chapter 5: Mini Meta-Analyses

Relative Coherence of Traumatic and Non-Traumatic Memories

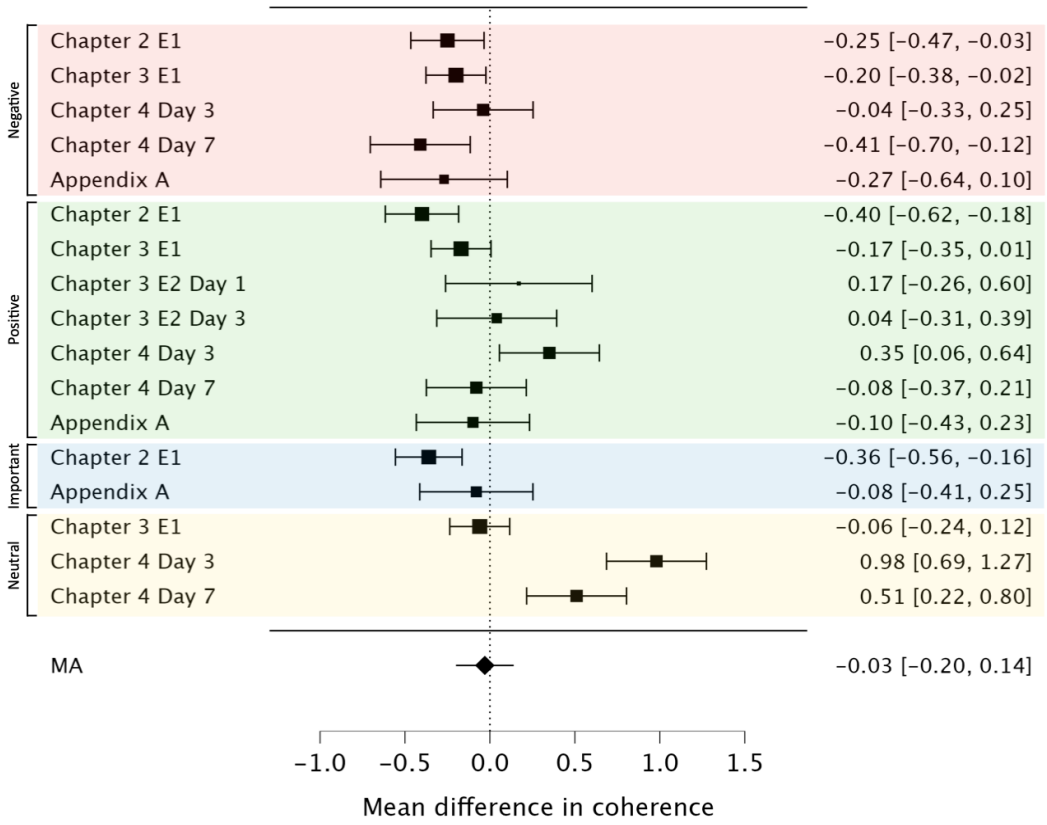
Across the experiments reported here, we examined the coherence of traumatic and various non-traumatic memories and found no particular pattern—the traumatic memories were sometimes more, sometimes less, and sometimes equivalently coherent to their non-traumatic counterparts. To estimate the relative coherence of people's traumatic and non-traumatic memories more precisely, in line with recommendations from Cumming (2014), we used the self-reported coherence data from our experiments to conduct a random effects model mini meta-analysis in JASP software (JASP Team, 2022). The data from Paper 1 Experiment 2 and Appendix D were not included because in those experiments we either did not ask people about a traumatic memory or did not include a comparison memory. A visualisation of this meta-analysis appears in Figure 1.

As the figure shows, people who recalled a traumatic memory rated that memory as 0.03 less coherent than those who recalled a non-traumatic memory (the maximum possible difference was 6, making this difference less than 1%). Furthermore, despite the increased precision in this analysis, the 95% confidence interval around this difference includes 0 as a plausible value. The findings from across our experiments, then, converge on the conclusion that traumatic memories are just as coherent as their various non-traumatic counterparts.

The figure also shows that meaningful variance across effect size estimates was high, indicating considerable heterogeneity between the comparisons, I^2 = 86.67% (I^2 is a measure of excess variance, expressed as a percentage of the total variance in the effect size estimates across comparisons). There are at least three factors that might help to explain this heterogeneity: 1) the type of comparison memory, 2) whether the

Figure 1

Forest Plot of Mean Difference in Self-Reported Coherence Between Traumatic and Various Non-Traumatic Memories



Note. The points plotted vertically represent the mean difference for each comparison, grouped by type of comparison memory. E = experiment. For Chapter 4, ratings were collapsed across repeatedly-recalled and control memories. The vertical line indicates the point of no mean difference between traumatic and non-traumatic memories; points to its left indicate lower mean coherence ratings for traumatic memories and points to its right indicate higher mean coherence ratings for traumatic memories. The black lines extending from each point show the 95% confidence interval around that mean difference. Larger points indicate samples given greater weighting in the meta-analysis; weighting determined by precision of effect size estimate (inverse of squared standard error). The diamond (labelled "MA") represents the result of the meta-analysis; its centre represents the estimated mean difference in the coherence of traumatic and non-traumatic memories, and its width shows the 95% confidence interval around that estimate.

experiment gathered measures of coherence of autobiographical or analogue memories, and 3) in the case of analogue memories, how long the delay was between watching the film and rating coherence. To determine whether these three factors helped to explain the observed differences between comparisons, we ran two random-effects model meta-regressions: first including as factors (a) the type of comparison memory and (b) whether the memories were for autobiographical or analogue events, and second using only the data from the trauma film paradigm experiments and including as factors (a) the type of comparison memory and (b) delay between watching the film and rating coherence.

In the first model, accounting for the type of comparison memory and whether the memories were for autobiographical or analogue events helped to explain some of the heterogeneity in the meta-analysis. That is, the omnibus test of model coefficients was significant, suggesting the coefficients for the included factors were not equal to zero, $Q(4) = 12.94$, $p = .012$; but none of the factors alone reached significance. Furthermore, there was still considerable residual heterogeneity unaccounted for; the test for residual heterogeneity was significant suggesting variation is larger than what we would expect if the comparisons were estimating the same effect size, $Q(12)= 57.98$, $p < .001$, $I^2 = 77.40\%$.

In the second model, accounting for the type of comparison memory and delay between watching a film and making a coherence rating also helped to explain some of the heterogeneity in the meta-analysis on data from experiments using the trauma film paradigm, $Q(4) = 15.24$, $p = .004$. Coefficient estimates suggested analogue traumatic memories were more coherent than their neutral counterparts, $M_{diff} = 0.42$, 95% CI [0.03, 0.80]. The estimates also suggested traumatic memories were less coherent than their non-traumatic counterparts when the ratings were made five minutes after watching the films, $M_{diff} = -0.44$, 95% CI [-0.81, -0.07]. This difference in coherence had resolved by

Days 3 and 7. It is possible that people who watched traumatic films were not able to predict what was going to happen next during those films as well as people who watched non-traumatic films could (for a similar idea, see Sherrill et al., 2019). The lower ratings of coherence for traumatic versus non-traumatic memories shortly after watching the films might, then, be the result of a residual feeling of unpredictability or chaos. Of course, unpredictability is not unique to traumatic events; positive events can also be unpredictable, such as unexpected wins in political elections or sports matches. It might be, however, that the traumatic films we used here happen to portray events that are less predictable than those portrayed by the non-traumatic films. Although we normed the films to ensure they were equivalent on "storyness" (a measure comprised of clarity, cohesiveness, coherence, understanding, and comprehensibility), we did not collect a measure of the extent to which people could predict the sequence of events in each film. It would be interesting to collect these data in the future, and to manipulate the predictability of various traumatic and non-traumatic events to examine any downstream effects on coherence.

Still, even including these factors in the model, there was substantial residual heterogeneity unaccounted for, $Q(6)= 23.64$, $p < .001$; $I^2 = 72.65\%$. This residual heterogeneity suggests (a) the type of comparison memory, (b) whether the memories were autobiographical or analogue, and (c) in the case of analogue memories, length of delay, account for some (but not all) of the variance between the comparisons.

This residual variance is perhaps not surprising considering the ratings of coherence people provided were subject to various experimental manipulations. More specifically, in the experiments reported in Paper 1 (Chapter 2), people made ratings of coherence after we varied the way in which we asked them to bring the memory to mind. In the experiment reported in Paper 3 (Chapter 4), people made ratings of coherence after

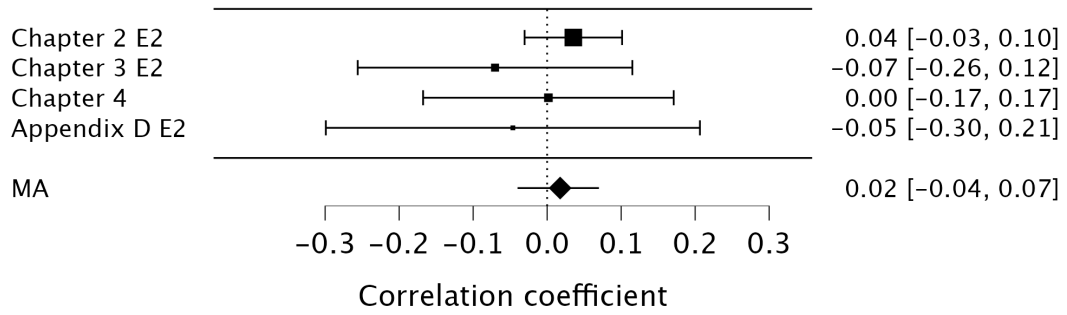
repeatedly recalling one memory, but not the other. Furthermore, the memory cues we provided to people differed between experiments. In the experiments reported in Paper 1 (Chapter 2), people completed a questionnaire about their exposure to various traumatic events and nominated the memory of the event that continued to bother them the most. In the pilot study reported in Appendix A, we randomly selected one of the three memories we asked people to recall that made them feel most negative, and then used their description of that memory to manually code whether the event satisfied the DSM-5 criteria for a traumatic event (American Psychiatric Association, 2013). Considering these differences, the residual variance between comparisons makes sense. Despite this residual variance, the meta-analysis supports the conclusion that, overall, traumatic memories were no less coherent than their non-traumatic counterparts.

Relation Between Reported Coherence and Symptoms of Posttraumatic Stress

Across the experiments reported here, we also found no evidence of a significant relation between ratings of coherence and symptoms of posttraumatic stress. But in some of these experiments, our sample size limited our ability to interpret these correlations; that is, in typical research scenarios, correlations do not tend to stabilise until they are using the data from at least 260 people (Schönbrodt & Perugini, 2013, 2018). Therefore, to estimate the relation between people's ratings of the coherence of their traumatic memories and the symptoms of posttraumatic stress they report more precisely, we used the data from four of our experiments (Paper 1 Experiment 2, Paper 2 Experiment 2, Paper 3, and Appendix D Pilot Study 2) to conduct a random-effects model mini meta-analysis. A visualisation of this meta-analysis can be found in Figure 2.

Figure 2

Forest Plot of Correlation Coefficients Between Ratings of Coherence of Traumatic Memories and Reported Symptoms of Posttraumatic Stress



Note. The points plotted vertically represent the correlation coefficient for each experiment. E = Experiment. For Chapter 4, data are from repeatedly-recalled memories. For Appendix D Experiment 2, data are from the first memory subjects rated. The vertical line indicates the point of no relation between traumatic and non-traumatic memories; points to its left indicate a negative relation such that less coherence is associated with greater symptomatology, and points to its right indicate a positive relation such that more coherence is associated with greater symptomatology. The black lines extending from each point show the 95% confidence interval around that correlation coefficient. Larger points indicate samples given greater weighting in the meta-analysis; weighting determined by precision of effect size estimate (inverse of squared standard error). The diamond (labelled "MA") represents the result of the meta-analysis; its centre represents the estimated correlation coefficient between ratings of coherence of traumatic memories and reported symptoms of posttraumatic stress, and its width shows the 95% confidence interval around that estimate.

As the figure shows, the meta-analysed correlation coefficient between coherence ratings and symptomatology was .02, and the 95% confidence interval around this

coefficient includes 0 as a plausible value. Meaningful variance across effect size estimates was low, suggesting little heterogeneity between the experiments, $I^2 = 0\%$. The findings from across our experiments, then, converge on the conclusion that there was no evidence of a relation between people's reports of coherence of a traumatic memory and their symptoms of posttraumatic stress.

Chapter 6: General Discussion

Summary of the Findings

The primary aim of this thesis was to reconcile discrepant views about the coherence of traumatic and non-traumatic memories by drawing on principles from basic cognitive psychology (for discrepant views, see Brewin, 2016; McNally, 2022; Rubin et al., 2016; for basic principles, see Alter & Oppenheimer, 2009; Campoy & Baddeley, 2008; Ebbinghaus, 1913; Marsh, 2007; Meeter et al., 2005; Rubin, Berntsen, & Bohni, 2008; Rubin, Boals, & Berntsen, 2008; Schwarz, 2012; Tversky & Marsh, 2000; Weingarten & Hutchinson, 2018). To achieve this aim, we conducted five experiments reported across three papers (Chapters 2, 3, and 4), two mini meta-analyses (Chapter 5), and three pilot studies (Appendices A and D).

In the first paper (Chapter 2), we reported two experiments in which we varied the way we asked people to recall their traumatic and non-traumatic memories. We found that when people were asked lots of questions about their memory, they judged that memory as less coherent than did those who were not asked lots of questions. In contrast, when people recalled their memory from beginning to end, as if telling a story, they judged that memory as more coherent than did those who were not asked to recall their memory in that way. These findings persisted regardless of whether the memories were traumatic or non-traumatic. Furthermore, the traumatic and non-traumatic memories alike were, in absolute terms, coherent. Finally, people's judgements of coherence did not vary depending on their reported symptoms of posttraumatic stress. Taken together, these findings suggest the ways in which people recall their memories might produce changes in how coherent those memories feel, in turn contributing to the mixed evidence about the coherence of traumatic memories.

In the second paper (Chapter 3), we reported on a new set of materials for use in the trauma film paradigm. We then used these materials to examine the coherence of people's memories for the various traumatic and non-traumatic films. We found that regardless of the type of film people watched, they judged their memories as not only coherent but equivalently coherent. Again, we found no evidence to suggest the coherence of people's traumatic memories varied depending on their reported symptomatology. These findings fit with those from the autobiographical memory literature, providing further evidence against the idea that people's memories for trauma are less coherent than their memories for non-traumatic events (Porter & Birt, 2001; Porter & Peace, 2007; Rubin, 2011; Rubin et al., 2004; Rubin et al., 2016; Wenzel et al., 2004).

In the third paper, a manuscript in preparation for submission (Chapter 4), we reported work in which we asked people to watch two films portraying traumatic or various non-traumatic events, and then to repeatedly describe their memory of one of those films over five days. This experiment produced a pattern of data consistent with the idea that repeatedly recalling a memory prevented a subjective loss of coherence that occurred for control memories. Furthermore, a similar pattern emerged regardless of whether that repeatedly-recalled memory was traumatic or non-traumatic. Across the computer-scored measures of coherence, traumatic memories were sometimes more, sometimes less, and sometimes equivalently coherent to their non-traumatic counterparts. Finally, there was no evidence of a significant relation between reported symptomatology and coherence. Taken together, these findings fit with the idea that developing coherence is not a necessary component of recovery from posttraumatic stress disorder (PTSD; see also, Bedard-Gilligan et al., 2017; Rubin, Berntsen, & Bohni, 2008; Rubin et al., 2016; Yufik & Simms, 2010).

In Chapter 5, we conducted two mini meta-analyses using the data from experiments reported here: the first to obtain a more precise estimate of the relative coherence of traumatic and various non-traumatic memories, and the second to obtain a more precise estimate of the relation between memory coherence and symptoms of posttraumatic stress. These meta-analyses converged on the conclusions that across our experiments, (1) people judged their memories for traumatic events as just as coherent as did those who judged their memories for non-traumatic events, and (2) there was no evidence of a relation between people's judgements of coherence and their reported symptomatology.

Relations With the Autobiographical Memory Literature

The findings reported here build on work from the autobiographical memory literature. A body of evidence suggests when people are asked about their traumatic and non-traumatic autobiographical memories, they report a pattern of data consistent with the idea that these memories "look like" each other with respect to coherence (Fernández-Lansac & Crespo, 2017; Gray & Lombardo, 2001; O'Kearney et al., 2011; Peace et al., 2008; Porter & Birt, 2001; Rubin, 2011; Rubin, Boals & Berntsen, 2008; Rubin et al., 2016). In our first paper (Chapter 2), we built on these findings, reporting data that suggest people's judgements about the coherence of their autobiographical traumatic and non-traumatic memories alike are sensitive to momentary attributions based on the way they bring a memory to mind. These findings provided another demonstration of traumatic and non-traumatic memories being similarly affected by basic cognitive mechanisms and manipulations (see for examples, Bornstein et al., 1998; Read & Lindsay, 2000; Rubin, Boals, & Berntsen, 2008; Rubin, Berntsen, & Bohni, 2008; Strange & Takarangi, 2012).

In our second and third papers (Chapters 3 and 4), our experimental designs that afforded comparisons between traumatic and various non-traumatic events buttressed findings about people's autobiographical memories. We gathered data on traumatic and non-traumatic memories comparable on important factors that could plausibly affect coherence—after equivalent delays—to address limitations in the existing literature. Even when accounting for these important factors, we found no consistent pattern of coherence: traumatic memories were sometimes more, sometimes less, and sometimes equivalently coherent compared to their non-traumatic counterparts. Taken together, then, the data do not support the idea that traumatic memories are uniquely incoherent (c.f. Brewin et al., 1996, 2010; Ehlers & Clark, 2000; Foa & Kozak, 1985, 1986; Foa et al., 1989, 2006; van der Kolk, 1987, 1994, 1997).

Limitations

Use of Analogue Traumatic Events

Some might see our use of the trauma film paradigm as a limitation. After all, the paradigm itself creates "analogue" experiences that would stand apart from one's life. In this way, the paradigm is clearly unsuitable for addressing the effects of any mechanism in which autobiography or personal involvement is paramount, such as proximity to a traumatic event or the integration of a trauma to people's life stories (for proximity, see May & Wisco, 2016; Wozniak et al., 2020; for integration, see Berntsen et al., 2003; Berntsen & Rubin, 2007; Brewin, 2016; Broadbridge, 2018).

Moreover, some would argue that the degree of distress people feel as a result of watching films of traumatic events does not match the distress caused by directly experiencing a traumatic event (see for example, Butler & Spiegel, 1997; Marks et al., 2018; van der Kolk & Fisler, 1995). Of course that is exactly the point of the paradigm: it provides a way to balance the demands of experimental control with those of ethical

research standards, exposing people to temporary and minimal harm (Stirling et al., 2021). People typically report experiencing a similar combination of PTSD symptoms after watching a traumatic film as they do for actual traumatic events, but to a lesser extent and for a shorter period of time (Holmes & Bourne, 2008; James et al., 2016; Segovia et al., 2016). We found a similar pattern across our experiments. More specifically, across the three experiments in which we collected data on symptoms, the majority of subjects who watched a traumatic film reported experiencing symptoms of posttraumatic stress (272/311). This finding fits with the idea that our traumatic films produced the same kind of symptoms as those produced by the traumatic events people experience. Experiments using the trauma film paradigm, then, should be seen to play a complementary role in understanding the relation between memory and dysfunction (for a similar point, see Lin et al., 2021; Stirling et al., 2021).

Use of Self-Report Measures

Self-Reported Coherence. Across our experiments we relied mostly on self-report measures of memory coherence. When we added computer-scored measures of coherence in the third paper (Chapter 4), the conclusions did not change. That is, regardless of whether the measures of coherence were self-report or computer-scored, there was little evidence to suggest traumatic memories were uniquely incoherent. Whether the traumatic memories were more, less, or equivalently coherent varied between measures and comparison memories. We did not, however, gather any observer-rated measures of coherence. Therefore, we cannot draw conclusions about the extent to which people's descriptions of traumatic and non-traumatic events contain the necessary information to make sense to an outside observer (Reese et al., 2011). Having said that, in one study that compared traumatic and various non-traumatic memories on 28 self-report, computer-scored, and observer-rated measures of coherence, those measures produced

similar patterns to the ones reported here (Rubin et al., 2016). Taken together, the evidence converges on a lack of unique incoherence in traumatic memories. Nonetheless, future research should endeavour to measure coherence in a variety of ways, including observer-rated coherence, to ensure a wide range of aspects of coherence are captured (O'Kearney & Perrott, 2006; Rubin et al., 2016).

Self-Reported PTSD Symptomatology. A critic might argue the lack of incoherent traumatic memories across our experiments is an artefact of using a self-report measure of symptoms rather than a clinician-administered interview. That is, perhaps people did not provide accurate reports of their symptoms of posttraumatic stress (for a review of the validity of self-report measures, see Chan, 2008). To the extent that when people complete the IES-R they do not accurately place themselves along the continuum from low- to high-distress, then our ability to detect a relation between memory coherence and symptomatology would be diminished. But such a counter-explanation is unsatisfying for at least three reasons. First the IES-R has good discriminative validity, and its scores are strongly correlated with those on both clinician-administered interviews and other self-report measures of symptomatology (Creamer et al., 2003; Ohtani et al., 2004; Weathers et al., 1993). Second, people diagnosed with PTSD by a clinician frequently score higher on the IES-R than do people classified as not having PTSD (Beck et al., 2008; Elsesser et al., 2004). And third, in one study comparing the traumatic memories of clinician-diagnosed PTSD sufferers with those of people classified as not having PTSD, across 28 measures of coherence there was no consistent evidence to suggest the PTSD sufferers' memories were less coherent (Rubin et al., 2016). Taken together, these findings suggest a more parsimonious explanation for our results: people who report high levels of symptomatology do not, in the mean, report incoherent traumatic memories. Nevertheless, this issue could be addressed in future research by

measuring symptoms of PTSD using a clinician-administered interview (such as the Clinician-Administered PTSD Scale; Blake et al., 1995; Weathers et al., 2018).

General Population Samples

A related concern might be our use of general population samples, as opposed to clinical samples with PTSD diagnoses. To the extent that both a) symptoms of posttraumatic stress and b) coherence fall along a continuum, there is little reason to think that mechanisms that operate on the memories of people who fall above the cut-off for a diagnosis of PTSD would be qualitatively different from those that operate on the memories of people who fall slightly below this cut-off (for a similar point, see Altman & Royston, 2006). That is, any relation between coherence and distress is most likely one of degree. Therefore, if traumatic memories are uniquely incoherent in PTSD, we would have expected to detect a relation between symptoms and coherence across our experiments, such that less coherent memories would be associated with a greater degree of symptomatology. But we consistently found no evidence of such a relation. Furthermore, the experiments reported here were not designed to be interpreted in isolation, but in conjunction with the body of research on autobiographical memories and observations from clinical settings.

Implications and New Questions

Implications for the Diagnosis of PTSD

Our findings have implications for the DSM-5's conceptualisation of PTSD. Dissociative amnesia—the "inability to remember an important aspect of the traumatic event(s)"—is listed as one of the symptoms of PTSD in the current DSM diagnostic criteria (American Psychiatric Association, 2013, p. 271). But several lines of research have called the utility of this symptom into question. More specifically, across two studies of people with PTSD, the amnesia symptom was one of the two least endorsed

symptoms in the DSM-5 diagnostic criteria, with only 8% of people reporting the symptom to a severe degree (Miller et al., 2012). Furthermore, the dissociative amnesia symptom accounts for little of the variance in people's PTSD symptomatology and does not fit with the other symptoms (Miller et al., 2012; for reviews of factor analyses on the DSM-IV criteria for PTSD, see Rubin, Berntsen, & Bohni, 2008; Yufik and Simms, 2010). Moreover, more recent thinking suggests symptoms of PTSD are not the outward expression of an underlying disorder but are instead themselves the disorder (McNally et al., 2015). In this line of thinking, symptoms of mental disorders are related to one another by way of causal networks. A recent review of network analyses of PTSD symptoms found the dissociative amnesia symptom was, across 27 network analyses, one of the three symptoms with the lowest centrality (Birkeland et al., 2020). Finally, a recent analysis of reports of dissociative amnesia suggested the majority of the reports failed to consider alternative explanations for the apparent amnesia, and the evidence in favour of dissociative amnesia was weak (Mangiulli et al., 2022). Our findings fit with this work—in the experiments reported here, we found no good evidence to suggest traumatic memories are uniquely incoherent, missing important pieces. Taken together, these findings converge on the idea that the dissociative amnesia symptom is of little value to the diagnostic criteria for PTSD.

Implications for Accounts of PTSD

The findings reported here also have implications for theories of PTSD. Recall several theories of PTSD take a "special mechanisms" view of traumatic memories. More specifically, these accounts propose that PTSD arises, in part, because of the fragmented and incoherent nature of traumatic memories (Brewin et al., 1996, 2010; Ehlers & Clark, 2000; Foa & Riggs, 1993; van der Kolk, 1987, 1994, 1997). This incoherence is said to stem from incomplete encoding and processing at the time of the event. Our findings are

not consistent with these accounts. The pattern of results found across our experiments does not fit with the idea that trauma-specific mechanisms are required to explain incoherence in traumatic memories: when we looked at both the absolute and relative levels of coherence, we found little evidence to suggest incoherence exists.

Furthermore, in our first paper (Chapter 2) we found no evidence to suggest people who report high levels of PTSD symptomatology have traumatic that they judge as less coherent than those who report lower levels of symptomatology. Across a further three experiments (reported in Chapters 3, 4, and Appendix D), we found no evidence of a relation between reported symptomatology and coherence; coherence did not vary with symptomatology in way that would suggest less coherent memories are associated with a greater degree of posttraumatic stress. Indeed, as reported in Chapter 5, the meta-analysed estimate of the relation between coherence and symptomatology across our experiments was .02. These findings are inconsistent with the special mechanisms accounts of PTSD in which incoherent traumatic memories underlie dysfunction, and should therefore be targeted in treatment (Brewin et al., 1996, 2010; Ehlers & Clark, 2000; Foa & Riggs, 1993; van der Kolk, 1987, 1994, 1997).

Instead, our findings are consistent with the autobiographical memory theory of PTSD (Rubin, Berntsen, & Bohni, 2008; Rubin, Boals, & Berntsen, 2008). According to this theory, the coherence of traumatic memories can be explained using the well-established cognitive and affective processes that account for memory more broadly. Therefore, the theory suggests drawing conclusions about the coherence of traumatic memories requires comparison to a battery of non-traumatic memories that account for these processes—for example, positive memories to account for emotional intensity, and negative memories to account for negative valence. Across our experiments, we used this battery of comparison memories and found no evidence to suggest these processes

interact in a way to produce a pattern of incoherent traumatic memories—even among people reporting the greatest degree of posttraumatic stress. Our findings support the conclusion there is no reason to view incoherent traumatic memories as playing a fundamental role in PTSD (see also Rubin et al., 2016).

An interesting avenue for future research would be to examine the extent to which, for some people, other cognitive disturbances that are common in PTSD might create a feeling of incoherence. After all, to the extent that symptoms of PTSD are causally related and reinforce one another, it seems possible that symptoms such as avoidance or intrusions might themselves create a feeling of incoherence (McNally et al., 2015). More specifically, if one continuously avoids thinking about the event, we might expect such a lack of rehearsal to promote decay and therefore a feeling that pieces of the event are missing from memory (Ebbinghaus, 1913; Meeter et al., 2005; for a review, see Anderson & Huddleston, 2012). Conversely, if certain aspects of the event continually come to mind without the additional aspects that tie the memory together, we might expect such selective rehearsal to lead to better memory for those aspects that intrude, promoting a feeling of the memory being disconnected and fragmented (Herz et al., 2020). Across our data we did not find evidence to suggest symptoms of PTSD were related to judgements of coherence, but perhaps for a subset of people experiencing PTSD, these more common symptoms might create a feeling of incoherence. Most relevant to the work I report in this thesis, we would expect a lack of, or selective, rehearsal to contribute to a feeling of incoherence regardless of whether the memory were for a traumatic or non-traumatic event. Such a finding, then, would provide evidence consistent with basic mechanisms accounts of PTSD, as opposed to special mechanisms theories (for basic mechanisms accounts, see Rubin, Berntsen, & Bohni, 2008; Rubin,

Boals, & Berntsen, 2008; for special mechanisms accounts, see Brewin et al., 1996, 2010; Ehlers & Clark, 2000; Foa & Riggs, 1993; van der Kolk, 1987, 1994, 1997).

Implications for the Justifications Given to Clients in Therapies for PTSD

Our findings have implications for cognitive therapies that seek to help clients develop coherent memories over the course of treatment. In prolonged exposure therapy, for example, one of the justifications for repeated recall is "processing and organizing the memory" (Foa et al., 2019, p. 99; Foa & Rothbaum, 1998). But we consistently found that people reported their traumatic memories were, in absolute terms, coherent—not disorganised or missing important pieces; a finding that suggests the incoherence that these therapies seek to resolve does not exist, in the mean. Why, then, does the idea that traumatic memories are incoherent persist? One possible explanation is that some people's memories are generally incoherent. We know some people recall their personal experiences as highly vivid and coherent memories that they frequently rehearse, while others recall less vivid, incoherent memories that they do not frequently rehearse (Berntsen et al., 2019). This general tendency to recall memories in a particular way has been shown to predict the phenomenological characteristics of people's specific autobiographical memories (Gehrt et al., 2022). Perhaps, then, clinical observations of incoherent traumatic memories stem from the subset of people who tend to recall their memories—traumatic and non-traumatic—in an incoherent manner. But in therapeutic settings, people are not asked about the coherence of their various non-traumatic memories, and so they never make this comparison.

Our meta-analysis on the relation between judgements of coherence and reported symptomatology (reported in Chapter 5) further supported the conclusion coherence and symptomatology do not vary together. Moreover, as reported in our third paper (Chapter 4), when we asked people to engage in repeated recall in line with these procedures, we

found no evidence to suggest this recall helped to boost coherence; instead, people's reports suggest it prevented a subjective loss of coherence seen in the memories that were not repeatedly recalled. When people did report a change in coherence over repeated recall, there was no evidence to suggest that change played a role in reducing symptomatology. These findings fit with those from other work on changes in coherence over therapy (Bedard-Gilligan et al., 2017; Kindt et al., 2017; van Minnen et al., 2012). Taken together, the evidence is inconsistent with clinical observations of increased coherence over the course of successful therapy (for example, Freud & Breuer, 1895/2004; Foa & Riggs, 1993; Janet, 1919/1925; van der Kolk & Fisler, 1995). As such, it might be time to question whether these therapeutic procedures should continue to be justified in this way.

If repeatedly recalling memories over the course of therapy does not boost coherence, and increased coherence does not underpin recovery, how might we explain the discrepancy between experimental findings and therapeutic observation? I suggest one way we could understand this discrepancy would be to draw on the literature on expectancy effects.

Expectancy effects. A large body of research demonstrates that people's beliefs about the way they will respond to a certain situation contribute to their actual responses (Kirsch, 1985, 1997, 2018). A common example of one of these "expectancy effects" is the placebo effect—a phenomenon in which people's expectations about how they will respond to medication produce changes in both experience and physiology (for reviews, see Kirsch, 1990; Kirsch, 2019). Expectancy effects have been demonstrated in many different contexts. For example, in one study, people who were told that others tend to find a given cartoon funny not only rated the cartoon as funnier, but also smiled and laughed more than those not given that expectancy (Wilson et al., 1989). In another study,

people given information about withdrawal symptoms to expect upon quitting smoking reported experiencing a greater number of those symptoms than people who were told to expect no symptoms (Tate et al., 1994).

Expectancy effects are even thought to play a role in the effectiveness of psychotherapeutic treatments, with much of the efficacy from treatments attributed to "non-specific" factors (including patients' expectancies) and some therapies specifically targeting clients' expectancies about their psychopathology (Klerman & Weissman, 1982; Rounsaville & Chevron, 1982; for reviews, see Kirsch, 1997; Kirsch et al., 2016; Noble et al., 2001; Tambling, 2012). Furthermore, there is some evidence to suggest the treatment rationales therapists give clients at the beginning of treatment can, themselves, produce expectancy effects (for a review, see Kanter et al., 2002). Considering these literatures, it would be prudent to ask the question: could the therapeutic justifications for repeated recall be feeding the persistent misconception that traumatic memories are incoherent, with coherence developing over the course of therapy as recovery begins? It is possible that expectancy effects could even account for some of the positive effects of prolonged exposure therapy more broadly (see also, McLane et al., 2019). Future research should address these issues.

Implications for the Courtroom

Many events that would satisfy Criterion A in the diagnostic criteria for PTSD would also constitute crimes; for example, sexual abuse, physical assault, and witnessing a murder (American Psychiatric Association, 2013). In these cases, the criminal justice system is often reliant on eyewitnesses to recall information about the event (Howe & Knott, 2015; Wells et al., 2006). It is important, then, to consider how the ways people might be asked about a traumatic memory in therapy could affect the testimonies they might subsequently give in court.

Content of Memories. In our first paper (Chapter 2), we reported evidence that fits with the idea people's judgements of coherence are sensitive to momentary attributions depending on how they retrieve a memory. In our third paper (Chapter 4), we found that repeatedly recalling a memory prevented a sense of lost coherence people reported in the memories they did not repeatedly recall. Taken together, these findings suggest the simple act of retrieving a memory can have downstream effects for judgements about that memory. But across our experiments, we did not examine how these different types of retrieval affected the content of people's memories. A possible direction for future research would be to investigate the degree to which the manipulations we reported here also have downstream effects for how people remember traumatic and non-traumatic experiences. After all, we know that asking people to retrieve a memory in various ways can affect the types of details they later report (for a review, see Marsh, 2007).

One interesting line of research would be to examine the extent to which the specific questions people might be asked in therapy shape the details they would later report when asked to freely recall the event. For example, would a focus on internal sensations during therapy hinder later recall of details that might be valuable in identifying a perpetrator? Or might these questions serve a facilitative function, strengthening memory for both retrieved and unretrieved information about the event? There are several bodies of literature supporting both possibilities (for reasons to think it might hinder, see Anderson et al., 1994; Aue et al., 2015; Bäuml & Sameniegh, 2010; Roediger, 1978; Shaw et al., 1995; for reasons to think it might help, see Bäuml, 2019; Bäuml & Schlichting, 2014; Chan et al., 2006; for a review, see Roediger & Abel, 2022).

It would also be interesting to examine the content of people's traumatic and non-traumatic memories over repeated recall. There are at least two reasons to suspect that

people will reconstruct a memory as they repeatedly recall it, in turn producing enduring changes to the memory itself. First, we know that as people repeatedly retell a story, they engage in "levelling" and "sharpening"—omitting details that they deem unimportant or confusing to the story, or highlighting other details that help to create meaning (Allport & Postman, 1945; Bartlett, 1932; Bergman & Roediger, 1999). In our third paper (Chapter 4), when we asked people to repeatedly recall their memories, we found some evidence that fits with the idea they were engaging in levelling and sharpening. More specifically, we found that people's descriptions of their memories were longer on Day 5 than they were on Day 3, and then shortened on Day 7 back to a similar length to Day 3—and yet their reports of coherence did not reflect this lost detail from Days 5 to 7. Compared to the memories that were not the target of repeated recall, these repeatedly-recalled memories retained their sense of subjective coherence. Perhaps, then, as people repeatedly recalled their memory, they dropped details they thought unimportant or confusing, and sharpened other details, preserving the coherence of the memory.

Second, we know that each time people bring an event to mind, they can develop the illusion of remembering more (Garry et al., 1996; Garry & Wade, 2005; Li et al., 2020; Mazzoni & Memon, 2003; Thomas et al., 2003; Wade et al., 2002). It is possible, then, that people might display "imagination inflation" as they recall their memories, prompting them to manufacture thoughts, images, and feelings that make sense and fill in their memories. The trauma film paradigm materials we reported here provide the necessary baseline to examine the consistency and accuracy of traumatic and non-traumatic memories alike over repeated recall.

Confidence in Memories. It is plausible that the manipulations reported here might also affect people's confidence in their memories. If being asked many specific questions about a traumatic event draws people's attention to gaps in their memories, they

might report less confidence in that memory than they would have were they not asked those questions. In contrast, if being asked to repeatedly recall a memory leads people to fill in their memories with manufactured details, people might report more confidence in that memory than they would have were they not asked to repeatedly recall it.

But misplaced confidence can wreak havoc in the courtroom. For example, people who are more confident in their memories report more willingness to testify in court than those who are less confident (Fukushima et al., 2022; Wells & Bradfield, 1998). Moreover, laypeople tend to believe confidence is an indicator of accuracy—the more confident an eyewitness, the more accurate their memory is assumed to be (Simons & Chabris, 2011). The various ways people are asked to recall a memory in therapy, then, might plausibly have downstream consequences for the outcomes of any subsequent criminal investigations. Furthermore, if repeated recall simultaneously increases confidence while decreasing accuracy, it is possible those with less reliable memories would be more likely to end up testifying in the courtroom—and their testimonies given a greater weight among jurors—than their more accurate counterparts.

It is important, then, to consider the possibility that encouraging people to search for missing details in an attempt to resolve incoherence might actually be doing more harm than good. After all, if extreme arousal during a traumatic event causes problems for encoding and consolidation, then any "lost" information cannot be recovered (Loftus, 1993; Otgaar, Howe & Patihis, 2022). And we know that in searching for lost memories, people can come to remember things that never happened at all—sometimes with disastrous consequences (Hyman & Pentland, 1996; Lindsay & Read, 1994; Loftus, 1993; Otgaar, Curci, et al., 2022; Otgaar et al., 2019).

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Appendix A: Pilot Study for Paper 1 (Chapter 2)

Before running the experiment proper, we first conducted a pilot study to a) test our manipulation of retrieval task, and b) determine how many subjects we needed to include to have a sufficiently large sample of people rating traumatic memories. This study was pre-registered and the pre-registration can be found on OSF (https://osf.io/n2jwe).

Method

Subjects

We recruited subjects on Amazon's Mechanical Turk (MTurk) using the TurkPrime platform (Litman et al., 2017). We set our target sample size at 900 completed responses and collected data from a total of 937 MTurk workers. After applying our pre-registered exclusion criteria (see Results), we retained the data from 744 subjects for analyses (M_{age} = 38.54, SD_{age} = 12.72; 29% identified as men, 70% identified as women, and 1% identified as gender diverse). Of these subjects, 96% reported English was their first language. Subjects received $0.70US for participating in the study.

Design

We used a 4 (memory type: positive, negative, important, traumatic) x 3 (retrieval task: describe then rate, rate only, probing then rate) between-subjects design.

Procedure

The experiment was approved by the University of Waikato's School of Psychology Research and Ethics Committee under the delegated authority of the University's Human Research Ethics Committee. The experiment consisted of three phases, administered online using Qualtrics.

Phase 1. First, we randomly assigned subjects to nominate the memories of three events from their lives that were either positive, negative, or important. To cue for

positive memories, we asked subjects to "please bring to mind three memories that now make you feel most positive." To cue for negative memories, we asked subjects to "please bring to mind three memories that now make you feel most negative." To cue for important memories, we asked subjects "please bring to mind three memories that now feel like your most important life events." All subjects were instructed to bring to mind memories for events that occurred at least one month ago. After bringing these memories to mind, subjects were asked to provide a short title for each of the memories. Qualtrics then selected one of the three memories at random, piped the title for that memory back to the subject, and asked them to "keep in mind the following memory" for the duration of the survey.

Phase 2. Next, subjects were randomly assigned to complete one of three retrieval tasks. "Describe then rate" subjects were first asked to "please take your time and report everything that you can remember about the event. Make sure to leave nothing out. Start at the beginning and give a complete description of the entire event." After providing this description, these subjects rated their memory on 21 items from the Autobiographical Memory Questionnaire (AMQ; Rubin et al., 2003, 2004). As in the experiment proper, we created a coherence sum variable using the following two AMQ items: the *story* item, "My memory comes to me in words or in pictures as a coherent story or episode and not as an isolated fact, observation, or scene" (*1 = not at all, 7 = completely*), and the *pieces* item, "This memory comes in pieces, with bits missing (*1 = not at all, 7 = completely; reverse coded)*." "Probing then rate" subjects were first asked a series of 24 specific probing questions about their memory (the same questions used in the experiment proper). Next, Qualtrics randomly selected five of these probing questions for each subject, piped the subject's answers back to them, and asked them to please provide more detail about each of these aspects of the event. After providing these additional details,

subjects then completed the AMQ as above. Finally, "rate only" subjects were asked only to complete the AMQ.

Phase 3. In the final phase of the experiment, to determine whether our manipulation of retrieval task was successful, subjects rated "how much effort did it require to recall details from this event?" (*1 = no effort, 5 = a lot of effort*), and answered a yes/no question designed to capture Criterion A traumas: "Did the event involve actual or threatened death, serious injury, or sexual violence to yourself or others?" (American Psychiatric Association, 2013). The 93 subjects who said "yes" in response to this question and had nominated either a negative or an important memory were reassigned from their original memory type into the "traumatic memory" group. Finally, subjects completed a short demographics questionnaire, and answered a series of questions about their compliance with our instructions throughout the experiment, before being fully debriefed.

Results and Discussion

Exclusions

We excluded 193 subjects on the basis of our pre-registered exclusion criteria. Of these 193, 2 subjects provided nonsensical descriptions of their event or nonsensical responses to the probing questions; 144 subjects nominated either (a) a negative memory they rated below the scale midpoint on negativity and intensity, (b) a positive memory they rated below the scale midpoint on positivity and intensity, or (c) an important memory they rated below the scale midpoint on significance; 63 subjects failed the attention check we had built in to the AMQ. We excluded some subjects on the basis of more than one of these criteria. After these exclusions, we retained 744 useable data-points.

We report the number of subjects in each cell, after exclusions, in Table 1. This table displays two important findings. First, the number of subjects who rated traumatic memories was much smaller than the number who rated positive, negative, and important memories. This under sampling of traumatic memories means we have much less precision in that group and is a threat to internal validity. This finding led us to change the way we cued for traumatic memories in the experiment proper. Second, "probing then rate" subjects dropped out of the experiment at a higher rate than their "describe then rate" and "rate only" counterparts. More specifically, 52% of probing then rate subjects dropped out compared to 17% of describe then rate and 4% of rate only subjects. This drop-out rate led to the uneven cell sizes evident in the table and was a threat to internal validity. Therefore, in the experiment proper we added in a monetary bonus for subjects to complete these probing questions, and dropped the requirement for subjects to provide more information about some of the probed aspects of the event.

Manipulation Checks

Next, we carried out manipulation checks to ensure that subjects' nominated positive, negative, and important memories were reasonable comparison memories to the

Table 1

Number of Subjects per Cell After Exclusions

	Retrieval Task		
Memory type	Describe then rate	Rate only	Probing then rate
Positive	74	106	58
Negative	66	65	29
Important	100	102	51
Traumatic	36	34	23

traumatic memories. We report these data in Table 2. As the table shows, subjects' traumatic memories were more negative (and less positive) than their positive and important counterparts, but similar to their negative counterparts. The memories were all similarly intense, and the important memories were slightly more significant than the traumatic, negative, and positive memories. Taken together, these data suggest the nominated positive, negative, and important memories are suitable comparisons to the traumatic memories.

Coherence of Memory

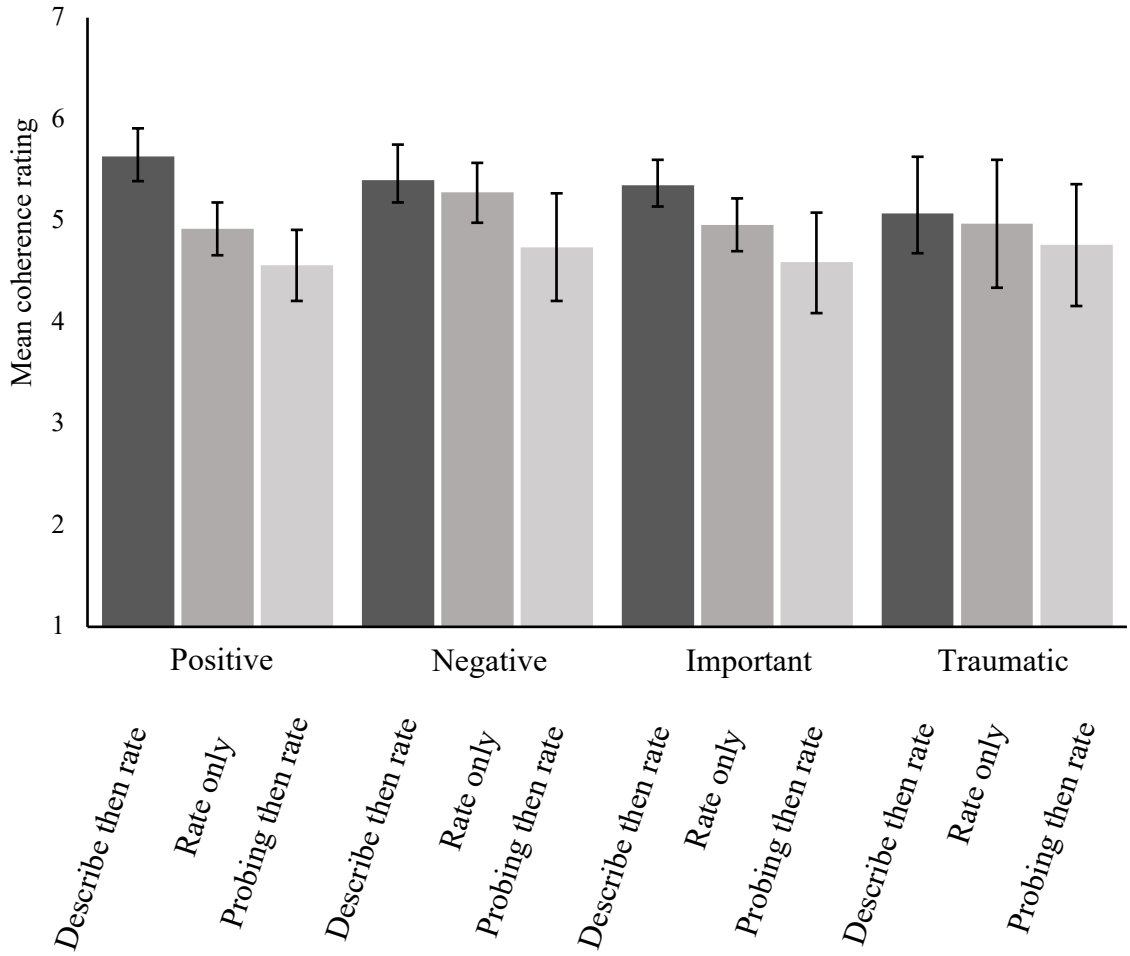
We now turn to the primary research question: To what extent do people make momentary attributions about the coherence of their memories, depending on the conditions under which they retrieve those memories? To answer this question, we determined the extent to which subjects' ratings of coherence depended on the type of memory they retrieved and the way they retrieved that memory. We first calculated subjects' scores on our coherence sum variable, and then calculated the mean coherence sum variable score and 95% CI around it for each possible combination of type of memory and retrieval task. We display these data in Figure 1.

The figure reveals at least three interesting findings. First, regardless of the type of memory they retrieved, subjects rated their memories as fairly coherent. Second, people's ratings of coherence depended on the way they retrieved their memory. That is,

Figure 2

Subjects' Mean Manipulation Check Ratings Split by Memory Type

	Positive Memories		Negative Memories		Important Memories		Traumatic Memories	
Item	*M*	95% CI	*M*	95% CI	*M*	95% CI	*M*	95% CI
Positivity	6.43	[6.32, 6.54]	1.81	[1.60, 2.01]	5.57	[5.33, 5.81]	2.08	[1.74, 4.41]
Negativity	1.70	[1.52, 1.88]	6.04	[5.89, 6.19]	2.44	[2.19, 2.69]	5.68	[5.32, 6.04]
Intensity	5.51	[5.38, 5.64]	5.64	[5.47, 5.81]	5.39	[5.22, 5.56]	6.05	[5.82, 6.28]
Significance	5.37	[5.15, 5.60]	5.23	[4.95, 5.50]	6.08	[5.95, 6.21]	5.57	[5.24, 5.90]



describe then rate subjects rated their memories as more coherent than their rate only and probing then rate counterparts. Likewise, probing then rate subjects rated their memories as less coherent than their rate only and describe then rate counterparts. These findings suggest people's judgements of coherence are sensitive to attributions made in the moment depending on the way they retrieve a memory. And third, subjects' ratings of coherence did not vary depending on the type of memory they retrieved. More specifically, subjects who retrieved traumatic memories rated their memories as similarly coherent to those who retrieved negative, positive, and important memories. We found no evidence, then, of incoherent traumatic memories.

In statistical terms, there was no Memory Type (positive, negative, important, traumatic) x Retrieval task (describe then rate, rate only, probing then rate) interaction on ratings of coherence: $F(6, 737) = 0.90$, $p = 0.49$. There was an effect of retrieval task on coherence, $F(2, 741) = 11.29$, $p < .0001$. Post-hoc Tukey comparisons revealed significant differences in coherence ratings comparing all combinations of retrieval tasks. More specifically, describe then rate subjects rated their memories as more coherent than both their rate only counterparts, $M_{diff} = 0.38$, 95% CI [0.11, 0.66], and their probing then rate counterparts, $M_{diff} = 0.77$, 95% CI [0.44, 1.09]. Probing then rate subjects also rated their memories as less coherent than their rate only counterparts, $M_{diff} = -0.38$, 95% CI [-0.70, -0.07]. There was no effect of memory type on coherence, $F(3, 740) = 0.59$, $p = 0.62$.

Taken together, the findings from this pilot study provide support for our hypothesis that coherence is sensitive to attributions people make in the moment, and do not fit with the idea that people's memories for traumatic experiences are incoherent. But by conducting this pilot study we identified two methodological challenges that made it difficult to interpret these findings (the small number of subjects rating traumatic

memories, and the drop-out issue for subjects assigned to answer probing questions) and we therefore set out to address these challenges in the experiment proper.

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Appendix B: Supplemental Information for Paper 1 (Chapter 2)

Experiment 1

Method

Recall that after each subject had produced a title for all of types of memories, Qualtrics selected one of the four memories at random, and piped it back to the subject, asking "keep in mind the following memory" for the duration of the survey. Consistent with previous findings, exposure to traumatic events in our sample was 88% (Breslau, Reboussin, Anthony, & Storr, 2005). The 52 subjects assigned to rate a traumatic memory who reported never experiencing a TLEQ event were randomly re-assigned to a negative (n = 18), positive (n = 20), or important memory (n = 14). As pre-registered, we analysed results both with and without these subjects—because their inclusion did not affect the pattern of results, we retained them in the dataset.

In addition, at the end of the final phase, we asked subjects a question to satisfy institutional ethics requirements: "Compared to how you felt about this event before you began the study, how do you feel about it now?" (1 = *Much worse*, 3 = *The same*, 5 = *Much better*). Subjects who rated negative and traumatic memories reported feeling slightly worse about the event; those who rated positive and important memories reported feeling slightly better. That is, M_{negative} = 2.91, 95% CI [2.84, 2.97]; $M_{\text{traumatic}}$ = 2.92 [2.84, 2.99]; M_{positive} = 3.35 [3.28, 3.41]; $M_{\text{important}}$ = 3.34 [3.27, 3.41]. Note that subjects who rated negative and traumatic memories reported only a mean 2% shift away from feeling "the same."

Number of Subjects per Cell

Table S1
Experiment 1: Number of Subjects per Cell After Exclusions

	Retrieval Task		
Memory type	Describe then rate	Rate only	Probing then rate
Positive	143	150	125
Negative	143	140	135
Important	151	159	121
Traumatic	135	133	105

Drop-out Rate

With the promise of a small bonus, only 23% of "probing then rate" subjects dropped out of the study compared to 52% of those in the pilot. Retrieval task still mattered for drop-out rates, but rates did not depend on the combination of retrieval task and type of memory recalled. Put another way, retrieval task predicted drop-out—but memory type, and the interaction between retrieval task and memory type, did not; $\chi^2_{\text{retrieval task}}(2, N = 2467) = 27.14$, $p < .001$; $\chi^2_{\text{type of memory}}(3, N = 2467) = 4.92$, $p = 0.177$. $\chi^2_{\text{retrieval task*type of memory}}(6, N = 2467) = 5.33$, $p = 0.502$. Therefore, although "probing then rate" subjects were more likely to drop out of the experiment, they were no more likely to drop if they answered probing questions about a traumatic memory than a positive memory, for example.

This differential drop-out rate probably works against our effect, because subjects who dropped out of the study might have been the ones who found the experience of answering probing questions most difficult. If our overarching hypotheses are correct, these subjects would have reported less coherent memories than their counterparts who went on to answer all the probing questions and finish the study.

Manipulation Checks

Positive memories were rated as more positive (and less negative) than negative and traumatic memories, and as similar in emotional intensity. Important memories were rated as more significant than traumatic memories, but similarly emotionally intense. That is, *Positivity*: $M_{positive}$ = 6.46, 95% CI [6.38, 6.54], $M_{negative}$ = 1.80 [1.69, 1.92], $M_{important}$ = 5.24 [5.04, 5.45], $M_{traumatic}$ = 1.91 [1.77, 2.05]; *Negativity*: $M_{positive}$ = 1.62 [1.50, 1.74], $M_{negative}$ = 6.06 [5.96, 6.16], $M_{important}$ = 2.73 [2.53, 2.93], $M_{traumatic}$ = 5.70 [5.55, 5.84]; *Intensity*: $M_{positive}$ = 5.17 [5.02, 5.31], $M_{negative}$ = 5.50 [5.36, 5.64], $M_{important}$ = 5.43 [5.29, 5.57], $M_{traumatic}$ = 5.41 [5.25, 5.57], $M_{important}$ = 5.43 [5.29, 5.57]; *Significance*: $M_{positive}$ = 5.57 [5.42, 5.72], $M_{negative}$ = 5.19 [5.02, 5.36], $M_{important}$ = 6.32 [6.23, 6.40], $M_{traumatic}$ = 5.08 [4.89, 5.27].

Cultural Life Script

The number of subjects who reported an event from the CLS differed depending on the type of memory they reported: χ^2 (3, N = 1640) = 38.07, $p < .001$. Fifty three percent of "positive" subjects reported a CLS event: 15% "having children," 10% "marriage," 7% "falling in love," and 6% "college." The remaining 15% were distributed across the remaining CLS categories; 45% of "negative" subjects reported a CLS event:12% "other's death," 8% "marriage," and 6% "parent's death." The remaining 19% were distributed across the remaining CLS categories; 66% of "important" subjects reported a CLS event—19% "having children," 13% "marriage," 6% "college," and 6% "falling in love." The remaining 22% were distributed across the remaining CLS categories; 53% of "traumatic" subjects reported a CLS event—25% "other's death," 9% "parent's death," and 5% "having children." The remaining 14% were distributed across the remaining CLS categories.

Table S2

Experiment 1: AMQ Ratings Broken Down by Cell (for wording of items please see osf.io/jzbr7)

	Positive Memories			Negative Memories			Important Memories			Traumatic Memories		
Item	Describe then rate *M* [95% CI]	Rate only *M* [95% CI]	Probing then rate *M* [95% CI]	Describe then rate *M* [95% CI]	Rate only *M* [95% CI]	Probing then rate *M* [95% CI]	Describe then rate *M* [95% CI]	Rate only *M* [95% CI]	Probing then rate *M* [95% CI]	Describe then rate *M* [95% CI]	Rate only *M* [95% CI]	Probing then rate *M* [95% CI]
Reliving	5.76 [5.55, 5.97]	5.28 [5.03, 5.53]	5.34 [5.11, 5.56]	5.36 [5.11, 5.61]	4.74 [4.46, 5.02]	5.22 [4.94, 5.50]	5.66 [5.45, 5.86]	5.21 [4.95, 5.47]	5.45 [5.19, 5.70]	5.32 [5.05, 5.58]	4.61 [4.30, 4.92]	5.12 [4.83, 5.42]
Hear	5.55 [5.30, 5.80]	5.28 [5.01, 5.55]	4.96 [4.67, 5.25]	5.35 [5.08, 5.62]	4.85 [4.56, 5.15]	5.19 [4.91, 5.48]	5.43 [5.19, 5.67]	5.25 [4.99, 5.50]	5.02 [4.72, 5.33]	5.42 [5.13, 5.69]	4.81 [4.51, 5.12]	5.01 [4.68, 5.34]
See	6.10 [5.93, 6.28]	5.97 [5.79, 6.15]	5.86 [5.66, 6.07]	5.96 [5.78, 6.14]	5.58 [5.36, 5.80]	5.76 [5.53, 6.00]	6.11 [5.96, 6.27]	5.96 [5.78, 6.15]	6.07 [5.87, 6.26]	5.95 [5.73, 6.16]	5.49 [5.27, 5.71]	5.87 [5.63, 6.10]
Hear talking	4.94 [4.64, 5.25]	4.87 [4.59, 5.15]	4.77 [4.47, 5.07]	4.87 [4.57, 5.18]	4.65 [4.32, 4.98]	4.43 [4.09, 4.77]	4.93 [4.64, 5.22]	4.82 [4.54, 5.10]	4.81 [4.50, 5.12]	4.77 [4.46, 5.09]	4.25 [3.92, 4.57]	4.36 [3.99, 4.74]
Emotions	5.92 [5.72, 6.12]	5.65 [5.42, 5.89]	5.58 [5.35, 5.81]	5.73 [5.49, 5.97]	5.55 [5.31, 5.79]	5.75 [5.51, 5.99]	6.03 [5.85, 6.21]	5.74 [5.52, 5.95]	5.78 [5.56, 6.00]	5.50 [5.27, 5.74]	5.31 [5.04, 5.57]	5.47 [5.16, 5.78]
Story	5.59 [5.35, 5.84]	5.06 [4.78, 5.34]	5.11 [4.84, 5.38]	5.48 [5.22, 5.73]	4.93 [4.64, 5.22]	4.78 [4.48, 5.08]	5.55 [5.32, 5.78]	5.15 [4.87, 5.43]	5.07 [4.75, 5.39]	5.27 [4.99, 5.54]	4.74 [4.44, 5.03]	5.04 [4.73, 5.35]
Setting	6.27 [6.10, 6.43]	6.23 [6.06, 6.41]	6.15 [5.99, 6.32]	6.19 [6.02, 6.35]	5.96 [5.76, 6.17]	5.97 [5.76, 6.18]	6.25 [6.11, 6.39]	6.13 [5.95, 6.31]	6.11 [5.92, 6.30]	6.03 [5.82, 6.24]	5.87 [5.67, 6.08]	6.05 [5.87, 6.22]
Pieces (R)	5.21 [4.92, 5.50]	5.13 [4.84, 5.43]	4.34 [4.00, 4.69]	5.20 [4.90, 5.50]	5.01 [4.68, 5.34]	4.16 [3.80, 4.51]	5.11 [4.80, 5.41]	5.00 [4.70, 5.30]	4.27 [3.92, 4.63]	5.04 [4.72, 5.36]	4.36 [4.01, 4.71]	3.48 [3.08, 3.87]
Talked	5.21 [4.91, 5.51]	5.45 [5.17, 5.72]	5.03 [4.71, 5.35]	4.85 [4.55, 5.14]	4.48 [4.16, 4.79]	4.11 [3.78, 4.45]	5.13 [4.85, 5.42]	5.49 [5.23, 5.75]	5.33 [5.03, 5.63]	4.46 [4.15, 4.77]	4.17 [3.86, 4.48]	4.61 [4.25, 4.97]
Thought	5.30 [5.03, 5.58]	5.49 [5.24, 5.73]	4.94 [4.64, 5.25]	4.71 [4.42, 5.01]	4.50 [4.19, 4.81]	4.11 [3.78, 4.44]	5.35 [5.11, 5.59]	5.55 [5.31, 5.80]	5.24 [4.96, 5.52]	4.27 [3.95, 4.60]	3.99 [3.66, 4.33]	4.39 [4.03, 4.75]
Out of blue	4.70 [4.40, 5.00]	4.65 [4.35, 4.95]	4.30 [3.96, 4.64]	4.69 [4.37, 5.01]	4.70 [4.39, 5.01]	4.16 [3.82, 4.49]	4.74 [4.44, 5.03]	4.83 [4.56, 5.10]	4.79 [4.48, 5.11]	4.39 [4.04, 4.73]	4.74 [4.43, 5.06]	4.75 [4.40, 5.10]
Remember/ know	5.98 [5.76, 6.20]	5.93 [5.72, 6.18]	5.75 [5.51, 5.99]	5.84 [5.61, 6.07]	5.76 [5.51, 6.02]	5.48 [5.22, 5.74]	6.10 [5.92, 6.28]	5.84 [5.60, 6.08]	5.64 [5.36, 5.93]	5.63 [5.36, 5.90]	5.80 [5.55, 6.05]	5.63 [5.36, 5.89]
Travel in time	5.20 [4.91, 5.50]	5.04 [4.77, 5.31]	4.86 [4.56, 5.17]	4.90 [4.61, 5.19]	4.44 [4.11, 4.76]	4.87 [4.56, 5.17]	5.21 [4.94, 5.49]	5.14 [4.85, 5.42]	5.02 [4.70, 5.33]	4.90 [4.60, 5.20]	4.57 [4.26, 4.88]	4.84 [4.50, 5.18]

Table S2

Experiment 1: AMQ Ratings Broken Down by Cell (for wording of items please see osf.io/jzbr7)

	Positive Memories			Negative Memories			Important Memories			Traumatic Memories		
Item	Describe then rate *M* [95% CI]	Rate only *M* [95% CI]	Probing then rate *M* [95% CI]	Describe then rate *M* [95% CI]	Rate only *M* [95% CI]	Probing then rate *M* [95% CI]	Describe then rate *M* [95% CI]	Rate only *M* [95% CI]	Probing then rate *M* [95% CI]	Describe then rate *M* [95% CI]	Rate only *M* [95% CI]	Probing then rate *M* [95% CI]
Significance	5.58 [5.33, 5.83]	5.61 [5.36, 5.87]	5.51 [5.24, 5.79]	5.20 [4.91, 5.48]	5.21 [4.92, 5.50]	5.15 [4.83, 5.47]	6.33 [6.18, 6.48]	6.36 [6.22, 6.50]	6.24 [6.07, 6.41]	5.17 [4.89, 5.46]	4.65 [4.31, 5.00]	5.50 [5.18, 5.83]
General knowledge	2.52 [2.22, 2.81]	2.31 [2.02, 2.59]	2.30 [2.00, 2.60]	2.17 [1.92, 2.43]	2.58 [2.26, 2.90]	2.15 [1.89, 2.41]	2.10 [1.84, 2.36]	2.17 [1.90, 2.44]	2.31 [2.00, 2.62]	1.99 [1.72, 2.27]	2.38 [2.08, 2.69]	2.32 [1.98, 2.67]
Persuaded wrong	2.33 [2.04, 2.61]	2.27 [2.02, 2.53]	2.61 [2.28, 2.93]	2.24 [1.98, 2.51]	2.60 [2.29, 2.91]	2.33 [2.05, 2.60]	2.21 [1.95, 5.46]	2.30 [2.04, 2.55]	2.90 [2.58, 3.22]	2.19 [1.91, 2.46]	2.35 [2.08, 2.61]	2.83 [2.46, 3.19]
Belief	6.43 [6.27, 6.59]	6.41 [6.25, 6.56]	6.30 [6.11, 6.48]	6.22 [6.02, 6.43]	6.23 [6.03, 6.43]	6.21 [6.01, 6.41]	6.48 [6.36, 6.61]	6.22 [6.03, 6.41]	6.27 [6.08, 6.46]	6.31 [6.10, 6.52]	6.20 [6.00, 6.41]	6.2 [5.97, 6.43]
Intensity	5.20 [4.96, 5.45]	5.25 [4.99, 5.50]	5.03 [4.77, 5.30]	5.59 [5.35, 5.82]	5.31 [5.05, 5.56]	5.61 [5.36, 5.87]	5.51 [5.29, 5.73]	5.31 [5.06, 5.56]	5.48 [5.21, 5.75]	5.48 [5.23, 5.74]	5.15 [4.88, 5.42]	5.65 [5.34, 5.95]
Negativity	1.59 [1.39, 1.80]	1.62 [1.43, 1.81]	1.64 [1.42, 1.86]	6.01 [5.85, 6.17]	6.09 [5.92, 6.26]	6.08 [5.89, 6.27]	2.66 [2.32, 2.99]	2.52 [2.19, 5.84]	3.11 [2.71, 3.51]	5.70 [5.47, 5.92]	5.55 [5.28, 5.82]	5.89 [5.61, 6.16]
Positivity	6.43 [6.28, 6.58]	6.56 [6.44, 6.68]	6.39 [6.24, 6.55]	1.91 [1.69, 2.12]	1.81 [1.60, 2.03]	1.67 [1.49, 1.84]	5.35 [5.02, 5.69]	5.36 [5.04, 5.69]	4.95 [4.54, 5.36]	1.90 [1.68, 2.12]	1.93 [1.68, 2.19]	1.90 [1.62, 2.17]
Specific/ extended/ merged	S: 59% E: 33% M: 8%	S: 70% E: 20% M: 10%	S: 76% E: 18% M: 6%	S: 45% E: 47% M: 8%	S: 54% E: 35% M: 11%	S: 70% E: 22% M: 8%	S: 50% E: 42% M: 5%	S: 57% E: 32% M: 11%	S: 73% E: 19% M: 8%	S: 47% E: 44% M: 9%	S: 61% E: 30% M: 10%	S: 64% E: 28% M: 9%

LIWC Analyses

To examine the objective coherence of subjects' memories, we turned to the descriptions of their events. To attain a more objective measure of memory coherence, we ran subjects' descriptions through Linguistic Inquiry and Word Count—a software program that analyses text and matches words in that text to words in its dictionary fitting different linguistic categories (LIWC; Pennebaker, Booth, Boyd, & Francis, 2015). To prepare the descriptions for LIWC, we first processed them in line with recommendations from the Operator's Manual and cut from the descriptions any content that was not part of the memory, such as messages directed at the researchers. We used four LIWC measures which have been conceptually linked to coherence (Rubin et al., 2016)—insight (e.g. "think," "know"), causation (e.g. "because," "effect"), nonfluencies (e.g. "er," "hm," "um"), and fillers (e.g. "anyway," "dunno"). Descriptions that are coherent should contain a greater percentage of insight and causation words, and a smaller percentage of nonfluencies and fillers, than descriptions that are incoherent.

In Table S3, we display the mean percentage of words in subjects' descriptions fitting each of these four LIWC categories, split by the type of memory subjects were assigned to rate. As the table shows, subjects' descriptions of traumatic memories were similar in objective coherence to descriptions of the non-traumatic memories. It is important to note, however, that on the whole subjects' descriptions included only a small percentage of words related to these four aspects of coherence. Of course, the low rate of nonfluencies and fillers in subjects' descriptions is not surprising given that the descriptions were written rather than spoken.

Table S3

Experiment 1: Percentage of Words Fitting LIWC Coherence Categories for Each Type of Memory Subjects Described

LIWC Category	Positive Memories			Negative Memories			Important Memories			Traumatic Memories		
	M	95% CI	*SD*	*M*	95% CI	*SD*	*M*	95% CI	*SD*	*M*	95% CI	*SD*
Insight	1.84	[1.64, 2.03]	2.07	2.24	[2.05, 2.43]	2.20	2.12	[1.93, 2.31]	2.09	1.85	[1.64, 2.05]	1.67
Causation	1.16	[1.02, 1.30]	1.45	1.39	[1.25, 1.52]	1.60	1.15	[1.01, 1.28]	1.28	1.01	[0.87, 1.16]	1.27
Nonfluencies	0.10	[0.07, 0.14]	0.38	0.08	[0.05, 0.12]	0.28	0.11	[0.07, 0.14]	0.50	0.06	[0.02, 0.10]	0.25
Fillers	0.01	[0.00, 0.01]	0.05	0.01	[0.00, 0.01]	0.07	0.01	[0.00, 0.02]	0.08	0.00	[0.00, 0.01]	0.03

Experiment 2

Number of Subjects per Cell

Table S4

Experiment 2: Number of Subjects per Cell After Exclusions

	Retrieval Task		
PTSD symptoms	Describe then rate	Rate only	Probing then rate
Below Threshold	196	209	191
Above Threshold	100	92	98

AMQ Ratings

Table S5

Experiment 2: AMQ Ratings Broken Down by Cell

	Below threshold			Above threshold		
Item	Describe then rate *M* [95% CI]	Rate only *M* [95% CI]	Probing then rate *M* [95% CI]	Describe then rate *M* [95% CI]	Rate only *M* [95% CI]	Probing then rate *M* [95% CI]
Story	5.21 [4.97, 5.46]	4.29 [4.05, 4.54]	4.38 [4.13, 4.62]	5.37 [5.01, 5.73]	4.28 [3.90, 4.67]	4.47 [4.13, 4.81]
Pieces (R)	5.01 [4.74, 5.28]	4.72 [4.46, 4.98]	3.70 [3.42, 3.97]	4.9 [4.46, 5.34]	4.23 [3.80, 4.66]	3.47 [3.08, 3.86]
Talked	4.14 [3.90, 4.38]	4.15 [3.92, 4.39]	4.49 [4.26, 4.72]	4.31 [3.96, 4.66]	4.20 [3.82, 4.57]	4.06 [3.68, 4.45]
Thought	3.74 [3.49, 4.00]	3.66 [3.42, 3.89]	4.11 [3.87, 4.35]	4.61 [4.23, 4.99]	4.24 [3.86, 4.62]	4.33 [3.93, 4.72]
Out of blue	4.28 [4.02, 4.53]	4.01 [3.77, 4.26]	4.29 [4.05, 4.53]	5.6 [5.29, 5.91]	5.55 [5.26, 6.08]	5.29 [4.97, 5.60]
Intensity	4.96 [4.72, 5.20]	4.44 [4.22, 4.66]	5.20 [5.00, 5.41]	6.11 [5.86, 6.36]	5.82 [5.55, 6.08]	6.01 [5.77, 6.25]
Negativity	6.09 [5.95, 6.23]	5.91 [5.77, 6.06]	6.05 [5.90, 6.19]	6.38 [6.19, 6.57]	6.17 [5.96, 6.39]	6.23 [6.04, 6.43]
Positivity	1.45 [1.32, 1.58]	1.53 [1.39, 1.66]	1.80 [1.62, 1.98]	1.71 [1.43, 1.99]	1.41 [1.23, 1.59]	1.63 [1.33, 1.93]

Cultural Life Script

Fifty two percent of subjects reported a CLS event. Of these events, 24% were "other's death," 10% "parent's death," and 5% "having children." The remaining 9% were distributed across the remaining CLS categories. These findings are in line with those from Experiment 1. There was no evidence to suggest the percentage of subjects who reported a CLS event differed depending on PTSD symptoms; χ^2 (1, N = 886) = 0.34, p = 0.558.

Objective Coherence

Table S6

Experiment 2: Percentage of Words Fitting LIWC Coherence Categories Split by Subjects' PTSD Symptom Group

	Below Threshold			Above Threshold		
LIWC Category	*M*	95% CI	*SD*	*M*	95% CI	*SD*
Insight	2.12	[1.97, 2.27]	1.91	2.15	[1.93, 2.37]	2.15
Causation	0.99	[0.90, 1.10]	1.19	1.10	[0.95, 1.24]	1.24
Nonfluencies	0.06	[0.04, 0.08]	0.25	0.05	[0.03, 0.08]	0.22
Fillers	0.01	[0.01, 0.02]	0.10	0.01	[0.00, 0.01]	0.06

As in Experiment 1, we turn to subjects' descriptions of their events and first examine the extent to which these descriptions contain insight, causation, filler words, and nonfluencies. In Table S6, we display the mean percentage of words in subjects' descriptions fitting each of these four LIWC categories, split by the severity of subjects' reported PTSD symptoms. As the table shows, regardless of their reported PTSD symptoms, subjects' descriptions were similar in objective coherence. As in Experiment 1, however, subjects' descriptions included only a small percentage of words related to these four aspects of coherence.

References

Breslau, N., Reboussin, B., Anthony, J., & Storr, C. (2005). The Structure of Posttraumatic Stress Disorder. *Archives of General Psychiatry, 62*, 1343. doi: 10.1001/archpsyc.62.12.1343

Appendix C: Supplemental Information for Paper 2 (Chapter 3)

Films Included in the Final set of Materials

Table S1

Additional Information About Films Included in the Final set of Materials

Film	Synopsis	Length (min:sec)	Source
Traumatic variant 1	A young woman is texting while driving with two friends in the car. She causes a car accident with multiple fatalities, including a baby, and serious injuries.	2:47	United Kingdom public service announcement (Strange & Takarangi, 2012)
Traumatic variant 2	A pregnant woman goes to the hospital for an ultrasound and the technician is unable to find a heartbeat. The woman then gives birth to her stillborn child and holds the child whilst visibly distressed.	2:52	Television show *Offspring* (Bell & Murphy, 2017)
Negative variant 1	A young boy with a facial disfigurement joins a new school and overhears his peers talking about the way he looks. The boy later confides in his mother about his concern that he will never fit in.	3:04	Motion picture *Wonder* (Beugg & Chbosky, 2017)
Negative variant 2	A mother tells her teenage son about a new cancer treatment she is going to try. He then takes a key from her purse and breaks down in tears after finding that the key opens a storage locker containing gifts from his mother for all the big occasions to come in his life, in case she doesn't survive to be there for those occasions.	3:04	Television show *The Big C* (Hunt & Engler, 2011)
Positive variant 1	An ice skater performs a flawless routine at the 2002 Winter Olympics. She receives her scores, learns she has won the gold medal, and then receives the medal at the awards ceremony.	3:15	2002 Winter Olympics
Positive variant 2	Travellers interact with a digital Santa before their flight takes off, telling him what they want for Christmas. When they arrive at their destination, their Christmas wishes are granted as the gifts come out on the luggage conveyor belt.	3:05	WestJet advertising campaign (WestJet, 2013)
Neutral variant 1	A man talks about the history of parquet floors and describes the process of making parquet floors.	3:00	How It's Made YouTube channel (How It's Made Official, 2018)
Neutral variant 2	A pair of hands demonstrate how to use chopsticks, with an accompanying instructional voice-over.	2:26	YouTube (film has since been removed)

Films Excluded From the Final set of Materials

Over the course of the data collection process, we collected data on 10 additional films, which were excluded from the final set of materials for at least one of the following reasons: [1] subjects' responses suggested the film was different in "storyness" from the rest of the set, [2] subjects' responses suggested the film was not inducing the negative or positive affect as expected, and [3] more than 20% of subjects reported they had previously seen the film. We provide a description of each of these films below, as well as data regarding the films' storyness ratings, induced affect, and the percentage of subjects who reported having seen each film before.

Negative - Boyfriend's Death From Cancer

We collected data from 27 subjects on a negative film that depicted a young man telling his girlfriend his cancer had returned, and her distress upon subsequently learning of his death. This film was taken from the motion picture *The Fault in our Stars* (Bowen, Godfrey, & Boone, 2014). We excluded the data from one subject for providing a nonsensical or inaccurate description of the film.

After watching this film, subjects reported experiencing a reduction in positive affect and an increase in negative affect ($M_{positive}$ = -4.12, 95% CI [-2.32, -5.92]; $M_{negative}$ = 2.19 [0.44, 3.94]). The film was excluded because storyness ratings were lower than the films included in the final set (M = 5.45 [4.73, 6.18]), and 31% of subjects reported having seen the film before.

Neutral - Baking Bread

We collected data from 23 subjects on a neutral film that guided people through a step-by-step process of baking bread (Sim, 2013). We excluded the data from three subjects—one who failed the attention check, one who provided a nonsensical or

inaccurate description of the film, and one who reported watching the film more than once.

After watching this film, subjects reported slight reductions in both positive and negative affect ($M_{positive}$ = -1.35, 95% CI [-3.85, 1.15]; $M_{negative}$ = -1.20 [-2.31, -0.09]). None of the subjects reported having seen the film before. The film was excluded because storyness ratings were higher than the films included in the final set (M = 6.64 [6.28, 7.00]).

Traumatic - Car Crash

We collected data on three additional iterations of the car crash film included in the final set of materials (Traumatic variant 1), in order to create a version similar on "storyness" to the rest of the films.

Extended Full Beginning and end

We collected data from 109 subjects on an extended version of the film that contained additional material at both the beginning and end. This film showed the driver texting and joking around with her friends before the crash, and provided more information about the cause of the crash and what happened in the aftermath of the crash. For example, instead of the film ending with an aerial shot from a rescue helicopter, subjects saw the helicopter land and load the driver of one of the cars into the helicopter to be flown to hospital. We excluded the data from seven subjects—one who failed the attention check, one who provided a nonsensical or inaccurate description of the film, two who reported failing to complete the study in a single session, and three who reported watching the film more than once.

After watching this film, subjects reported a reduction in positive affect and an increase in negative affect ($M_{positive}$ = -6.37, 95% CI [-7.88, -4.86]; $M_{negative}$ = 9.91 [8.32, 11.50]). Only 4% of subjects reported having seen the film before. The film was excluded

because storyness ratings were higher than the films included in the final set (M = 6.65 [6.54, 6.76]).

Extended Full Beginning

We collected data from 20 subjects on a version of the car crash film that contained only the additional material from the beginning of the film mentioned in the version above. We excluded the data from three subjects—two provided nonsensical or inaccurate descriptions of the film, one reported watching the film more than once, and one reported not being able to hear the audio in the film (some of the subjects were excluded on the basis of more than one of the above criteria).

After watching this film, subjects reported a reduction in positive affect and an increase in negative affect ($M_{positive}$ = -5.71, 95% CI [-9.47, -1.94]; $M_{negative}$ = 8.41 [4.78, 12.04]). Only 6% of subjects reported having seen the film before. The film was excluded because storyness ratings were higher than the films included in the final set (M = 6.92 [6.80, 7.04]).

Extended Partial Beginning

We collected data from 29 subjects on a version of the car crash film that was identical to the version used in final set, but had part of the beginning from the versions above added on, providing slightly more context about what was happening in the car prior to the crash. We excluded data from one subject who reported watching the film more than once.

After watching this film, subjects reported a reduction in positive affect and an increase in negative affect ($M_{positive}$ = -3.86, 95% CI [-7.15, -0.56]; $M_{negative}$ = 10.00 [6.61, 13.39]). Only 7% of subjects reported having seen the film before. As with the other versions of the car crash film, the film was excluded because storyness ratings were higher than the films included in the final set (M = 6.69 [6.48, 6.89]).

Traumatic - American Racial Violence

We collected data from 21 subjects on a traumatic film that depicted a white man discovering a black man attempting to break into his car—he then violently beats the robber before stomping on his head. This film was taken from the motion picture *American History X* (Morrissey & Kaye, 1997). We excluded the data from one subject who provided a nonsensical or inaccurate description of the film and reported watching the film more than once.

After watching this film, subjects reported a reduction in positive affect and an increase in negative affect ($M_{positive}$ = -6.70, 95% CI [-11.32, -2.08]; $M_{negative}$ = 10.25 [5.93, 14.57]). Subjects rated the film as fairly high on storyness (M = 6.18 [5.73, 6.63]). The film was excluded because 40% of subjects reported having seen the film before.

Traumatic - Boxing Death

We collected data from 35 subjects on a traumatic film that depicted a young boy witnessing his father die in the locker room after a boxing match. This film was taken from the motion picture *The Champ* (Lovell & Zeffirelli, 1979). We excluded the data from six subjects—one failed the attention check, three provided nonsensical or inaccurate descriptions of the film, one reported failing to complete the study in a single session, and five reported watching the film more than once (some of the subjects were excluded on the basis of more than one of the above criteria).

The film was excluded for three reasons. First, storyness ratings were lower than the films included in the final set (M = 5.37, 95% CI [4.80, 5.93]). Second, after watching this film, subjects reported a reduction in positive affect, but only a slight increase in negative affect ($M_{positive}$ = -7.07, 95% CI [-10.25, -3.89]; $M_{negative}$ = 2.41 [0.90, 3.93]). Third, 21% of subjects reported having seen the film before.

Traumatic - Domestic Violence

We collected data from 48 subjects on a traumatic film that depicted a man violently beating his wife at a party. This film was taken from the motion picture *Once Were Warriors* (Scholes & Tamahori, 1994). We excluded the data from eight subjects—one failed the attention check, seven provided nonsensical or inaccurate descriptions of the film, one reported watching the film more than once, and one reported not being able to hear the audio in the film (some of the subjects were excluded on the basis of more than one of the above criteria).

After watching this film, subjects reported a reduction in positive affect and an increase in negative affect ($M_{positive}$ = -6.95, 95% CI [-9.69, -4.21]; $M_{negative}$ = 10.63 [8.19, 13.06]). Only 3% of subjects reported having seen the film before. The film was excluded because storyness ratings were lower than the films included in the final set (M = 4.91 [4.51, 5.31]).

Traumatic - Australian Racial Violence

We collected data from 22 subjects on a traumatic film that depicted a group of white men attacking and beating a group of Asian men. This film was taken from the motion picture *Romper Stomper* (Pringle, Scharf, & Wright, 1992).

After watching this film, subjects reported a reduction in positive affect and an increase in negative affect ($M_{positive}$ = -7.31, 95% CI [-11.40, -3.24]; $M_{negative}$ = 8.59 [4.53, 12.65]). Only 5% of subjects reported having seen the film before. The film was excluded because storyness ratings were lower than the films included in the final set (M = 4.26 [3.62, 4.91]).

Traumatic - Miscarriage

We collected data from 25 subjects on a traumatic film that depicted a young woman experiencing a miscarriage. This film was taken from the motion picture *The*

Light Between Oceans (Heyman, Clifford, & Cianfrance, 2016). We excluded data from five subjects—one who failed the attention check, three who provided nonsensical or inaccurate descriptions of the film, and one who reported watching the film more than once.

Only 5% of subjects reported having watched the film before. The film was excluded for two reasons. First, after watching this film, subjects reported a reduction in positive affect, but only a slight increase in negative affect ($M_{positive}$ = -4.85, 95% CI [-7.51, -2.19]; $M_{negative}$ = 3.20 [1.52, 4.88]). Second, subjects' storyness ratings were lower than the films included in the final set (M = 5.53 [4.77, 6.29]).

Ratings of Individual Items Comprising the Storyness Sum Variable

Table S2

Subjects' Ratings of Individual Items Comprising the Storyness Sum Variable

	Traumatic		Negative		Positive		Neutral	
Item	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]
Clarity	6.17 [5.97, 6.37]	6.35 [6.18, 6.53]	6.50 [6.37, 6.63]	6.19 [5.99, 6.38]	6.38 [6.20, 6.56]	6.42 [6.24, 6.60]	6.21 [6.04, 6.38]	6.59 [6.47,6.71]
Cohesiveness	6.13 [5.96, 6.30]	6.12 [5.92, 6.32]	6.06 [5.85, 6.26]	6.06 [5.86, 6.26]	6.27 [6.11, 6.43]	6.38 [6.21, 6.54]	6.19 [6.00, 6.37]	6.38 [6.21, 6.56]
Coherence	6.19 [6.02, 6.36]	6.26 [6.08, 6.45]	6.37 [6.21, 6.52]	6.13 [5.94, 6.32]	6.33 [6.15, 6.50]	6.42 [6.26, 6.58]	6.25 [6.08, 6.42]	6.55 [6.43, 6.68]
Understanding	6.26 [6.08, 6.44]	6.47 [6.29, 6.65]	6.61 [6.49, 6.73]	6.31 [6.14, 6.49]	6.52 [6.38, 6.66]	6.38 [6.20, 6.57]	5.96 [5.78, 6.15]	6.55 [6.43, 6.68]
Comprehensibility	6.26 [6.07, 6.46]	6.79 [6.71, 6.88]	6.36 [6.18, 6.54]	6.28 [6.09, 6.48]	6.45 [6.28, 6.62]	6.49 [6.32, 6.65]	6.13 [5.94, 6.31]	6.59 [6.47, 6.71]

Additional Ratings About Final set of Films

In the final phase of the experiment, we asked subjects some additional questions about the film they had watched. We report subjects' responses to those questions in Table S3. As the table shows, subjects reported finding the traumatic and negative films more traumatic (and less pleasant) than the positive and neutral films. Subjects reported being moderately likely to think about all of the films over the next few days. Subjects

reported having fairly vivid memories for the films, and having paid a great degree of attention to the films when they watched them. The films were rated as moderately personally relevant, and the extent to which subjects reported recognizing where the film came from varied between films.

Table S3

Subjects' Responses to Additional Questions About the Films

	Traumatic		Negative		Positive		Neutral	
Question	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]
How traumatic did you find the film? [a]	6.03 [5.80, 6.23]	5.10 [4.79, 5.42]	4.32 [4.01, 4.63]	4.51 [4.22, 4.81]	1.24 [1.08, 1.40]	1.24 [1.09, 1.38]	1.13 [1.02, 1.23]	1.14 [1.04, 1.24]
How pleasant did you find the film? [a]	1.31 [1.13. 1.48]	1.48 [1.32, 1.63]	2.78 [2.41, 2.94]	2.71 [2.43, 2.99]	6.05 [5.82, 6.28]	6.28 [6.06, 6.50]	5.36 [5.10, 5.62]	5.04 [4.77, 5.32]
How likely are you to think about the film over the next few days? [a]	4.72 [4.42, 5.01]	3.88 [3.56, 4.20]	4.08 [3.79, 4.37]	4.68 [4.38, 4.98]	3.31 [3.00, 3.62]	4.43 [4.11, 4.76]	3.04 [2.75, 3.32]	3.43 [3.13, 3.73]
How vivid and clear is your memory for the film? [b]	5.85 [5.67, 6.03]	6.07 [5.89, 6.24]	6.01 [5.83, 6.19]	6.16 [6.00, 6.33]	5.93 [5.75, 6.12]	6.01 [5.86, 6.17]	5.27 [5.04, 5.49]	5.86 [5.68, 6.05]
Please indicate how much attention you paid to the film you have just seen. [c]	6.86 [6.79, 6.94]	6.79 [6.71, 6.88]	6.78 [6.69, 6.87]	6.81 [6.71, 6.90]	6.71 [6.59, 6.82]	6.86 [6.80, 6..92]	6.55 [6.41, 6.69]	6.64 [6.51, 6.77]
Please indicate how personally relevant you found the film. [a]	4.61 [4.31, 4.90]	3.62 [3.30, 3.94]	4.35 [4.05, 4.64]	4.40 [4.08, 4.73]	3.30 [3.01, 3.58]	4.33 [4.03, 4.62]	3.11 [2.81, 3.41]	4.32 [4.02, 4.62]
Do you recognize where the film came from? [d]	Y: 5% N: 95%	Y: 0% N: 100%	Y: 34% N: 66%	Y: 4% N: 96%	Y: 61% N: 39%	Y: 10% N: 90%	Y: 16% N: 84%	Y: 1% N: 99%

Note. [a] 1 = *not at all*, 7 = *extremely*; [b] 1 = *not at all*, 7 = *completely*; [c] 1 = *none at all*, 7 = *total attention*; [d] *yes/no.*

Pre-Test PANAS Ratings

Table S4

Subjects' Pre-Test PANAS Ratings

	Traumatic		Negative		Positive		Neutral	
Subscale	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]
Positive	30.28 [28.69, 31.88]	31.01 [29.33, 32.69]	29.14 [27.76, 30.52]	30.21 [28.74, 31.68]	29.97 [28.34, 31.59]	28.56 [26.99, 30.11]	29.81 [28.19, 31.44]	29.81 [28.30, 31.31]
Negative	13.59 [12.47, 14.71]	12.55 [11.72, 12.55]	12.51 [11.73, 13.29]	12.59 [11.76, 13.41]	12.54 [11.76, 13.32]	11.91 [11.27, 12.55]	12.19 [11.54, 12.83]	12.64 [11.88, 13.40]

Reported Change in Individual PANAS Items

Table S5

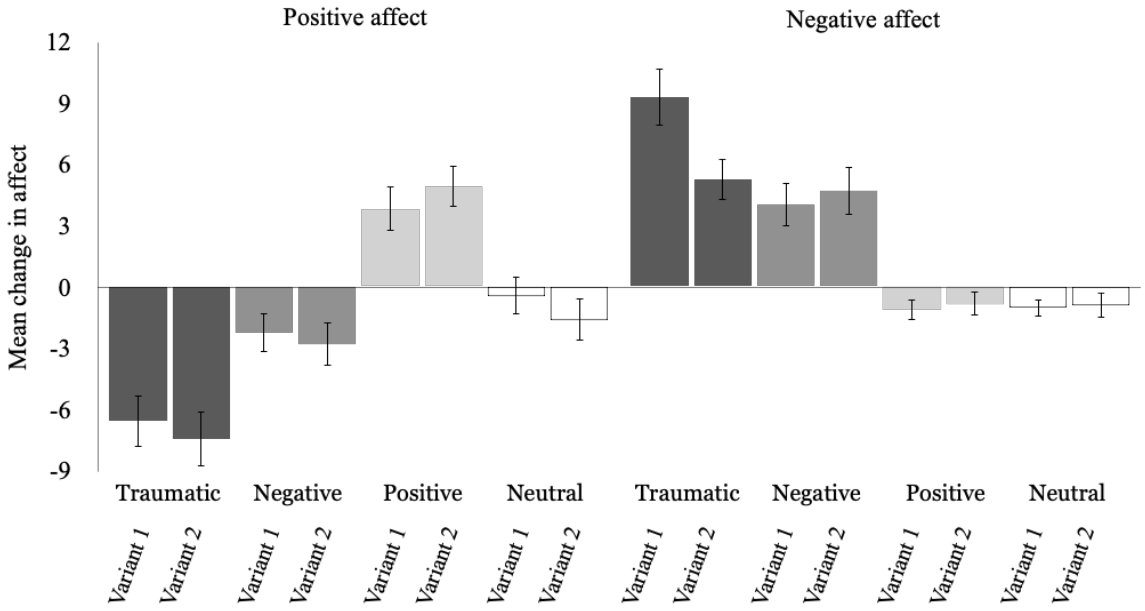
Subjects' Reported Change in Individual PANAS Items

	Traumatic		Negative		Positive		Neutral	
Item	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]	Variant 1 *M* [95% CI]	Variant 2 *M* [95% CI]
Afraid	1.13 [0.91, 1.36]	0.40 [0.23, 0.56]	0.19 [0.07, 0.32]	0.58 [0.39, 0.77]	-0.14 [-0.22, -0.06]	-0.09 [-0.16, -0.01]	-0.05 [-0.11, 0.01]	-0.09 [-0.18, 0.01]
Scared	1.26 [1.03, 1.49]	0.51 [0.34, 0.68]	0.11 [-0.04, 0.25]	0.51 [0.33, 0.69]	-0.07 [-0.14, 0.01]	-0.06 [-0.15, 0.03]	-0.05 [-0.11, 0.00]	-0.08 [-0.15, -0.01]
Nervous	0.97 [0.75, 1.19]	0.21 [0.04, 0.39]	0.06 [-0.08, 0.20]	0.27 [0.10, 0.44]	-0.15 [-0.25, -0.05]	-0.22 [-0.32, -0.11]	-0.22 [-0.32, -0.13]	-0.25 [-0.36, -0.14]
Jittery	0.91 [0.68, 1.13]	0.31 [0.17, 0.44]	0.01 [-0.11, 0.13]	0.22 [0.07, 0.38]	-0.13 [-0.24, -0.01]	-0.04 [-0.11, 0.04]	-0.12 [-0.21, -0.03]	-0.11 [-0.21, -0.01]
Guilty	0.31 [0.12, 0.49]	0.26 [0.12, 0.41]	0.35 [0.21, 0.48]	0.40 [0.22, 0.57]	-0.07 [-0.16, 0.02]	-0.09 [-0.18, 0.01]	-0.02 [-0.07, 0.03]	-0.07 [-0.16, 0.02]
Ashamed	0.26 [0.12, 0.39]	0.13 [0.01, 0.24]	0.54 [0.36, 0.72]	0.26 [0.14, 0.39]	-0.09 [-0.16, -0.02]	-0.04 [-0.09, 0.02]	-0.06 [-0.13, 0.01]	-0.07 [-0.15, 0.01]
Irritable	0.36 [0.18, 0.53]	0.10 [-0.07, 0.26]	0.37 [0.19, 0.55]	-0.02 [-0.19, 0.15]	-0.2 [-0.32, -0.08]	-0.22 [-0.34, -0.10]	-0.16 [-0.23, -0.08]	-0.04 [-0.17, 0.09]
Hostile	0.33 [0.18, 0.47]	0.22 [0.09, 0.34]	0.30 [0.17, 0.44]	0.10 [0.00, 0.21]	-0.05 [-0.10, 0.00]	0.04 [-0.05, 0.12]	-0.06 [-0.14, 0.02]	0.04 [-0.02, 0.12]
Upset	1.93 [1.71, 2.14]	1.71 [1.49, 1.95]	1.28 [1.07, 1.49]	1.37 [1.14, 1.61]	-0.08 [-0.15, -0.02]	-0.05 [-0.16, 0.05]	-0.14 [-0.22, -0.06]	-0.09 [-0.20, 0.01]
Distressed	1.87 [1.61, 2.12]	1.41 [1.21, 1.62]	0.87 [0.64, 1.10]	1.02 [0.80, 1.25]	-0.13 [-0.21, -0.04]	-0.04 [-0.15, 0.08]	-0.14 [-0.23, -0.05]	-0.12 [-0.24, 0.00]
Active	-0.46 [-0.67, -0.25]	-0.66 [-0.85, -0.47]	-0.22 [-0.41, -0.03]	-0.34 [-0.53, -0.15]	0.26 [0.10, 0.42]	0.27 [0.10, 0.45]	-0.07 [-0.20, 0.07]	-0.05 [-0.19, 0.09]
Alert	0.03 [-0.14, 0.20]	-0.32 [-0.52, -0.11]	-0.04 [-0.19, 0.10]	-0.10 [-0.26, 0.05]	0.01 [-0.13, 0.16]	0.02 [-0.13, 0.18]	-0.18 [-0.34, -0.02]	-0.31 [-0.47, -0.15]
Attentive	-0.10 [-0.26, 0.05]	-0.37 [-0.55, -0.19]	-0.14 [-0.31, 0.02]	-0.08 [-0.21, 0.05]	-0.01 [-0.15, 0.12]	0.14 [-0.01, 0.30]	-0.16 [-0.31, -0.01]	-0.42 [-0.59, -0.24]
Enthusiastic	-1.28 [-1.49, -1.08]	-1.20 [-1.41, -0.99]	-0.43 [-0.61, -0.25]	-0.63 [-0.84, -0.42]	0.69 [0.49, 0.89]	0.85 [0.66, 1.04]	0.03 [-0.14, 0.20]	-0.22 [-0.40, -0.04]
Excited	-0.73 [-0.97, -0.49]	-0.77 [-0.98, -0.57]	-0.35 [-0.53, -0.16]	-0.48 [-0.67, -0.29]	0.86 [0.66, 1.06]	1.11 [0.91, 1.30]	0.10 [-0.05, 0.26]	0.00 [-0.16, 0.16]
Inspired	-0.92 [-1.12, -0.72]	-0.59 [-0.83, -0.35]	0.08 [-0.12, 0.28]	0.35 [0.11, 0.59]	1.04 [0.83, 1.26]	1.40 [1.17, 1.62]	0.46 [0.28, 0.64]	0.22 [0.03, 0.41]
Interested	-0.58 [-0.79, -0.37]	-0.83 [-1.03, -0.61]	-0.09 [-0.26, 0.07]	-0.19 [-0.37, -0.02]	0.09 [-0.06, 0.24]	0.40 [0.23, 0.56]	0.09 [-0.09, 0.28]	-0.30 [-0.50, -0.10]
Proud	-0.82 [-1.01, -0.63]	-0.96 [-1.16, -0.77]	-0.42 [-0.58, -0.27]	-0.31 [-0.48, -0.13]	0.84 [0.63, 1.06]	0.69 [0.50, 0.88]	-0.09 [-0.23, 0.05]	-0.04 [-0.21, 0.13]
Strong	-0.67 [-0.86, -0.48]	-0.74 [-0.95, -0.52]	-0.26 [-0.40, -0.11]	-0.44 [-0.61, -0.27]	0.10 [-0.09, 0.28]	0.13 [-0.01, 0.27]	-0.25 [-0.40, -0.11]	-0.17 [-0.31, -0.02]
Determined	-1.01 [-1.23, -0.80]	-1.01 [-1.23, -0.80]	-0.35 [-0.53, -0.17]	-0.54 [-0.73, -0.36]	-0.03 [-0.17, 0.11]	-0.06 [-0.23, 0.10]	-0.34 [-0.51, -0.17]	-0.30 [-0.48, -0.13]

Changes in Positive and Negative Affect Split by Variant of Film Type

Figure S1

Subjects' Mean Change in Positive and Negative Affect

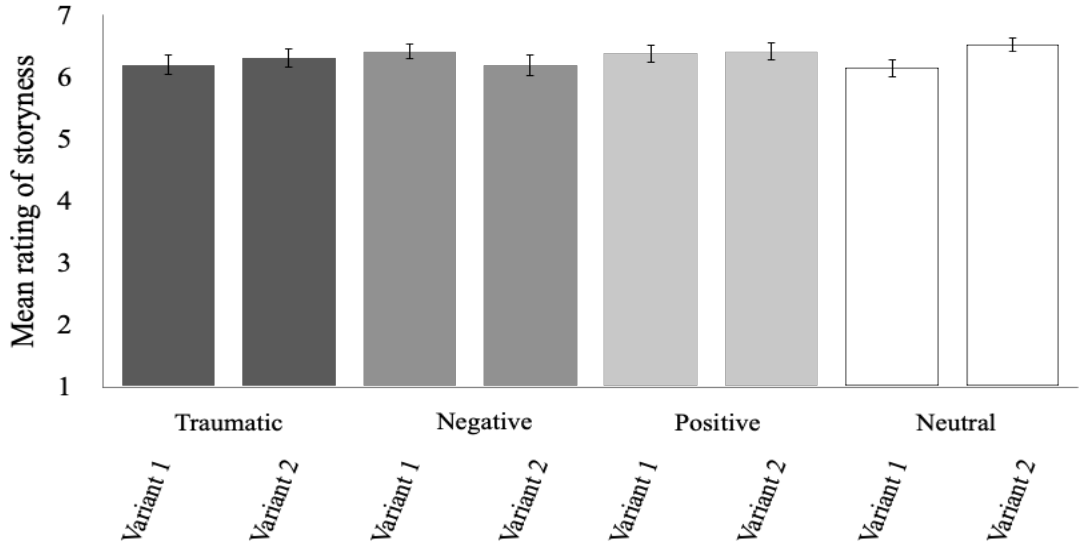


Note. Error bars represent 95% confidence intervals of cell means.

Storyness Ratings Split by Variant of Film Type

Figure S2

Subjects' Mean Ratings of Storyness



Note. Error bars represent 95% confidence intervals of cell means.

We conducted equivalence tests on subjects' ratings of the storyness sum-variable to determine the degree to which the events depicted by each of our comparison films can be considered equivalent to our traumatic films in terms of storyness. We set our smallest raw effect size of interest at 0.5. We display the results of these equivalence tests in Table S6. As the table shows, all equivalence tests were significant, with the exception of one. These data suggest that subjects found it equally easy to follow the vast majority of the events depicted by the films. These data also suggest that there is not enough evidence to reject the possibility that the second neutral film (demonstrating how to use chopsticks) depicts an event that is easier to follow than does the first traumatic film (a graphic car accident). But the magnitude of the difference in this variable between these two films is small, and both of these films are equivalent to the other films in the set. Furthermore, as reported in the paper, when we collapse across the variants of each film type, as researchers typically would when counterbalancing the films, this difference disappears.

Table S6

Equivalence Tests on Subjects' Ratings of Storyness

	Traumatic variant 1			Traumatic variant 2		
	M_{diff}	$p\ M_{diff} \leq 0.5$	$p\ M_{diff} \geq 0.5$	M_{diff}	$p\ M_{diff} \leq 0.5$	$p\ M_{diff} \geq 0.5$
Negative variant 1	-0.22	<.001	0.0041	-0.11	<.001	<.001
Negative variant 2	0.01	<.001	<.001	0.12	<.001	<.001
Positive variant 1	-0.19	<.001	0.0017	-0.07	<.001	<.001
Positive variant 2	-0.21	<.001	0.0038	-0.10	<.001	<.001
Neutral variant 1	0.06	<.001	<.001	0.17	0.001	<.001
Neutral variant 2	-0.33	<.001	0.0566	-0.23	<.001	0.0045

Note: M_{diff} = traumatic - comparison

Previous Exposure to Each Variant of Film Type

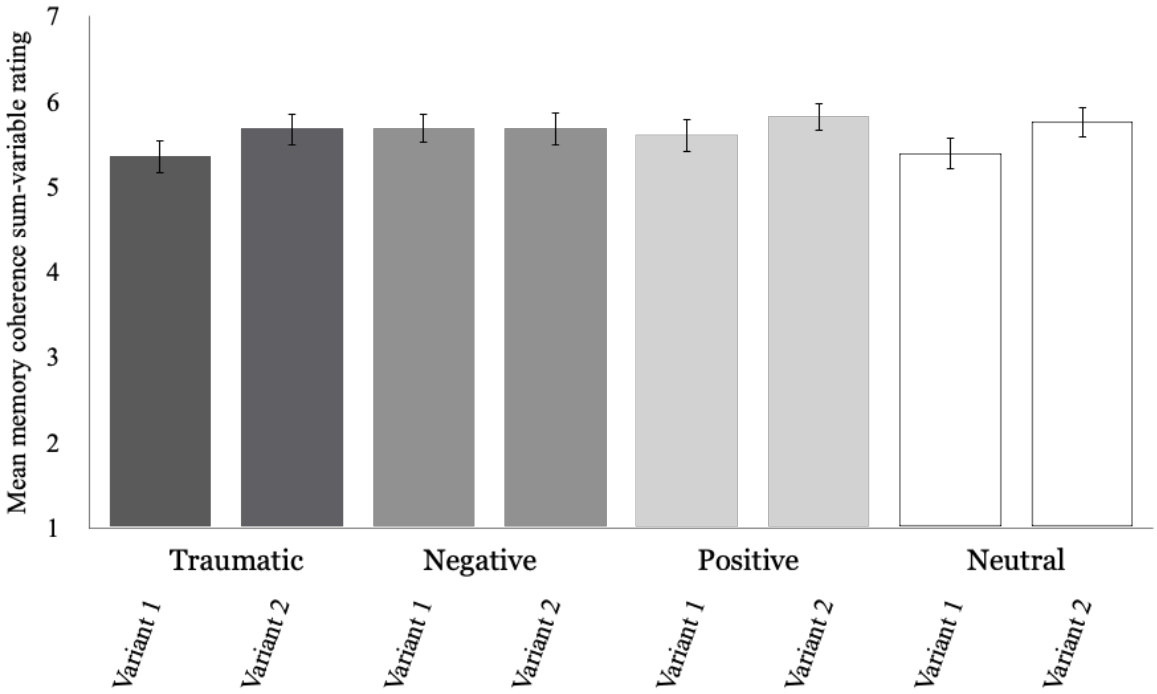
The percentage of subjects who reported having seen each of the films was: "Traumatic variant 1" = 8%, "Traumatic variant 2" = 0%, "Negative variant 1" = 12%,

"Negative variant 2" = 1%, "Positive variant 1" = 7%, "Positive variant 2" = 2%, "Neutral variant 1" = 4%, "Neutral variant 2" = 1%. The film with the highest rate of previous exposure was "Negative variant 1," which is not surprising considering that film clip was taken from a motion picture released just 2 years prior to data collection (Wonder; Beugg & Chbosky, 2017).

Memory Coherence Split by Variant of Film Type

Figure S3

Subjects' Mean Ratings of Memory Coherence

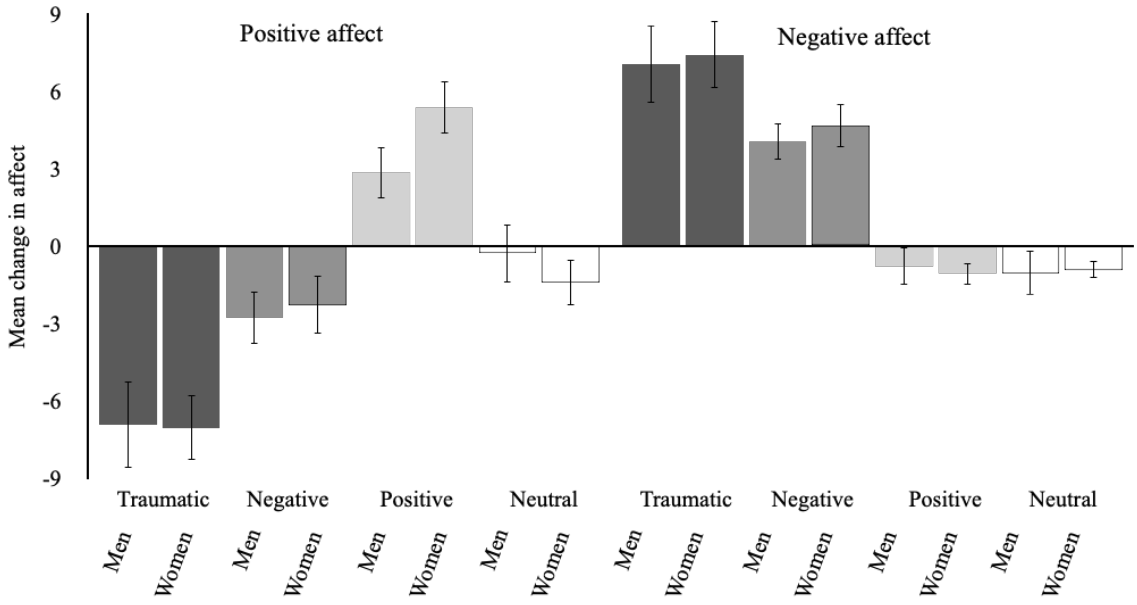


Note. Error bars represent 95% confidence intervals of cell means.

Changes in Positive and Negative Affect Split by Men and Women

Figure S4

Subjects' Mean Change in Positive and Negative Affect Classified According to Gender

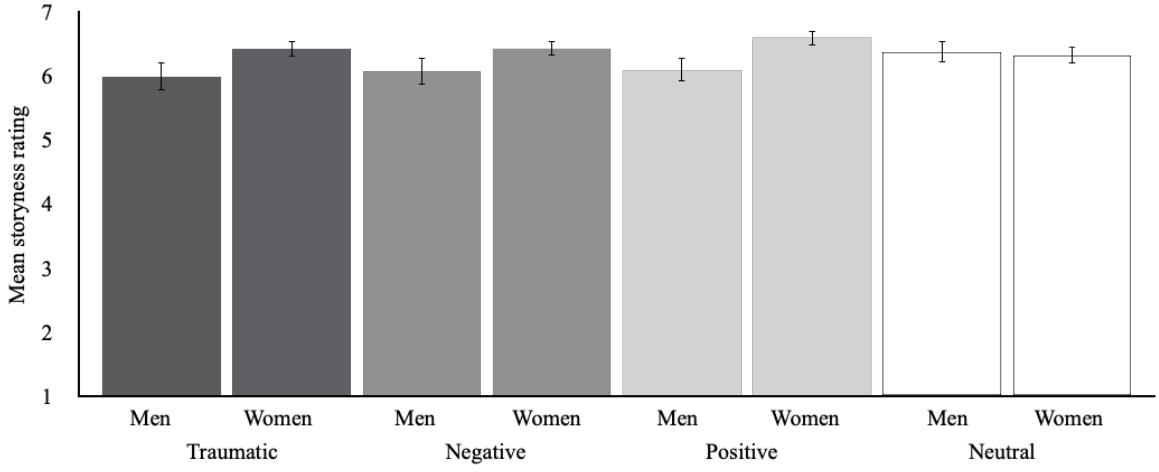


Note. Error bars represent 95% confidence intervals of cell means.

Storyness Ratings Split by Men and Women

Figure S5

Subjects' Mean Ratings of Affect Classified According to Gender

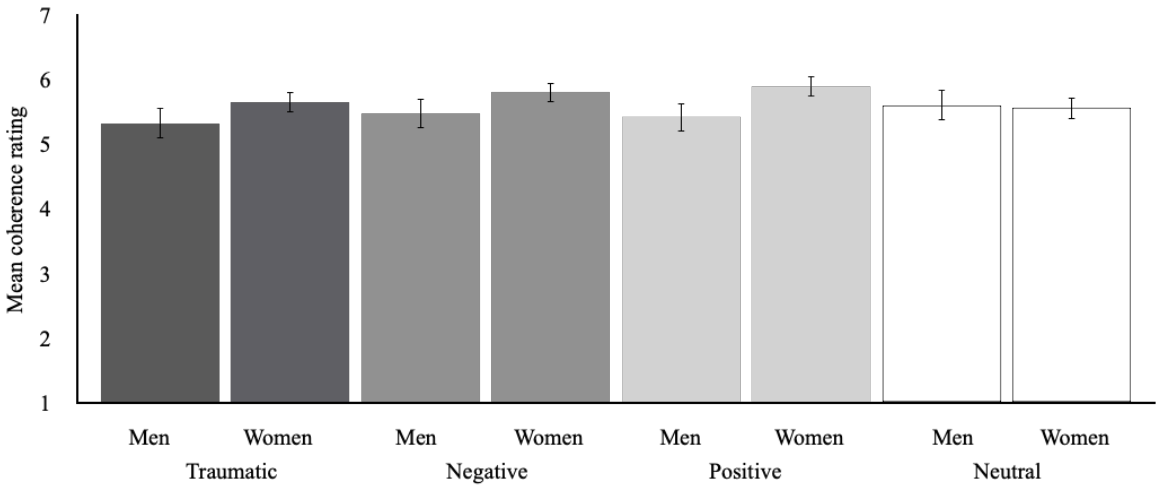


Note. Error bars represent 95% confidence intervals of cell means.

Storyness Ratings Split by Men and Women

Figure S6

Subjects' Mean Ratings of Memory Coherence Classified According to Gender



Note. Error bars represent 95% confidence intervals of cell means.

Experiment 2 Method

The a priori aim of this experiment was to examine how coherent people's memories for trauma analog films are after a 5-minute delay versus a 48-hour delay. The pre-registration for the experiment is available here: https://osf.io/n2jwe

Subjects

We recruited subjects on Amazon's Mechanical Turk (MTurk) using the CloudResearch platform. Based on the margin of error in coherence ratings attained in our previous datasets, we set our target sample size at 50 subjects for each of the four films, totalling a useable sample size (after exclusions) of 200. We collected data from a total of 246 MTurk workers, and ultimately retained 229 subjects for analysis (M_{age} = 40.70, *SD* = 12.35; 63% women, 37% men; 93% reported English was their first language). Subjects received $1.30 US for participating in the experiment ($0.30 for the first session and $1 upon completion of the second session).

Design

We used a between-subjects design: first rating delay had two levels (5m, 48h) and film type had two levels (traumatic, positive).

Procedure

This experiment was approved by the University of Otago Human Ethics Committee, with reciprocal approval at the University of Waikato. As in Experiment 1, subjects were warned about the content of the traumatic films and asked not to participate if they thought they might be adversely affected. This experiment consisted of two sessions, separated by approximately 48h (M = 49.61h, SD = 6.69, Med = 47.8).

Session 1

Session 1 consisted of three separate phases.

Phase 1. Subjects completed the same soundcheck detailed in Experiment 1, and were then randomly assigned to watch either a traumatic or a positive film. We counterbalanced which variant of film type subjects watched, such that half of the subjects watched the first variant of their assigned film type and half watched the second variant.

Phase 2. After completing a Sudoku for five minutes, half of the subjects (those who were randomly assigned to make their first rating after a delay of 5 minutes) made ratings about the coherence of their memories on four items: the *story* item ("as I remember the event, my memory comes to me as a coherent story"), the *pieces* item ("as I remember the event, my memory comes to me in pieces, with bits missing"), the *fragments* item ("as I remember the event, my memory comes to me as a collection of disconnected fragments"), and the *jumbled* item ("as I remember the event, my memory feels jumbled, with parts of the event coming to me out of order"). All items were rated on a 7-point Likert-type scale from 1 (*not at all*) to 7 (*completely*). The story and pieces

items were adapted from the Autobiographical Memory Questionnaire (AMQ; Rubin, Schrauf, & Greenberg, 2003), and the fragments and jumbled items were piloted in this experiment to capture to a broader range of aspects of coherence. We created a memory coherence sum variable by reverse coding the pieces, fragments, and jumbled items, and calculating subjects' mean rating of the four items. The sum variable had a Cronbach's α of 0.87.

Phase 3. Next, all subjects answered additional questions about the film, such as how personally relevant it was and whether they had seen the film before, completed a short questionnaire about the extent to which they complied with our instructions during the session, and provided basic demographic information. Finally, we reminded subjects that we would catch up with them in two days' time for the second session of this study, and that they should make sure they are checking their emails because they need to complete the second session within 24h of receiving the link.

Session 2

The second session of the study consisted of three phases.

Phase 1. First, we presented subjects with a still of the film they watched in Session 1 and asked them if they remembered this film (*yes/no*). The survey was programmed such that subjects who said "no" were sent to the end of the survey and debriefed, but this situation never arose.

Phase 2. Next, all subjects made ratings about the coherence of their memories, using the same items as in Session 1. It is this coherence rating that is reported in the paper in the Results of Experiment 2. After making coherence ratings, subjects made ratings about the extent to which they had rehearsed their memories of the film since Session 1, and the valence and intensity of their memories, using 6 AMQ items. Then, subjects completed a version of the revised Impact of Event Scale (IES-R; Weiss &

Marmar, 1997) adapted for use after a two-day delay. Finally, subjects were asked to provide a free recall description of their memory for the film.

Phase 3. At the end of the session, subjects completed a short questionnaire about the extent to which they complied with our instructions during the session and were then fully debriefed.

Experiment 2 Findings

Coherence Ratings After 5 Minutes vs. 48 Hours

Subjects rated their memories as fairly coherence regardless of whether their first rating was made 5 minutes after watching their assigned film (M = 5.61, 95% CI [5.39, 5.83]) or 48 hours after watching their assigned film (M = 5.05, 95% CI [4.81, 5.29]). Subjects who made coherence ratings both 5 minutes and 48 hours after watching their assigned film rated their memories as more coherent after 5 minutes than they did after 48 hours (M_{diff} = -0.89, 95% CI [-1.14, -0.65]).

Coherence Ratings Split by Variant of Film Type

Regardless of whether they watched the first or second variant of their assigned film type, subjects rated their memories as fairly coherent after both 5 minutes and 48 hours. Positive films: $M_{V1(5m)}$ = 5.51, 95% CI [5.04, 5.98], $M_{V2(5m)}$ = 5.54, 95% CI [5.12, 5.96], $M_{V1(48h)}$ = 4.85, 95% CI [4.43, 5.27], $M_{V2(48h)}$ = 4.88, 95% CI [4.52, 5.24]. Traumatic films: $M_{V1(5m)}$ = 5.72, 95% CI [5.30, 6.14], $M_{V2\ (5m)}$ = 5.68, 95% CI [5.16, 6.20], $M_{V1(48h)}$ = 5.28, 95% CI [4.98, 5.58], $M_{V2(48h)}$ = 4.52, 95% CI [4.16, 4.89].

Word Count of Free Recall Descriptions

Subjects' descriptions of the video contained a similar number of words, regardless of whether they completed their first coherence rating after 5 minutes or 48 hours. Positive films: $M_{(5m)}$ = 90.28, 95% CI [78.26, 102.30], $M_{(48h)}$ = 76.49, 95% CI

[64.68, 88.30]. Traumatic films: $M_{(5m)}$ = 100.455, 95% CI [82.38, 118.53], $M_{(48h)}$ = 110.36, 95% CI [92.76, 127.96].

Subjects used more words to describe the traumatic films than they did the positive films ($M_{traumatic}$ = 105.54, 95% CI [94.85, 116.23], $M_{positive}$ = 83.27, 95% CI [72.71, 93.82], M_{diff} = 22.28, 95% CI [7.24, 37.30]).

IES-R Scores

Subjects who watched a traumatic film reported more symptoms of distress than did those who watched a positive film ($M_{traumatic}$ = 11.70, 95% CI [9.78, 13.62], $M_{positive}$ = 4.47, 95% CI [2.57, 6.36], M_{diff} = 7.23, 95% CI [4.54, 9.93]).

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Appendix D: Pilot Studies for Paper 3 (Chapter 4)

Before conducting the experiment-proper, we first conducted two pilot studies. We ran the first pilot study to examine the degree to which people's autobiographical memories would cohere over repeated recall. We ran the second pilot study to establish the extent to which rating symptoms of posttraumatic stress about one memory affects subsequent ratings about another memory. Both studies informed the method of the experiment proper.

Pilot Study 1: Coherence of Autobiographical Memories Over Repeated Recall

Method

Subjects

We recruited subjects on Amazon's Mechanical Turk (MTurk) using the TurkPrime platform (https://www.turkprime.com/; Litman et al., 2017). A total of 184 MTurk workers completed the first session of the experiment. Of those subjects, 146 completed the second session, and 141 completed the third session. After exclusions (see Results), we retained the data from 94 subjects for analyses (M_{age} = 36.77 years, SD = 11.42, range = 19-67; 36% men, 62% women, 2% gender diverse). Subjects received $5US total for participating in the study, with payment split across the experimental sessions.

Design

We manipulated Type of Memory (positive, negative, important) between subjects.

Procedure

This experiment was approved by the University of Waikato's School of Psychology Research and Ethics Committee under delegated authority of the University's Human Research Ethics Committee. The experiment was conducted in accordance with

the ethical provisions of the World Medical Association Declaration of Helsinki. The experiment consisted of three sessions, administered online using Qualtrics (https://www.qualtrics.com/).

Session 1. Session 1 consisted of four phases.

Phase 1. First, subjects were randomly assigned to bring to mind the memory of either their most positive, most negative, or most important event. More specifically, one third of subjects were asked to bring to mind "the memory that most makes you feel negative when you think of it today," one third "the memory that most makes you feel positive when you think of it today," and the final third "the memory that feels like your most important life event when you think of it today." After bringing this memory to mind, subjects were asked to "report everything that you can remember about this event. Make sure to leave nothing out. Start at the beginning and give a complete description of the entire event."

Phase 2. Next, we asked subjects to answer a series of seven specific questions about the event in as much detail as possible. For example, "What were you doing just before?" and "How did you feel?"

Phase 3. Subjects then completed a 15-item version of the Autobiographical Memory Questionnaire (AMQ; Rubin et al., 2003, 2004). Within this questionnaire were two items designed to measure the coherence of people's memories prior to recall—the *story* item ("As I remember the event, my memory comes to me in words or pictures as a coherent story or episode and not as an isolated fact, observation, or scene") and the *pieces* item ("This memory comes in pieces with missing bits"). Both items were rated on a 7-point Likert-type scale (*1 = not at all, 7 = completely)*. We created a coherence sum variable by reverse coding the pieces item and taking the mean of subjects' ratings on these two items.

Next, to measure subjects' current symptoms of distress about their nominated events, we asked them to complete the revised Impact of Event Scale (IES-R; Weiss & Marmar, 1997) about their nominated memory. We adapted the instructions so they made sense for use with positive memories; for example, instead of referring to "stressful life events" we referred to "emotionally intense life events." Subjects were asked to report the extent to which they had been affected by each response listed on the scale "during the past day with respect to the memory you described earlier."

Finally, to measure the extent to which subjects believed their nominated event is central to their life story, we asked them to complete the 7-item Centrality of Event Scale (CES; Berntsen & Rubin, 2006). When completing this scale, subjects were asked to rate the extent to which they agree or disagree with a series of seven items measuring centrality of the event to the life story (*1 = totally disagree, 5 = totally agree*).

Phase 4. Finally, subjects provided some basic demographic information (age and gender), answered some questions about the extent to which they had complied with our instructions during the experiment, and were given some information about the second session of the experiment. More specifically, we told subjects "between now and the second session, we'd like you to work on trying to remember as much as you can about the event you described today, without talking to anyone else about it. Try as hard as you can to fill in any gaps in your memory for this event."

Session 2. Approximately 48 hours after participating in the first session, subjects were sent an email with the link to the second session and asked to complete the survey within the next 24 hours (M_{delay} = 48.01 hours, SD = 5.16, range = 37.38–63.85). In this session, subjects were reminded that "two days ago, during the first session of this study, we asked you to bring to mind the memory of one specific event you have experienced: The memory that "[most makes you feel positive/most makes you feel negative/feels like

your most important life event"] when you think of it today." We then asked subjects to again provide a free recall description of the event, using the same instructions as in Session 1, and to answer the same seven specific questions about the event. At the end of the session, subjects were once again asked to work on trying to remember as much as they could about the event and to try as hard as they could to fill in any gaps in their memory for the event.

Session 3. Approximately 48 hours after participating in the second session, subjects were sent an email with the link to the third and final session of the study and asked to please complete the survey within the next 24 hours (M_{delay} = 49.84 hours, SD = 7.72, range = 33.4–87.35). Session Three consisted of three phases.

Phase 1. First, subjects were again reminded about the memory they had brought to mind in the previous sessions of the study and asked to provide a free recall description of their memory for the event. Then subjects answered the same seven specific questions about the event as in Sessions 1 and 2.

Phase 2. Next, subjects completed the same 15-item version of the AMQ, the IES-R adapted for use with positive events, and the 7-item version of the CES. We then reminded subjects that "that at the end of the first and second sessions, we asked you to work on trying to remember as much as you could about the event you described, without talking to anyone else about it." We asked subjects "since the first session, how often have you tried to think about this event and remember more about what happened?" Subjects answered this question using a 5-point Likert-type (*0 = never, 4 = very often*). To satisfy an ethics requirement, we also asked subjects about the extent to which, compared to how they felt about the event before they began this study, they now felt (*1 = much worse, 3 = the same, 5 = much better).* Finally, subjects were fully debriefed about the experiment and asked if they had any comments.

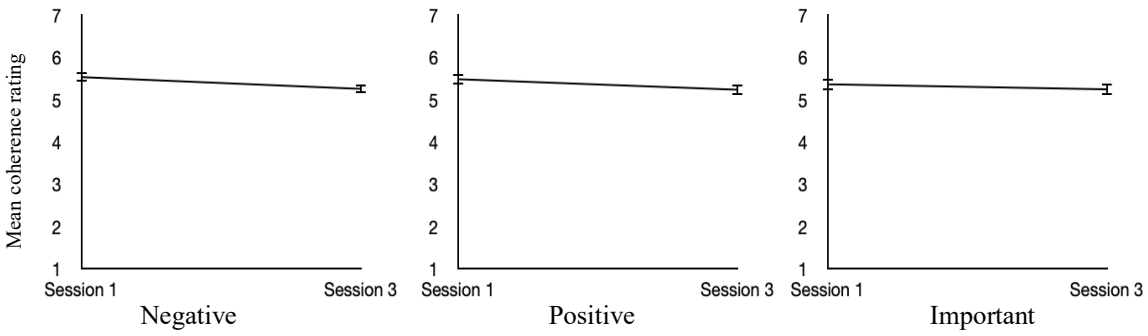
Results

We first excluded data from the 43 subjects who did not complete all three sessions, leaving 141 subjects. We then excluded data from 47 people who either a) did not recall same memory in all three sessions (27) or b) did not pass all 4 attention checks (34) (note some people were excluded on basis of both criteria). After these exclusions, we retained the data from 94 subjects for analysis (Negative n = 30, Positive n = 31, Important n = 33).

Changes in Memory Coherence

To what extent did people's memories cohere over the course of repeated recall? To answer this question, we compared subjects' mean coherence sum variable ratings from Session 1 to those from Session 3. We plot these data, classified according to the type of memory the subject recalled, in Figure 1. This figure shows at least three interesting findings. First, regardless of whether people were rating coherence in the first or third session, those who made ratings about a negative memory reported their memory was just as coherent as did those who made ratings about a positive or important memory (respectively, Session 1: M_{diff} = 0.05, 95% CI [-0.75, 0.85], M_{diff} = 0.17, 95% CI [-0.62, 0.96]; Session 2: M_{diff} = 0.02, 95% CI [-0.84, 0.88], M_{diff} = 0.01, 95% CI [-0.84, 0.85]). Therefore, we did not find evidence consistent with the idea that people's memories of

Figure 1



Note. Error bars represent 95% Cousineau-Morey confidence intervals

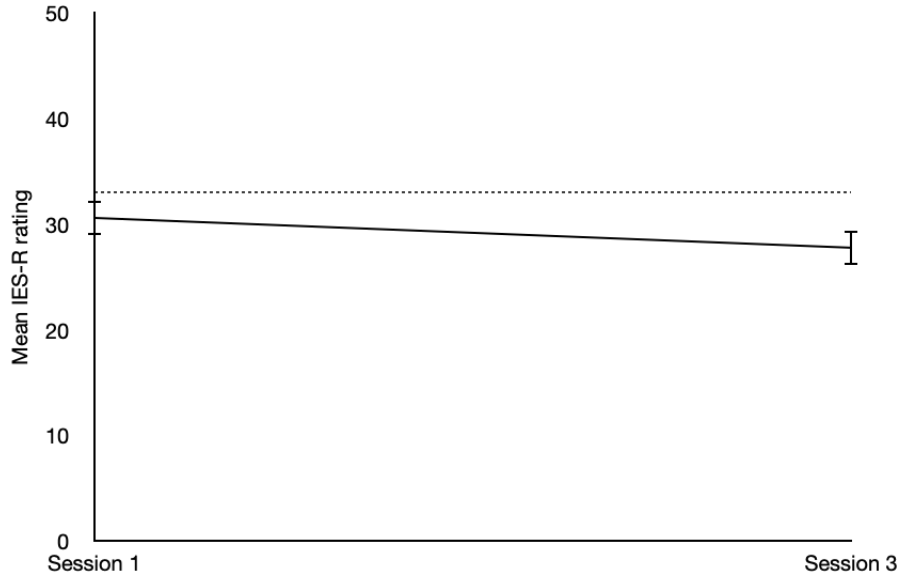
negative events are any less coherent than their memories of positive or important events. Second, the memories were all rated as fairly coherent; there was no evidence to suggest people's memories for negative events were incoherent during either Session 1 or Session 3.

Third, regardless of the type of memory people repeatedly recalled, they reported only trivial changes in coherence from Session 1 to Session 3 (Negative: M_{diff} = -0.28, 95% CI [-0.58, 0.02], Positive: M_{diff} = -0.26, 95% CI [-0.60, 0.08], Important: M_{diff} = -0.12, 95% CI [-0.52, 0.28]). This finding might suggest that people's memories do not cohere over the course of repeated recall. In fact, directionally, people tended to report a reduction in coherence over repeated recall. There are, however, several counterexplanations for this finding; I return to these explanations (and how we address them in the experiment proper) in the Discussion.

Changes in Symptoms of Distress

To what extent did people report a reduction in symptoms of distress over the course of repeated recall? To answer this question, we focused on the subset of subjects who repeatedly recalled a negative memory and calculated the mean of these subjects' IES-ratings in both Session 1 and Session 3. We display these data in Figure 2. As the figure shows, people reported a high degree of symptoms of distress about their negative experiences; the mean fell only slightly below the established threshold in the literature of 33 that is indicative of a high risk for meeting the diagnostic criteria for PTSD (see Creamer et al., 2003). Furthermore, people reported only a trivial reduction in their symptoms of distress between Sessions 1 and 3 (M_{diff} = -2.83, 95% CI [-7.39, 1.72]).

Figure 2



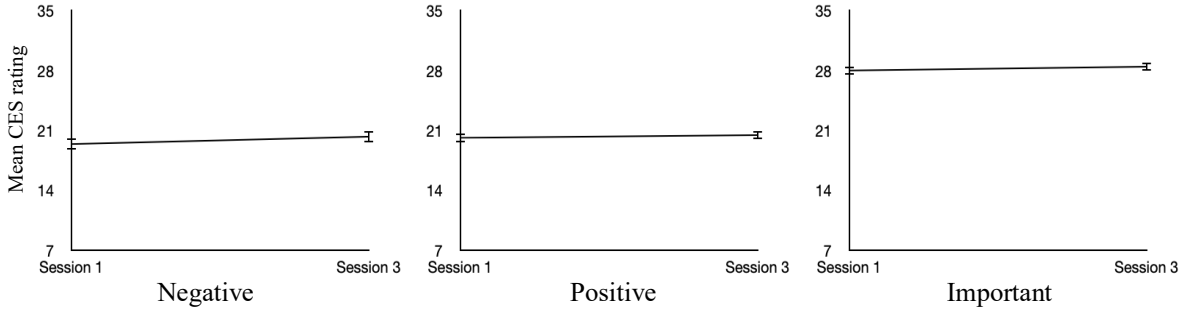
Note. Error bars represent 95% Cousineau-Morey confidence intervals. Reference line represents the established threshold in the literature above which people are at high risk for meeting the diagnostic criteria for PTSD (see Creamer et al., 2003).

This finding is not consistent with the idea that repeatedly recalling a negative memory helps to resolve a feeling of distress associated with that memory.

Changes in Centrality of Events

To what extent did people report a greater degree of centrality of their nominated memory to their life story over the course of repeated recall? To answer this question, we compared subjects' mean CES ratings from Session 1 to those from Session 3 and plot these data, classified according to the type of memory the subject recalled, in Figure 3. As the figure shows, regardless of whether they were making the ratings in the first or third session, those who recalled an important memory rated their memory as more central to their life story than did those who recalled both negative and positive memories (respectively, Session 1: M_{diff} = 8.60, 95% CI [3.20, 14.00], M_{diff} = 7.87, 95% CI [2.52, 13.22]; Session 2: M_{diff} = 8.19, 95% CI [2.86, 13.52], M_{diff} = 8.00, 95% CI [2.72, 13.29]).

Figure 3



Note. Error bars represent 95% Cousineau-Morey confidence intervals

Those who recalled negative and positive memories rated their memories as similarly central at both sessions (Session 1: M_{diff} = -0.73, 95% CI [-6.21, 4.75]; Session 2: M_{diff} = -0.18, 95% CI [-5.60, 5.23]). Furthermore, regardless of the type of memory people repeatedly recalled, they reported only trivial changes in the centrality of that event from Session 1 to Session 3 (Negative: M_{diff} = 0.87, 95% CI [-1.04, 2.77], Positive: M_{diff} = 0.32, 95% CI [-1.06, 1.70], Important: M_{diff} = 0.45, 95% CI [-0.90, 1.81]). This finding is not consistent with the idea that repeatedly recalling a memory helps people to integrate it into their life story. Of course, given these memories were of people's most negative, positive, and important events, it is possible the memories are highly rehearsed and have already been integrated into the life story as much as they ever will.

Discussion

In this pilot study, we asked people to repeatedly recall the memory of their most negative, most positive, or most important life event. People made ratings about the coherence of their memory, their symptoms of distress about the memory, and the centrality of the event to their life story, both before and after this repeated recall. We found that, regardless of the type of memory people recalled, they reported only trivial changes in memory coherence, distress, and centrality over the course of the experiment. Taken together, then, these findings are not consistent with the idea that repeatedly

recalling a memory resolves a feeling of incoherence, promotes a reduction in distress, or helps people to integrate the event into their life story.

These findings have at least three implications for the development of the Method for the experiment proper. First, when we look at Figure 1, we see that people reported their memories were already fairly coherent at Session 1. Perhaps, then, these memories had already had the chance to cohere over the years of rehearsal that likely occurred as people have talked about and thought about the event. It seems unlikely, then, that a mere three sessions of repeatedly recalling these highly rehearsed memories would be enough to further boost coherence. To eliminate this possibility in the experiment proper, we adopted the well-established trauma film paradigm (for reviews, see Holmes & Bourne, 2008; James et al., 2016) and instead asked people to repeatedly-recall the memories of events they encountered during the first session of the experiment.

Second, these findings raise the question: compared to what? Perhaps repeatedly recalling the memory is having some effect on these variables of interest, but we cannot detect the effect here because we do not have a comparison condition in which people do not repeatedly recall a similar memory. Therefore, in the experiment proper, we asked people to watch two films of a certain type of event, and then to repeatedly recall the memory of one of those films. People made ratings about both films at the beginning and end of the experiment, so we could compare the trajectory of coherence when people repeatedly recall a memory to when they do not.

Third, people made coherence ratings immediately after answering series of specific questions about their nominated event. We know that people's judgments of coherence are susceptible to attributions made in the moment depending on the way they retrieve a memory (Taylor et al., 2020). Perhaps, then, the repeated recall sessions did boost memory coherence, but the probing questions drew people's attention to remaining

gaps in their memories and produced a feeling of difficult retrieval, counteracting the effect of repeated recall. To avoid this potential counterexplanation in the experiment proper, we did not ask people to answer specific questions about their memories before making ratings about those memories.

Pilot Study 2: Effects of Order on People's Ratings of Symptoms of Posttraumatic Stress

In the experiment proper, we set out to determine the extent to which repeatedly recalling the memory of a traumatic event helps to resolve people's symptoms of posttrauamatic stress about that event. To address that aim, we wanted to compare their symptoms after repeated recalling one event to their symptoms about another similar event that they did not repeatedly recall. But when we examined the items on the scale we were using to gather a measure of symptoms (IES-R; Weiss & Marmar, 1997), we thought it might be possible that the order in which a person rates these two events might affect their reported symptoms. More specifically, although some of the items are event specific (for example, *any reminder brought back feelings about it, I felt as if it hadn't happened or wasn't real, I had dreams about it*), others are more difficult to attribute to one specific event (for example, *I had trouble staying asleep, I was jumpy and easily startled, I felt watchful and on guard*). We hypothesised, then, that people might attribute these symptoms to the first event they rate, producing a pattern of higher symptoms about that first event. We therefore conducted a pilot study to determine the degree to which the order of making ratings about the symptoms associated with two different events affects those symptom reports. This study was pre-registered and the pre-registration can be found at https://aspredicted.org/XPM_PCW.

Method

Subjects

We recruited subjects from the United States and Canada on Amazon's Mechanical Turk (MTurk) using the CloudResearch platform (https://www.cloudresearch.com/; Litman et al., 2017). A total of 102 MTurk workers completed the first session of the experiment. Of those subjects, 72 completed the second session. After exclusions (see Results), we retained the data from 62 subjects for analyses (M_{age} = 39.35 years, SD = 13.89 range = 18-74; 37% men, 61% women, 2% gender diverse). Subjects received $2US total for participating in the study, with payment split across the experimental sessions.

Design

We manipulated the order in which subjects made ratings about each film variant (Variant 1 first, Variant 2 first) between subjects.

Procedure

This experiment was approved by the University of Otago Human Ethics Committee, with reciprocal notification to the University of Waikato. The experiment was conducted in accordance with the ethical provisions of the World Medical Association Declaration of Helsinki. Before subjects provided their consent to participate in the study, they were informed that the film they would be asked to watch "may contain emotional content, such as a road traffic accident or stillbirth" and that "it is possible that some people may experience distress or discomfort when watching these videos." We therefore asked that if anyone thought they might be adversely affected by viewing any of these films, they should not participate in the study. The experiment consisted of two sessions, administered online using Qualtrics (https://www.qualtrics.com/).

Session 1. Session 1 contained three phases.

Phase 1. First, to verify that subjects' audio was working, they listened to an audio clip prompting them to "select the number 9 below." Subjects could not progress through to the next phase until they passed this soundcheck.

Phase 2. Subjects then watched two films depicting traumatic events (Traumatic Variant 1 and Traumatic Variant 2 from Paper 2; Chapter 3). We counterbalanced which of these films subjects watched first, and which they watched second. Subjects were asked to "please pay attention and watch the entire video once only. When the video ends, click the 'next' button to progress through the survey." We locked out the "next button" for the duration of each of the films. Immediately after watching each film, subjects were shown a screenshot from the film and asked to give the film "a short title (between 3 and 5 words)." We told subjects:

> Several days from now, we will use this video still plus your video title as a reminder for you, so that we can ask you questions about this particular video. So it is important this title is informative, and makes sense to you.

After providing that title, subjects then read a short filler passage of educational prose, before repeating the process for the other film (Butler et al., 2007).

Phase 3. Finally, subjects provided some basic demographic information, answered some questions about the extent to which they had complied with our instructions during the session, and were given some information about the second session of the experiment. More specifically, we told subjects we would contact them in approximately 48 hours with the link to the second session of the study, and that they should complete that session within 24 hours of receiving the link.

Session 2. Approximately 48 hours after participating in the first session, subjects were sent an email with the link to the second session and asked to complete the survey

within the next 24 hours (M_{delay} = 49.34 hours, SD =5.16, range = 43.87–68.95). Session 2 contained five phases.

Phase 1. First, we reminded subjects about the two films they had watched in Session 1. We then randomly assigned them to make ratings about the films in one of two orders: half of the subjects were assigned to first make ratings about Variant 1 and then about Variant 2, and the other half to first make ratings about Variant 2 and then about Variant 1. In their assigned order of rating, we then showed subjects a still image of each film, accompanied by their nominated title for the film, and asked them if they remembered this video. The survey was programmed to send anyone who reported they did not remember one of the films to the end of the experiment, but this situation did not arise.

Phase 2. Next, in their assigned order of rating, we again presented subjects with each combination of "still image and title" and asked subjects to rate the coherence of their memory for each film. Subjects made ratings on four items to capture a broad range of aspects of coherence (Rubin et al., 2003, 2004; Taylor et al., 2021): the story item, "as I remember the event, my memory comes to me as a coherent story," the pieces item, "as I remember the event, my memory comes to me in pieces, with bits missing," the fragments item, "as I remember the event, my memory comes to me as a collection of disconnected fragments," and the jumbled item, "as I remember the event, my memory feels jumbled, with parts of the event coming to me out of order." The items were presented in a random order and rated on a seven-point Likert-type scale from 1 (not at all) to 7 (completely). We created a coherence sum variable by reverse coding the pieces, fragments, and jumbled items, and calculating subjects' mean rating of the four items (Taylor et al., 2021). These four items produced Cronbach's αs of .84 and .87.

Phase 3. We then, in subjects' assigned order of rating, presented subjects with the still image and title of each film and asked them to report the symptoms of posttraumatic stress they had experienced over the last few days with respect to each film. To gather these symptom reports, we used the Impact of Events Scale Revised (IES-R; Weiss & Marmar, 1997), adapted for use a) with non-traumatic events, and b) over a 2-day timeframe rather than the typical 7-day timeframe. Subjects were told:

> we are now interested in how your memory of the video "[nominated title]" has affected you over the past two days. It is important you take the time to think about how this one video in particular has affected you.

Phase 4. Next, we asked subjects to provide a description of their memory for each of the films in their assigned order of rating. To gather these descriptions, we asked people to:

> report everything that you can remember about the event portrayed in this video. Make sure to leave nothing out. Start at the beginning and give a complete description of the entire event. It is important that your description contains as much detail as possible so that someone else who hasn't watched the video would be able to imagine the event accurately.

Phase 5. Finally, subjects answered some questions about the extent to which they had complied with our instructions during the session and were debriefed about the purpose of the experiment.

Results and Discussion

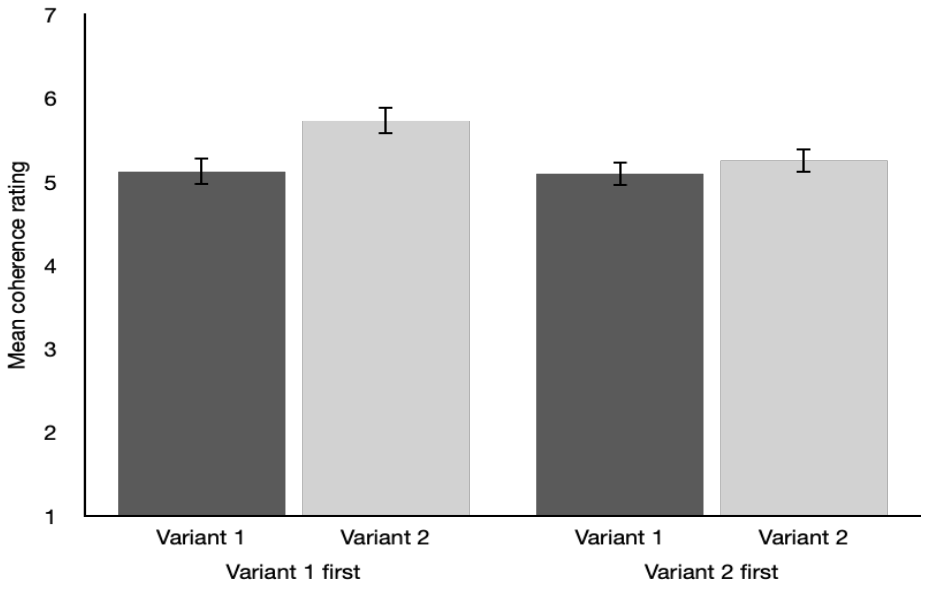
Of the 72 subjects who completed both experimental sessions, we excluded 10 subjects on the basis of pre-registered exclusion criteria. Of these 10, 8 subjects failed one

or more attention checks; 2 reported they completed at least one of the sessions of the study in more than one sitting, and 2 reported watching one of the videos more than once. Note some subjects were excluded on the basis of more than one of these criteria. After exclusions, then, retained the data from 62 for analyses.

Coherence of Memory

To what extent did the coherence of people's memories depend on the order in which they rated those memories? To answer this question, we calculated the mean coherence sum variable rating for each film variant, and classified that rating according to the order in which it had been made (Variant 1 first or Variant 2 first). We display these data in Figure 4. As the figure shows, people rated their traumatic memories as fairly coherent. Furthermore, people's coherence ratings for each film were similar regardless of the order in which they made those ratings. That is, there was no evidence of a significant Film x Order of Rating interaction $F(1,60) = 2.21$, $p = 0.143$. Regardless of the order in which they made the ratings, subjects rated their memories for Variant 2 as more coherent than Variant 1 ($M_{diff} = 0.39$, 95% CI [0.08, 0.70], $F(1,60) = 6.47$. $p = .014$).

Figure 4



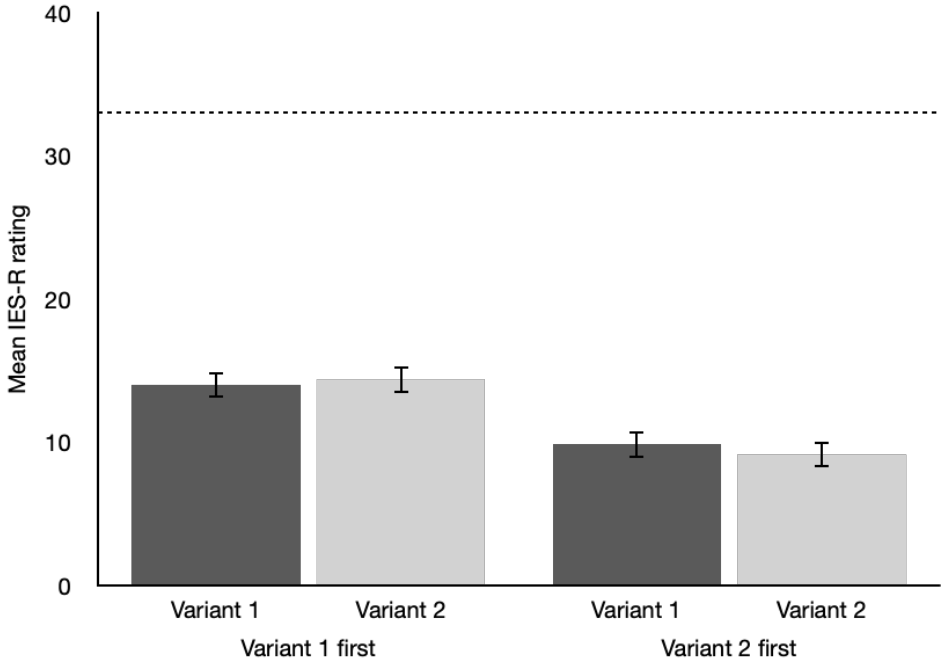
Note. Error bars represent 95% Cousineau-Morey confidence intervals

There was no evidence to suggest people reported different levels of coherence depending on whether they were making their first or second rating (M_{diff} = 0.26, 95% CI [-0.37, 0.88], $F(1,60) = 0.67$, $p = .416$).

Symptoms of Posttraumatic Stress

To what extent did people's reports of their symptoms of posttraumatic stress about each film depend on the order in which made those reports? To answer this question, we calculated the mean IES-R rating for each film variant, and classified that rating according to the order in which it had been made (Variant 1 first or Variant 2 first). We display these data in Figure 5. As the figure shows, people reported experiencing a non-trivial degree of symptoms of posttraumatic distress about each film. Furthermore, it looks like people's symptom reports for each film depended on the order in which they made those ratings. But there was no evidence of a significant Film x Order of Rating

Figure 5



Note. Error bars represent 95% Cousineau-Morey confidence intervals. Reference line represents the established threshold in the literature above which people are at high risk for meeting the diagnostic criteria for PTSD (see Creamer et al., 2003).

interaction $F(1,60) = 0.34$, $p = 0.564$. There was also no evidence to suggest people reported a different degree of symptoms depending on a) which film they were making reports about ($M_{diff} = 0.16$, 95% CI [-1.62, 1.94], $F(1,60) = 0.03$, $p = 0.858$) or whether they were making their first or second report ($M_{diff} = 4.69$, 95% CI [-2.07, 11.46], $F(1,60) = 1.93$, $p = .170$).

Taken together, then, there was no evidence to suggest the order in which people made ratings about the coherence of their memories and their symptoms of posttraumatic stress had a significant effect on those ratings. But when we look at Figure 5, it seems possible the first report people make about their symptoms anchors their second; perhaps we simply did not have the statistical precision to detect this effect. Out of an abundance of caution, because we were primarily interested in people's symptom reports about their repeatedly-recalled film, we fixed the order of IES-R ratings in the experiment proper such that subjects always reported their symptoms about their repeatedly-recalled film first and their control film second.

Finally, to what extent did people's reported symptoms of posttraumatic stress vary with their ratings of the coherence of their memory for the traumatic film? To answer this question, we ran a correlation using the data people provided about the first traumatic film they made ratings about. We found no evidence of a significant relation between these coherence ratings and reported symptomatology, $r = -0.05$, 95% CI [-0.29, 0.21], $p = 0.721$. This finding does not fit with the idea that incoherent traumatic memories underlie symptoms of posttraumatic stress.

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Appendix E: Supplemental Information for Paper 3 (Chapter 4)

Attrition and Exclusions

Of the 1076 subjects who completed the first session of the experiment, 106 were not invited back for the second session because they met the following pre-registered criteria: 51 reported having watched at least one of the films more than once, two reported that they did not watch at least one of the films the whole way through, 11 reported that they could not hear the audio in at least one of the films, 68 provided a description of the tasks they were asked to complete during Session 1 that did not reflect the nature of the tasks they were actually asked to complete (some subjects met more than one of these criteria). In total, then, 970 subjects were invited to take part in the second session.

A total of 721 subjects (74% of those invited) completed the second session. One of these subjects was not invited back for the third session because their description of their to-be repeatedly recalled film did not make sense, was completely incorrect, or indicated that they could not remember the film. In total, then, 720 subjects were invited to complete the third session.

A total of 662 subjects (92% of those invited) completed the third session. One of these subjects was not invited back for the fourth session because they did not describe their repeatedly recalled film during the session; we did not pre-register this criterion because we did not anticipate it would happen. In total, then, 661 subjects were invited to complete the fourth (and final) session.

A total of 620 subjects (94% of those invited) completed the fourth session. We excluded the data from 75 of these subjects (12%) on the basis of pre-registered exclusion criteria: 56 failed two or more attention checks; 16 reported they completed at least one of the sessions of the study in more than one sitting, and three provided a description of their

control memory at Session 4 either did not make sense or suggested they could not remember the film. In addition to these pre-registered criteria, we excluded the data from one subject who described the same memory twice in Session 4, one who did not describe their repeatedly-recalled memory in Session 3, and one who did not describe their repeatedly-recalled memory in Session 4. We did not pre-register these additional criteria because we did not anticipate the scenarios would happen. In total, then, we excluded the data from 78 subjects (13%), retaining 542 for analyses ($n_{traumatic}$ = 136, $n_{negative}$ = 136, $n_{positive}$ = 139, $n_{neutral}$ = 131).

Effect of Check-In Sessions

We measured the effect of the check-in sessions in three pre-registered ways.

Effect on Reported Coherence

There was no evidence to suggest the number of check-in sessions subjects completed mattered for their reports of memory coherence for their repeatedly-recalled memories. Subjects who completed two check-in sessions reported a similar degree of change in coherence to subjects who completed one, or zero, of the check-in sessions: respectively, M_{diff} = 0.14, 95% CI [-0.26, 0.53]; M_{diff} = 0.20, 95% CI [-0.44, 0.84]. In other words, a two-way ANOVA on subjects' reported change in coherence for their repeatedly-recalled memory, with valence of film and number of check-in sessions completed (0, 1, 2) as between-subjects variables was not significant: $F(11, 530) = 1.28$, $p = 0.23$.

Effect on Computer-Scored Measures of Coherence

There was no evidence to suggest the number of check-in sessions subjects completed mattered for the degree of change in their computer-scored measures of coherence for their repeatedly-recalled memories. In other words, a two-way MANOVA on the change in computer-scored measures of coherence gathered using subjects'

memory descriptions showed no evidence of a significant interaction between valence of film and number of check-in sessions completed, $F(6, 530) = 1.86$, $p = .086$. There was also no evidence of a significant main effect of number of check-in sessions completed, $F(2, 530) = .284$, $p = .753$.

Effect on Symptoms of Posttraumatic Stress

Finally, there was no evidence to suggest the number of check-in sessions subjects completed mattered for their reported symptoms of posttraumatic stress. Subjects who completed two check-in sessions reported a similar degree of symptoms of posttraumatic stress to subjects who completed one, or zero, of the check-in sessions: respectively, M_{diff} = -2.45, 95% CI [-5.18, 0.29]; M_{diff} = -2.71, 95% CI [-7.20, 1.78]. In other words, a two-way ANOVA on subjects' IES-R score for their repeatedly-recalled memories with type of film and number of check-in sessions completed (0, 1, 2) as between subjects variables showed no evidence of a significant interaction between valence of film and number of check-in sessions completed, $F(6, 535) = 0.47$, $p = .833$. There was also no evidence of a significant main effect of number of check-in sessions completed, $F(2, 539) = 2.98$, $p = .052$.

Autobiographical Memory Questionnaire Data

Table S1

Ratings of Repeatedly-Recalled and Control Memories on the Autobiographical Memory Questionnaire

	M								***M*$_{diff}$ [95% CI]**			
	Repeatedly-Recalled Memories				**Control Memories**							
Item	**Traumatic**	**Negative**	**Positive**	**Neutral**	**Traumatic**	**Negative**	**Positive**	**Neutral**	**Traumatic**	**Negative**	**Positive**	**Neutral**
As I remember this event, I feel as though I am reliving it.[a]	3.04	2.82	3.02	2.62	2.59	2.62	2.75	2.30	0.46 [0.16, 0.75]	0.22 [-0.03, 0.47]	0.27 [-0.01, 0.56]	0.32 [0.06, 0.58]
As I remember this event, the emotions that I feel are intense.[b]	3.53	3.42	2.25	1.66	3.40	3.09	2.06	1.45	0.13 [-0.17, 0.42]	0.33 [0.04, 0.62]	0.19 [-0.01, 0.40]	0.21 [0.04, 0.39]
As I remember this event, I have a physical reaction (I felt tense, sweaty, felt cramps or butterflies in my stomach, my heart pounded, etc.)[c]	1.78	1.54	1.27	1.19	1.63	1.55	1.22	1.15	0.15 [-0.07, 0.37]	-0.01 [-0.18, 0.15]	0.05 [-0.05, 0.15]	0.04 [-0.08, 0.16]
As I remember this event, the emotions that I feel are negative.[b]	4.92	3.77	1.37	1.55	4.73	3.84	1.29	1.56	0.19 [-0.05, 0.43]	-0.07 [-0.37, 0.24]	0.07 [-0.09, 0.23]	-0.01 [-0.25, 0.23]
As I remember this event, the emotions that I feel are positive.[b]	1.49	2.40	5.36	3.92	1.33	2.32	5.17	3.79	0.17 [0.01, 0.32]	0.07 [-0.16, 0.31]	0.19 [-0.07, 0.44]	0.12 [-0.17, 0.41]
Since I watched the video, I have talked about it.[d]	1.65	1.74	1.44	1.53	1.51	1.56	1.37	1.38	0.14 [0.00, 0.28]	0.18 [-0.13, 0.37]	0.06 [-0.08, 0.21]	0.15 [-0.04, 0.34]
Since I watched the video, I have thought deliberately about it.[d]	2.49	2.42	2.14	2.37	2.00	2.07	1.85	1.95	0.49 [0.24, 0.73]	0.35 [0.08, 0.61]	0.29 [0.11, 0.48]	0.42 [0.18, 0.66]
Since I watched the video, it has popped up in my mind by itself—that is, without my trying to think of it.[d]	2.38	2.57	2.12	2.20	2.26	2.05	1.92	1.90	0.12 [-0.16, 0.40]	0.52 [0.27, 0.77]	0.19 [0.01, 0.38]	0.30 [0.05, 0.54]

Note. *M*$_{diff}$ = repeatedly-recalled – control memory.

[a] *1 = not at all, 7 = as if it were happening now*

[b] *1 = not at all, 7 = to a very high degree*

[c] *1 = not at all, 7 = extremely strongly*

[d] *1 = not at all, 7 = all the time*

Computer-Scored Measures of Coherence

Information About Each Measure

Linguistic Inquiry Word Count (LIWC; Pennebaker et al., 2015).

- **Cognitive processes** Provides a measure of the percentage of words in each description that are indicative of a cognitive process or mechanism. For example, cause, think, ought.
- **Insight** One of the components of the cognitive processes measure. Provides a measure of the percentage of words in each description that are indicative of insight. For example, think, know.
- **Causation** Another of the components of the cognitive processes measure. Provides a measure of the percentage of words in each description that are indicative of causal processes. For example, because, effect
- **Nonfluencies** Provides a measure of the percentage of words in each description that are markers of a lack of fluent speech. For example, er, hm, umm
- **Fillers** Provides a measure of the percentage of words in each description that are fillers. For example, Imean, youknow

Coh-Metrix (Graesser et al., 2004).

- **Narrativity** A measure of the extent to which a description tells a story with components that are familiar to most people. Higher scores reflect a greater use of familiar words, knowledge, and language. Reported as z-scores.
- **Syntactic ease (or syntactic simplicity)** A measure of the extent to which a description is easier to process. Higher scores reflect shorter descriptions with simpler, more common, syntax structures. Reported as z-scores.

- **Word concreteness** A measure of the extent to which a description contains concrete, meaningful words. Higher scores reflect a more concrete and easier to understand description. Reported as z-scores.
- **Referential cohesion** A measure of the extent to which words and ideas overlap across sentences to form a cohesive narrative. Higher scores reflect a description contains a greater number of connective phrases and ideas that tie the story together. Reported as z-scores.
- **Deep cohesion** A measure of the extent to which the description contains causal and logical connectives. Higher scores reflect a description that contains more connections to create a coherent narrative, rather than requiring the reader to connect the ideas themselves. Reported as z-scores.
- **Content word overlap** A measure of the extent to which sentences in the description contain the same words. Higher scores reflect a greater degree of overlap, and therefore a common thread throughout the description. Reported as the proportion of words overlapping between pairs of sentences.
- **Latent semantic analysis** A measure of the extent to which sentences in the description overlap semantically. Higher scores reflect a greater degree of similarity in adjacent sentences. Reported as a score from 0 (low cohesion) to 1 (high cohesion).
- **Temporal connectives** A measure of the extent to which the description contains words to link ideas temporally, such as “first”. Reported as occurrence of temporal connective words per 1000 words.
- **Extended temporal connectives** A measure of the extent to which the description contains words to link ideas in an extended temporal manner, such as “until”. Reported as occurrence of extended temporal connective words per 1000 words.

- **Causal connectives** A measure of the extent to which the description contains words to link ideas causally, such as “because”. Reported as occurrence of causal connective words per 1000 words.
- **Adversative connectives** A measure of the extent to which the description contains words to indicate contrast among ideas, such as “whereas”. Reported as occurrence of adversative connective words per 1000 words.
- **Logical connectives** A measure of the extent to which the description contains words to link ideas logically, such as “and”. Reported as occurrence of logical connective words per 1000 words.
- **Additive connectives** A measure of the extent to which the description contains words to link ideas to build on one another, such as “moreover”. Reported as occurrence of additive connective words per 1000 words.

Table S2

Means and 95% Confidence Intervals of Computer-Scored Measures of Coherence From Days 5 and 7.

	Repeatedly-Recalled Memories								Control Memories			
	Day 5				Day 7				Day 7			
Measure	**Traumatic**	**Negative**	**Positive**	**Neutral**	**Traumatic**	**Negative**	**Positive**	**Neutral**	**Traumatic**	**Negative**	**Positive**	**Neutral**
Cognitive processes	6.89 [6.41, 7.35]	8.73 [8.27, 9.18]	6.67 [6.23, 7.30]	9.99 [9.30, 10.67]	6.27 [5.71, 6.83]	7.42 [6.93, 7.90]	5.51 [4.99, 6.03]	8.73 [7.80, 9.65]	6.71 [6.09, 7.32]	8.52 [7.94, 9.10]	7.15 [6.41, 7.88]	10.88 [9.86, 11.90]
Insight	1.78 [1.56, 2.01]	2.81 [2.56, 3.07]	1.46 [1.24, 1.67]	1.73 [1.42, 2.05]	1.28 [1.02, 1.54]	2.50 [2.23, 2.77]	1.12 [0.85, 1.39]	1.19 [0.83, 1.54]	1.67 [1.35, 1.98]	2.49 [2.22, 2.75]	1.43 [1.08, 1.78]	1.80 [1.36, 2.23]
Causation	0.95 [0.80, 1.09]	1.39 [1.20, 1.58]	0.78 [0.60, 0.97]	3.87 [3.48, 4.26]	0.74 [0.59, 0.88]	1.21 [1.01, 1.41]	0.42 [0.30, 0.54]	4.24 [3.64, 4.84]	0.73 [0.75, 0.88]	1.36 [1.12, 1.60]	0.48 [0.34, 0.63]	4.67 [4.02, 5.32]
Nonfluencies	0.07 [0.03, 0.10]	0.08 [0.04, 0.12]	0.15 [0.07, 0.23]	0.05 [0.01, 0.10]	0.09 [0.02, 0.15]	0.11 [0.05, 0.17]	0.20 [0.08, 0.32]	0.02 [-0.01, 0.05]	0.09 [0.03, 0.14]	0.03 [0.01, 0.06]	0.15 [0.05, 0.24]	0.04 [0.00, 0.08]
Fillers	0.00 [0.00, 0.01]	0.01 [0.00, 0.02]	0.00 [0.00, 0.01]	0.00 [0.00, 0.01]	0	0.01 [0.00, 0.01]	0	0.01 [-0.01, 0.04]	0.01 [-0.01, 0.03]	0.01 [0.00, 0.03]	0	0
Narrativity	0.66 [0.54, 0.80]	1.17 [1.07, 1.27]	0.52 [0.39, 0.65]	0.42 [0.26, 0.59]	0.67 [0.56, 0.78]	1.19 [1.05, 1.32]	0.26 [0.12, 0.40]	0.19 [0.02, 0.36]	0.60 [0.47, 0.74]	1.24 [1.10, 1.38]	0.48 [0.33, 0.63]	0.27 [0.07, 0.47]
Syntactic ease	-0.15 [-0.30, 0.00]	-0.53 [-0.70, -0.39]	-0.07 [-0.21, 0.06]	-0.16 [-0.32, 0.00]	-0.17 [-0.35, 0.01]	-0.63 [-0.77, -0.49]	-0.08 [-0.25, 0.09]	0.00 [-0.19, 0.19]	-0.13 [-0.30, 0.04]	-0.80 [-0.94, -0.67]	-0.34 [-0.50, -0.18]	-0.01 [-0.19, 0.18]
Word concreteness	2.06 [1.88, 2.25]	1.47 [1.32, 1.61]	1.43 [1.25, 1.61]	1.12 [0.93, 1.31]	2.48 [2.30, 2.65]	1.92 [1.74, 2.09]	1.81 [1.58, 2.05]	1.73 [1.49, 1.98]	2.22 [1.99, 2.44]	1.89 [1.70, 2.09]	1.67 [1.44, 1.91]	1.44 [1.15, 1.73]
Referential cohesion	0.76 [0.58, 0.94]	0.58 [0.44, 0.72]	0.12 [-0.04, 0.27]	0.91 [0.71, 1.12]	1.09 [0.89, 1.28]	0.76 [0.60, 0.92]	0.21 [0.01, 0.41]	1.35 [1.11, 1.59]	0.70 [0.47, 0.94]	0.92 [0.75, 1.08]	0.44 [0.23, 0.64]	1.15 [0.93, 1.37]
Deep cohesion	-0.14 [-0.32, 0.04]	0.28 [0.10, 0.47]	0.79 [0.54, 1.04]	0.24 [0.01, 0.48]	-0.28 [-0.47, -0.09]	0.01 [-0.18, 0.21]	0.52 [0.24, 0.80]	0.25 [-0.07, 0.56]	-0.45 [-0.67, -0.22]	-0.04 [-0.25, 0.16]	0.67 [0.34, 1.00]	0.35 [0.00, 0.69]
Content word overlap	0.13 [0.12, 0.14]	0.14 [0.13, 0.15]	0.13 [0.12, 0.14]	0.15 [0.13, 0.16]	0.14 [0.13, 0.15]	0.15 [0.14, 0.16]	0.12 [0.11, 0.14]	0.17 [0.16, 0.19]	0.12 [0.11, 0.14]	0.15 [0.14, 0.17]	0.13 [0.11, 0.14]	0.14 [0.13, 0.16]
Latent semantic analysis	0.23 [0.22, 0.25]	0.18 [0.17, 0.19]	0.19 [0.18, 0.20]	0.25 [0.23, 0.27]	0.26 [0.24, 0.28]	0.19 [0.18, 0.21]	0.19 [0.18, 0.21]	0.28 [0.25, 0.31]	0.25 [0.23, 0.27]	0.20 [0.18, 0.21]	0.20 [0.18, 0.22]	0.24 [0.21, 0.27]
Temporal connectives	22.88 [20.17, 25.58]	17.60 [15.96, 19.23]	25.48 [22.80, 28.17]	23.76 [20.93, 26.59]	24.68 [21.78, 27.59]	18.60 [16.78, 20.43]	26.94 [23.84, 30.04]	26.11 [22.48, 29.75]	22.71 [18.47, 24.95]	16.47 [14.39, 18.56]	26.39 [23.07, 29.71]	25.09 [20.90, 29.28]
Extended temporal connectives	9.14 [7.46, 10.83]	15.76 [13.91, 17.61]	18.01 [15.75, 20.28]	4.30 [2.87, 5.72]	6.63 [5.21, 8.05]	15.01 [12.49, 17.52]	15.51 [23.84, 30.04]	3.69 [1.38, 5.60]	5.13 [3.70, 6.56]	12.22 [10.27, 14.16]	18.32 [14.96, 21.67]	4.87 [2.12, 7.63]
Causal connectives	15.55 [13.43, 17.67]	24.40 [21.82, 26.97]	30.53 [27.05, 34.02]	21.53 [18.22, 24.84]	12.81 [11.04, 14.59]	21.17 [18.49, 23.84]	15.51 [12.83, 18.19]	20.99 [17.56, 24.43]	12.02 [9.81, 14.22]	21.86 [19.52, 24.19]	33.11 [28.26, 37.96]	25.70 [20.06, 31.35]
Adversative connectives	9.51 [7.97, 11.05]	7.00 [5.90, 8.10]	5.42 [4.29, 6.55]	8.48 [6.27, 10.69]	9.11 [7.46, 10.76]	6.07 [4.89, 7.26]	26.72 [22.90, 30.55]	4.93 [3.38, 6.48]	10.43 [8.20, 12.67]	8.09 [6.22, 9.97]	4.68 [3.30, 6.06]	7.07 [5.07, 9.07]
Logical connectives	29.52 [26.68, 32.35]	34.55 [32.06, 37.04]	38.84 [35.57, 42.11]	33.63 [30.73, 36.53]	27.53 [24.44, 30.62]	30.47 [27.47, 33.47]	5.02 [3.43, 6.61]	32.03 [28.03, 36.04]	25.96 [22.72, 29.20]	29.87 [26.93, 32.81]	38.10 [33.83, 42.36]	35.06 [30.70, 39.43]
Additive connectives	49.37 [46.12, 52.63]	49.19 [46.60, 51.78]	52.19 [48.70, 55.67]	51.62 [47.90, 55.33]	50.91 [46.86, 54.96]	50.97 [47.82, 54.12]	38.59 [34.62, 42.56]	52.74 [47.58, 57.90]	53.88 [49.87, 57.90]	49.14 [46.26, 52.03]	44.20 [39.99, 48.42]	51.39 [45.45, 57.32]

Word Count

Descriptions of the repeatedly-recalled memories contained a mean of 124 words on Day 3 (*Med* = 109.5, SD = 71.88), 163 words on Day 5 (*Med* = 142, SD = 98.09), and 120 words on Day 7 (*Med* = 106, SD = 73.37). Because we used a different probe on Day 5 to elicit these memory descriptions, it is possible the greater word count can be attributed to those more detailed instructions. Descriptions of control memories on Day 7 contained a mean of 98 words. Consistent with the idea that repeatedly-recalling a memory protected against decay, people's descriptions of their repeatedly-recalled memories on Day 7 contained more words than those of the control memories (M_{diff} = 21.76, 95% CI [17.58, 25.76]).

The length of these descriptions varied depending on the type of memory being described. On Day 3, descriptions of traumatic memories were similar in length to the negative and positive counterparts, and contained more words than the neutral counterparts (respectively, M_{diff} = -19.12, 95% CI [-40.42, -2.29]; M_{diff} = 20.48, 95% CI [-0.80, 41.77]; M_{diff} = 41.31, 95% CI [19.70, 62.91]; F (3, 538) = 19.37, p < .001). On both Days 5 and 7, descriptions of the repeatedly-recalled traumatic memories contained fewer words than the negative counterparts, were similar in length to the positive counterparts, and contained more words than the neutral counterparts (respectively, Day 5: M_{diff} = -40.49, 95% CI [-69.92, -11.07]; M_{diff} = 14.08, 95% CI [-15.19, 43.35]; M_{diff} = 37.90, 95% CI [8.20, 67.61]; F (3, 538) = 16.36, p < .001; Day 7: M_{diff} = -23.21, 95% CI [-44.95, -1.48]; M_{diff} = 19.31, 95% CI [-2.31, 40.92]; M_{diff} = 42.08, 95% CI [20.14, 64.02]; F (3, 538) = 21.38, p < .001). On Day 7, descriptions of the control traumatic memories contained fewer words than the negative counterparts, and more words than the positive and neutral counterparts (respectively, M_{diff} =

-21.79, 95% CI [-41.27, -2.32]; M_{diff} = 24.27, 95% CI [4.90, 43.64]; M_{diff} = 39.68, 95% CI [20.02, 59.34]; F (3, 538) = 25.30, p < .001).

Additional Analyses

Changes in Computer-scored Measures of Memory Coherence Gathered From Day 3, 5, and 7 Descriptions of Subjects' Repeatedly-Recalled Memories

There were systematic differences in the computer-scored measures of coherence depending on the experimental session. That is, a mixed-model MANOVA on the computer-scored measures of coherence with valence of film as a between-subjects factor and experimental session as a within-subject factor revealed a significant effect of experimental session, F(34, 3198) = 4.43, p <.001. To determine the effect of experimental session on computer-scored measures of coherence, we ran a series of follow-up repeated measures ANOVAs on each measure of computer-scored coherence, with experimental session as the within-subjects factor. We report these data in Table S3.

As the table shows, there was a significant effect of experimental session on 14 of the 18 computer-scored measures of coherence. For ten of these 14 measures there was a significant change in coherence between Day 3 and Day 5; for seven of the measures coherence increased from Day 3 to 5 (cognitive processes, insight, narrativity, syntactic ease, deep cohesion, causal connectives, and logical connectives), and for three coherence decreased (word concreteness, referential cohesion, and latent semantic analysis). For three of these 14 measures there was a significant change in coherence between Day 3 and Day 7; for two of the measures coherence increased from Day 3 to Day 7 (syntactic ease and additive connectives) and for one coherence decreased (latent semantic analysis). For nine of these 14 measures there was a significant change in coherence between Day 5 and Day 7; for

Table S3

Repeated Measures ANOVAs on Computer-Scored Measures of Coherence for Repeatedly-Recalled Memories by Experimental Session

Measure	*F*	*p*	Day 5 – Day 3	Day 7 – Day 3	Day 7 – Day 5
Cognitive processes	26.01	<.001*	0.88 [0.48, 1.27]	-0.23 [-0.62, 0.16]	-1.11 [-1.50, -0.72]
Insight	30.78	<.001*	0.55 [0.37, 0.73]	0.12 [-0.05, 0.30]	-0.43 [-0.60, 0.25]
Causation	2.21	.110	0.16 [-0.03, 0.35]	0.06 [-0.13, 0.25]	-0.10 [-0.29, 0.09]
Nonfluencies	0.30	.741	0.00 [-0.06, 0.05]	0.01 [-0.04, 0.07]	0.02 [-0.04, 0.07]
Fillers	0.02	.982	0.00 [-0.01, 0.01]	0.00 [-0.01, 0.01]	0.00 [-0.01, 0.01]
Narrativity	7.26	<.001*	0.11 [0.03, 0.19]	-0.01 [-0.09, 0.07]	-0.12 [-0.20, 0.04]
Syntactic ease	4.32	.014*	0.10 [0.00, 0.20]	0.11 [0.01, 0.21]	0.01 [-0.09, 0.11]
Word concreteness	54.10	<.001*	-0.40 [-0.51, -0.28]	0.07 [-0.05, 0.18]	0.46 [0.35, 0.58]
Referential cohesion	19.18	<.001*	-0.29 [-0.42, -0.17]	-0.04 [-0.16, 0.09]	0.26 [0.13, 0.38]
Deep cohesion	6.97	<.001*	0.24 [0.08, 0.39]	0.06 [-0.09, 0.22]	-0.17 [-0.33, -0.02]
Content word overlap	7.00	<.001*	-0.01 [-0.02, 0.00]	0.00 [-0.01, 0.01]	0.01 [0.00, 0.02]
Latent semantic analysis	18.89	<.001*	-0.03 [-0.04, -0.02]	-0.01 [-0.03, -0.01]	0.02 [0.01, 0.03]
Temporal connectives	2.52	.081	-0.50 [-2.30, 1.31]	1.15 [-0.65, 2.96]	1.65 [-0.16, 3.46]
Extended temporal connectives	4.30	.014*	0.97 [-0.36, 2.29]	-0.65 [-1.97, 0.68]	-1.61 [-2.94, -0.29]
Causal connectives	7.65	<.001*	2.88 [0.93, 4.82]	0.27 [-1.68, 2.22]	-2.60 [-4.55, -0.66]
Adversative connectives	4.36	.013*	0.96 [-0.13, 2.05]	-0.33 [-1.42, 0.76]	-1.29 [-2.38, -0.20]
Logical connectives	6.36	.002*	3.20 [1.03, 5.37]	1.23 [-0.94, 3.40]	-1.97 [-4.14, 0.20]
Additive connectives	3.81	.023*	0.79 [-1.65, 3.22]	2.72 [0.29, 5.16]	1.93 [-0.50, 4.37]

Note. For all tests, $F(2, 1082)$.
* represents significant effect of experimental session (Day 3, Day 5, Day 7) on measure of computer-scored coherence.

our of the measures coherence increased from Day 5 to Day 7 (word concreteness, referential cohesion, content word overlap, latent semantic analysis) and for five coherence decreased (cognitive processes, deep cohesion, extended temporal connectives, causal connectives, and adversative connectives).

Difference in Symptoms of Posttraumatic Stress About Repeatedly-Recalled and Control Memories

Subjects who watched traumatic films reported a greater degree of symptoms of posttraumatic stress about the memory they repeatedly recalled than they did about the memory that served as a control (respectively, M = 12.35, 95% CI[10.53, 14.18], M = 10.69, 95% CI [8.61, 12.78], M_{diff} = 1.66, 95% CI [0.16, 3.16]. At first glance, this finding is not consistent with the idea that repeatedly recalling the memory of a traumatic event helps to resolve the symptoms of posttraumatic stress people experience about that event. But it is possible that asking people to repeatedly recall their memory boosted the availability of their memory for that film, which might have produced this pattern. Furthermore, we did not counterbalance the order of people's symptomatology reports, so it is possible people attributed more of their symptoms to their repeatedly recalled memory than their control memory, simply because they made reports about their symptoms for that memory first. Therefore, we are hesitant to place much emphasis on this finding.

Were There Order Effects for the Ratings People Made About Their Repeatedly-Recalled and Control Memories?

There was no evidence to suggest the order in which people made their ratings about the coherence of their repeatedly-recalled and control memories mattered for how coherent they reported those memories were. More specifically, on Day 3, people's ratings of their repeatedly-recalled memories were similar regardless of which memory they rated first, respectively M = 4.91, 95% CI [4.73, 5.08], M = 4.94 [4.76, 5.12]. Also on Day 3, people's ratings of their control memories were regardless of which memory they rated first, respectively M = 5.04 [4.86, 5.21], M = 4.99 [4.82, 5.15]. On Day 7, people's ratings of their

repeatedly-recalled memories were similar regardless of which memory they rated first, respectively *M* = 5.01 [4.84, 5.18], *M* = 5.02 [4.85, 5.19]. Also on Day 7, people's ratings of their control memories were similar regardless of which memory they rated first, respectively *M* = 4.06 [3.87, 4.25], *M* = 4.14 [3.95, 4.34].

Even though people made their symptomatology reports in a fixed order (repeatedly-recalled memory then control memory) there was some evidence to suggest the order in which they made the rest of their ratings about their memories on Day 7 mattered for the degree of symptoms they reported. More specifically, when people made the rest of their ratings about their repeatedly-recalled memory first, they reported similar levels of symptomatology about their repeatedly-recalled and control memories, respectively *M* = 10.97, 95% CI [8.32, 13.62], *M* = 10.27 [6.86, 13.68]. But when people made the rest of their ratings about their control memory first, they reported a greater degree of symptomatology about their repeatedly-recalled memory than they did their control memory, respectively *M* = 13.55 [11.01, 16.08], *M* = 11.05 [8.44, 13.67], *M*diff = 2.49 [0.41, 4.57].

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Pennebaker, J.W., Boyd, R.L., Jordan, K., & Blackburn, K. (2015). *The development and psychometric properties of LIWC2015*. University of Texas at Austin.

Appendix F: Co-Authorship Forms

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Co-Authorship Form

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Chapter 2 (Manuscript 1)

Taylor, A., Jordan, K., Zajac, R., Takarangi, M. K. T., & Garry, M. (2020). Judgments of Memory Coherence Depend on the Conditions Under Which a Memory is Retrieved, Regardless of Reported PTSD Symptoms. Journal of Applied Research in Memory and Cognition, 9(3), 396–409. https://doi.org/10.1016/j.jarmac.2020.07.003

Nature of contribution by PhD candidate	Andrea led the planning, data collection, analysis, and writing of the manuscript with feedback and support from Kayla, Rachel, Melanie, and Maryanne.
Extent of contribution by PhD candidate (%)	70

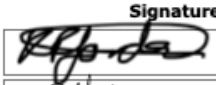
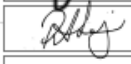
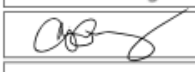
CO-AUTHORS

Name	Nature of Contribution
Kayla Jordan	Provided feedback and support for the planning, data collection, analysis, and writing of the manuscript
Rachel Zajac	Provided feedback and support for the planning, data collection, analysis, and writing of the manuscript
Melanie Takarangi	Provided feedback and support for the planning, data collection, analysis, and writing of the manuscript
Maryanne Garry	Provided feedback and support for the planning, data collection, analysis, and writing of the manuscript

Certification by Co-Authors

The undersigned hereby certify that:

- the above statement correctly reflects the nature and extent of the PhD candidate's contribution to this work, and the nature of the contribution of each of the co-authors; and

Name	Signature	Date
Kayla Jordan		22/11/2022
Rachel Zajac		21 Nov 2022
Melanie Takarangi	Melanie Takarangi	20/11/2022
Maryanne Garry		21/11/2022

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Chapter 3 (Manuscript 2)

Taylor, A., Zajac, R., Takarangi, M. K. T., & Garry, M. (2021). Evidence From the Trauma-Film Paradigm That Traumatic and Nontraumatic Memories Are Statistically Equivalent on Coherence. Clinical Psychological Science, 10(3), 417–429. https://doi.org/10.1177/21677026211053312

Nature of contribution by PhD candidate	Andrea led the planning, data collection, analysis, and writing of the manuscript with feedback and support from Rachel, Melanie, and Maryanne.
Extent of contribution by PhD candidate (%)	70

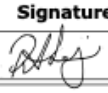
CO-AUTHORS

Name	Nature of Contribution
Rachel Zajac	Provided feedback and support for the planning, data collection, analysis, and writing of the manuscript
Melanie Takarangi	Provided feedback and support for the planning, data collection, analysis, and writing of the manuscript
Maryanne Garry	Provided feedback and support for the planning, data collection, analysis, and writing of the manuscript

Certification by Co-Authors

The undersigned hereby certify that:

- the above statement correctly reflects the nature and extent of the PhD candidate's contribution to this work, and the nature of the contribution of each of the co-authors; and

Name	Signature	Date
Rachel Zajac		21 Nov 2022
Melanie Takarangi	Melanie Takarangi	21/11/2022
Maryanne Garry		21/11/2022

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Chapter 4 (Manuscript 3)

Taylor, A., Zajac, R., Takarangi, M. K. T., & Garry, M. (2022). The Coherence of Analogue Traumatic and Non-Traumatic Memories Over Repeated Recall. [Manuscript in preparation]. School of Psychology, The University of Waikato.

Nature of contribution by PhD candidate	Andrea led the planning, data collection, analysis, and writing of the manuscript with feedback and support from Rachel, Melanie, and Maryanne.
Extent of contribution by PhD candidate (%)	70

CO-AUTHORS

Name	Nature of Contribution
Rachel Zajac	Provided feedback and support for the planning, data collection, analysis, and writing of the manuscript
Melanie Takarangi	Provided feedback and support for the planning, data collection, analysis, and writing of the manuscript
Maryanne Garry	Provided feedback and support for the planning, data collection, analysis, and writing of the manuscript

Certification by Co-Authors

The undersigned hereby certify that:

- the above statement correctly reflects the nature and extent of the PhD candidate's contribution to this work, and the nature of the contribution of each of the co-authors; and

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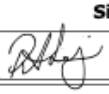
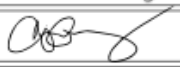
Rachel Zajac		21 Nov 2022
Melanie Takarangi	Melanie Takarangi	21/11/2022
Maryanne Garry		21/11/2022