Journal of Abnormal Psychology

Manuscript version of

Participant, Rater, and Computer Measures of Coherence in Posttraumatic Stress Disorder

David C. Rubin, Samantha A. Deffler, Christin M. Ogle, Nia M. Dowell, Arthur C. Graesser, Jean C. Beckham

Funded by:

- Danish National Research Foundation
- Department of Defense, Air Force Office of Scientific Research
- Institute of Education Sciences
- National Institute of Mental Health
- VA Clinical Sciences Research Development

 The final version of record is available via its DOI: https://dx.doi.org/10.1037/abn0000126

Participant, Rater, and Computer Measures of Coherence in Posttraumatic Stress Disorder

David C. Rubin, Duke University and Aarhus University

Samantha A. Deffler and Christin M. Ogle, Duke University

Nia M. Dowell and Arthur C. Graesser, University of Memphis

Jean C. Beckham, Duke University Medical Center & Durham Veterans Affairs Medical Center

Author Note: DCR, Department of Psychology and Neuroscience, Duke University, and Center on Autobiographical Memory Research, Aarhus University. SAD and CMO, Department of Psychology and Neuroscience, Duke University; NMD and ACG, Psychology Department, University of Memphis. JCB, Department of Psychiatry and Behavioral Sciences, Duke University Medical Center and the Mid-Atlantic Research Education and Clinical Center, Durham Veterans Affairs Medical Center. This study was funded by the National Institute of Mental Health (R01-MH066079); the Coh-Metrix measures by the Institute of Education Sciences (R305G020018, R305A080589) and the Minerva Initiative, Air Force Office of Scientific Research, Department of Defense (FA9550-14-0308); the Danish National Research Foundation (DNRF93); and partial support to JCB by a VA Clinical Sciences Research and Development through a Career Scientist Award. This material is the result of work supported in part with resources and the use of facilities at the Durham, NC, VAMC and NIH. The contents do not represent the views of NIH, the U.S. Department of Veterans Affairs, or the United States Government. Address correspondence to David C. Rubin, Department of Psychology and Neuroscience, Duke University, Box 90086, Durham, NC 27708. Email: david.rubin@duke.edu

Abstract

We examined the coherence of trauma memories in a trauma-exposed community sample of 30 adults with and 30 without PTSD. The groups had similar categories of traumas and were matched on multiple factors that could affect the coherence of memories. We compared the transcribed oral trauma memories of participants with their most important and most positive memories. A comprehensive set of 28 measures of coherence including 3 ratings by the participants, 7 ratings by outside raters, and 18 computer-scored measures, provided a variety of approaches to defining and measuring coherence. A MANOVA indicated differences in coherence among the trauma, important, and positive memories, but not between the diagnostic groups or their interaction with these memory types. Most differences were small in magnitude; in some cases, the trauma memories were more, rather than less, coherent than the control memories. Where differences existed, the results agreed with the existing literature, suggesting that factors other than the incoherence of trauma memories are most likely to be central to the maintenance of PTSD and thus its treatment.

Keywords: Posttraumatic Stress Disorder; Acute Stress Disorder, narrative, coherence

General Scientific Summary: We used a battery of 28 different measures of narrative coherence drawn from educational research, developmental psychology, autobiographical memory research, and clinical psychology to investigate the coherence of trauma memories in PTSD. We found that for most measures trauma memories were as coherent as very important and very positive memories, and we found no evidence that people with PTSD differ on how coherent their memories were when compared to people without PTSD. Thus, counter to some views, incoherent trauma memories do not seem to be a common property of PTSD.

Participant, Rater, and Computer Measures of Coherence in Posttraumatic Stress Disorder

Since the formulation of the posttraumatic stress disorder (PTSD) diagnosis, researchers have differed on whether memories of traumas in people with PTSD are especially incoherent, incomplete, and fragmented. This *incoherence issue* remains unresolved. The claim that trauma memories are especially incoherent in people with PTSD justifies incoherence as a symptom in the diagnosis of PTSD and as a mechanism that produces and maintains PTSD. Mechanisms that have been suggested specifically to operate more strongly after a trauma in people who develop PTSD include impoverished encoding, active repression, dissociation of trauma memories, and the reduction of conceptual and verbal processes combined with increases of sensory or perceptual processes that produce sensory details that are not well integrated conceptually (e.g., Brewin, Dalgleish, & Joseph, 1996; Ehlers & Clark, 2000; Horowitz, 1976; for reviews from various perspectives see Brewin & Holmes, 2003; Dalgleish, 2004; McNally, 2003a, b; Porter & Birt, 2001; and Shobe & Kihlstrom, 1997).

According to an alternative view, incoherence can be understood in terms of cognitive and affective processes that have been developed to account for memory in general. These processes hold equally for all memories of stressful events regardless of the person remembering them and for all people regardless of the type of memory they are recalling. For example, trauma memories may be less coherent in all people regardless of diagnostic status because traumatic events are actually less coherent. Alternatively they could be more coherent due to the increased continuing effort expended in trying to understand the trauma. In either case, the effect should be similar in people with and without PTSD. Similarly, a wide range of memories may be less coherent in people with PTSD because PTSD affects cognitive abilities. The combination of these memory-specific and individual-differences processes is assumed to be

additive and large enough to account for any observed incoherence in the memories of traumas in people with PTSD (Rubin, 2011; Rubin, Boals, & Berntsen, 2008; Rubin, Dennis, & Beckham, 2011). If these memory-specific and person-specific processes did not interact in a way that was especially powerful for trauma memories in people with PTSD, there would be no reason to consider the incoherence of trauma memories especially important for PTSD. Rather, negatively emotional events would have similar properties in all people and people with PTSD would have general changes in cognitive processes.

To investigate these alternative views, the concept of coherence needs to be examined in more detail. Coherence is not a concept that is restricted to or is well defined in the PTSD literature. Diagnostic manuals give little guidance on how to measure incoherence, so PTSD researchers have developed a multitude of measures. However, these measures have not been compared to one another or calibrated outside of PTSD (or even within studies in which they are dependent measures). This can be a serious problem because memories can be incoherent in some ways and not others. Thus, we cast a wide net in our selection of measures to include in the present study in order to have the greatest chance of finding any way in which trauma memories in people with PTSD are incoherent. We include measures from PTSD research, and we add measures that have been used in other contexts and so have known properties outside any role in PTSD.

In particular, we include measures from autobiographical memory research, such as participant ratings of whether their memory is a coherent story, or comes in pieces with parts missing, or has a known setting, measures which have also been used in PTSD research (Rubin et al., 2011). We include a system from developmental psychology where the ability to narrate a coherent telling of an event increases dramatically until adulthood, allowing a direct way to

calibrate the system (Reese et al., 2011). We adapt questions given to patients with neuropsychological damage (Greenberg & Rubin, 2003) by asking the same questions of raters provided with narratives. These include knowing the goals and emotions of the narrator, which are basic to showing that the narrative is coherent at a more abstract level than has been used in the PTSD literature. We borrow measures from educational research that are used to calibrate texts on how easy they are to understand and to grade student essays (McNamara, Graesser, McCarthy, & Cai, 2014). We use counts of the frequencies of various classes of words developed to measure how people are coping with stressful events in the expressive writing method (Pennebaker, Booth, & Francis, 2007). All of these measures address different aspects of coherence and all have been used extensively so that much is known about their relations to coherence as measured outside of PTSD research.

In addition to comparing a wide variety of coherence measures in a trauma-exposed sample, we ask two different questions about the incoherence of trauma memories in people with PTSD. First, we ask the standard question of whether we can show differences in the incoherence of trauma memories between people with and without PTSD. Second, we ask whether these differences in incoherence are substantial enough to be part of a causal mechanism that produces and maintains PTSD. Providing strict conceptual criteria for this second question is more difficult; however, we can define incoherence operationally as being out of the range of normal behavior to the extent that it would be classified as a symptom by a well-trained clinician. At this level, incoherence could be the basis of either the inability-to-recall-an-important-aspect-of-the trauma symptom of PTSD or one of the dissociation symptoms of Acute Stress Disorder (ASD; American Psychiatric Association, 2000), or it can be a cause of other

symptoms that can be measured independently of coherence as is claimed by some theoretical approaches (e.g., Horowitz, 1976).

Considerable data already exist on the incoherence issue from three sources: the inclusion of the incoherence of trauma memories in the PTSD diagnosis, the overall level of the incoherence of trauma memories in PTSD, and comparisons of the incoherence of trauma memories with non-trauma memories in populations with and without PTSD.

The first source of information for the incoherence issue is the diagnosis itself. The symptoms of PTSD have been translated in a fairly literal fashion into many PTSD diagnostic and screening instruments for which the psychometric properties of the individual symptoms have been analyzed. The DSM-IV-TR, which was used in the current study, includes the "inability to recall an important aspect of the trauma" as symptom C3 (American Psychiatric Association, 2000, pp. 467–468). The DSM-5 changed this wording slightly; "trauma" became "traumatic event(s)" and "typically due to dissociative amnesia and not to other factors such as head injury, alcohol, or drugs" was added (American Psychiatric Association, 2013, p. 271). The DSM-5 also moved this item from an avoidance, or C, symptom to a newly added category of negative alterations in cognition and mood, which are termed D symptoms. Using these guidelines, a clinician must determine whether a person cannot recall an important aspect of an event to a degree beyond the normal range. If so, the memory of that event can be considered incoherent. Invoking dissociative amnesia as a cause also indicates incoherence (Berntsen & Rubin, 2014). However, two meta-analyses demonstrated that the psychometric properties of the C3, inability-to-recall-an-important-aspect-of-the-trauma, symptom did not support the view that memories of traumas in PTSD are highly correlated with the other PTSD symptoms.

Rubin, Berntsen, and Bohni (2008, Table 4) reviewed studies containing 35 separate analyses that investigated the underlying factor structure of the 17 symptoms of the DSM-IV-TR diagnosis. The studies involved a wide variety of subject populations, including populations both with and without a clinical diagnosis of PTSD. Different types of measures were used for the 17 symptoms, including both self-report measures and structured clinical interviews. The factor analyses varied and included exploratory and confirmatory analyses and 2-, 3-, and 4-factor solutions. Across these studies, the results for the C3 symptom were similar: The magnitude of the loading of the C3 symptom in the majority of analyses had a rank of 15, 16, or 17 among the 17 symptoms. The C3 often had the lowest loading, often much lower than and out of the range of, the rest of the items. A study by Foa, Riggs, and Gershuny (1995) could not be included because the C3 loaded on its own factor and, thus, the study removed it from their factor analysis. In contrast, a study by Stewart, Conrod, Pihl, and Dongier (1999) reported that the C3 ranked highest among the 17 symptoms. However, participants for this study were selected because of their substance abuse, not their PTSD. The changes in awareness and memory that often accompany substance abuse provide an alternative explanation for the correlation of the C3 with other PTSD symptoms, one explicitly excluded from the DSM-5.

Yufik and Simms (2010) obtained, combined, and reanalyzed original data from 40 studies, which included a total of 14,827 participants. They produced two preferred models, one of which had four factors that approximated the DSM-5 symptom categories. For both models, the C3 loaded at .53, whereas the remaining 16 symptoms loaded between .71 and .87, indicating that the C3 accounted for 37 to 56% as much variance as the other 16 items. Similar results exist for the DSM-5 (Gentes et al., in press). Thus, the psychometric properties of the symptom most directly related to the incoherence issue is an outlier, with lower loadings than other symptoms.

It is possible for an item that does not correlate with other items on a test to still be useful in diagnosis. Assume that dissociation was a co-occurring disorder or tendency. This assumption would allow the inability-to-recall-an-important-aspect-of-the-trauma symptom to have low loadings in factor analyses and for its explicit association with dissociation in the DSM-5 to be maintained. This assumption would also be consistent with the claims being made here, according to which many forms of dissociation would lead to a lack of coherence, but that such forms of dissociation are separate mechanisms that need not be an integral part of the diagnosis of PTSD because they are not present in the vast majority of recalls of trauma memories. In addition, this assumption would be consistent with the inability-to-recall-an-important-aspect-of-the-trauma symptom not being required for a PTSD diagnosis, but for that same diagnosis to be specified as *with dissociative symptoms* if one of the two dissociation symptoms that are not part of the PTSD diagnosis itself but that are listed in the *with dissociative symptoms* section of the DSM-5 PTSD diagnosis is present.

The second source of information on the incoherence issue is the overall level of the incoherence of trauma memories in PTSD. As incoherence is included in the diagnosis of PTSD and is considered important in the production and maintenance of PTSD, it should be fairly easy to observe in experimental studies. We searched for studies of groups of individuals in which the trauma memories of participants with PTSD were incoherent to the degree that would be needed to argue for incoherence as an explanatory mechanism. Because the theoretical concepts of coherence, incoherence, and fragmentation are hard to define and their operational definition is often not well justified (O'Kearney & Perrott, 2006), we searched for studies that had measures with clear face validity and interpretations, such as scales of incoherence defined by the anchors of 0 as extremely coherent and 10 as extremely incoherent. We also searched for

studies without such easy-to-interpret measures but that included a comparison to memories that should not be especially incoherent, such as non-trauma memories in participants with PTSD or trauma memories in participants without PTSD. We could find no experimental study that met these criteria that had clearly incoherent memories.

As examples of studies that had measures with clear face validity and interpretations, we start with results from two research groups that consider the incoherence of trauma memories as an important factor in PTSD. Halligan, Micheal, Clark, and Ehlers (2003) had assault victims rate their own trauma memory on a 0 to 4 scale of disorganization. In Study 1, three groups of participants, those with current PTSD, those who no longer had PTSD, and those who never had PTSD had ratings of 1.2 (*SD* = 1.2), 0.9 (*SD* = 0.8), and 0.4 (*SD* = 0.6) on their scale, respectively. In Study 2, trauma and non-trauma memories were compared and had means of 0.69 (*SD* = 0.84) and 0.42 (*SD* = 0.56), respectively. Thus, although the effects were in the direction that would be expected, on average, the participants in both studies rated their trauma memories as quite coherent. Jones, Harvey, and Brewin (2007) found differences between people with and without PTSD for their trauma memory of a road traffic accident on an experimenter-rated measure of global (in)coherence. The scale ranged from 0 (extremely coherent) to 10 (extremely incoherent). The mean ratings averaged over three testing points for the trauma memories of participants with PTSD were 2.09 if participants did not have traumatic brain injury and 1.69 if they did. For participants without PTSD these values were 0.75 and 0.93, respectively. The average standard deviation calculated from the square root of the average variances were 2.18, 1.00, 0.66, and 0.82, respectively. Because all of the participants were victims of road traffic accidents, some incoherence might be due to the reported or subclinical brain injury. However, none of the values indicated incoherence on the 11-point scale. In both

of these papers, each of which used scales starting at zero, the standard deviations of each measure were roughly equivalent to the means of the measure.

The third source of information on the incoherence issue compares the trauma memories of people with PTSD both to their non-trauma memories and to the trauma and non-trauma memories of people without PTSD. Many studies find that for some conditions and measures of coherence, trauma memories can be more or less coherent than non-trauma memories, or that people with PTSD can have more or less coherent trauma memories. The issue here, however, is whether people with PTSD have especially incoherent memories for their trauma(s); this requires both a comparison across memory types and across participants with and without PTSD. Few studies report data that allows this comparison and none seem to show that participants with PTSD have especially incoherent trauma memories.

The Halligan et al. (2003) Study 2, just reviewed, found an interaction in which participants who had PTSD at any of four testing points compared to participants who did not reported more self-rated incoherence for trauma versus non-trauma memories, but as noted earlier, none of the values indicated incoherence. Jelinek, Randjbar, Seifert, Kellner, and Moritz (2009) found a similar interaction for one rater-coded measure but not for another rater-coded measure or a self-reported coherence questionnaire. Römisch, Leban, Habermas, and Döll-Hentschker (2014) found more fragmentation in their analysis of distressing, trauma-like memories than for memories involving anger or happiness, but these effects did not interact with whether participants had PTSD or not.

In one study from our group, trauma exposed participants with and without PTSD wrote narratives of their trauma and of their most important and most positive events that occurred within a year of their trauma (Rubin, 2011). There were three rater measures of general

coherence, which are used in modified form here. In addition, the participants provided a measure of coherence which was the average of their ratings on six scales, three of which are included as individual measures here. For these measures, there was one significant effect: a main effect in which participants with PTSD rated their own narratives as more disorganized than participants without PTSD, with means of 1.80 (*SD* = .32) and 1.51 (*SD* = .28), respectively, on a scale of 1 (none) to 7 (almost all). There were no effects of memory type or of a memory type by PTSD interaction. The study also included computer scored measures of the frequency of cognitive mechanisms, insight, and causal words from the Linguistic Inquiry Word Count (LIWC; Pennebaker et al., 2007) that measure coherence. These measures varied with memory type but not with PTSD or the interaction of diagnostic group and memory type.

In another study from our group, 75 participants with and 42 without PTSD each rated their 3 most stressful, 3 most positive, 7 most important, and 15 word-cued memories on a number of scales (Rubin et al., 2011). Among 18 rating scales were two measures of coherence: story and pieces. The story measure asked whether the memory was recalled as a coherent story and pieces asked whether the memory was recalled in pieces with missing bits. For all four types of memories, the PTSD group had numerically higher scores than the control group on both the story and the pieces scale, even though the scales have different conceptual directions in terms of coherence. It is possible that the more one thinks about one's memories, the more coherent they become, but also the more often gaps are found. For stressful memories, pieces was significantly higher for the PTSD than the control group, with means of 3.68 (*SD* = 1.95) versus 2.93 (*SD* = 1.82), on a scale of 1 (not at all) to 7 (completely). They were not significantly higher for positive memories, which had means of 3.18 (*SD* = 2.09) versus 2.89 (*SD* = 1.93), leading to the one significant interaction between group and stressful versus each of the three types of control

memories. However, there was no indication of high levels of incoherence for trauma memories in the PTSD group.

Participants also recorded involuntary memories as they occurred for two weeks following the experiment just described, and then recorded a voluntary memory from approximately the same time period as the involuntary memory. Again the PTSD group rated story higher, but here there was no difference for pieces. There were no differences for voluntary versus involuntary memories and no interactions for the story and pieces scales. When memories related and unrelated to their traumas were compared, the only significant effect in the story and pieces scales was that events related to the trauma were rated higher on story. There were no interactions with PTSD group, involuntary versus voluntary retrieval, or the combined three-way interactions.

A similar experiment was conducted with 115 undergraduates who were high or low on PTSD symptom severity (Rubin, Boals, & Berntsen, 2008) and produced results similar to Rubin et al. (2011) though the effects were often smaller, most likely because the range of PTSD symptom severity was smaller. In both studies, there were significant effects of other self-ratings, especially of emotional intensity and emotional reactions, and retrospective reports of the frequency of voluntary and involuntary rehearsal, and of the event's centrality to the life story. Thus, overall, the results are replicable, but they offer little support for large differences on coherence in either voluntary or involuntary memories, and the differences that exist are in some cases in the direction of more coherence for trauma memories.

A recent review of research on memory disorganization and fragmentation in ASD and PTSD also discusses empirical findings relevant to the issue of incoherence (Brewin, 2014). Synthesizing data from across nine studies (reported in 8 articles), the author concludes, "A

considerable amount of evidence now strongly favors the claims that in samples suffering from ASD or PTSD trauma memories" include "fragmentation or disorganization accompanying voluntary recall," (Brewin, 2014, p .78). However, in our own review of the studies included in Brewin (2014), we found little support for the claim that incoherence is especially severe in the trauma memories of people with PTSD. We discussed the findings of three of the eight articles reviewed by Brewin (2014) previously in this paper. To summarize in brief here, Jelinek et al., (2009) reported a memory type by PTSD diagnosis interaction for one of three measures of incoherence, but no interactions were found for the other two incoherence measures. Halligan et al. (2003) and Jones et al. (2007) reported main effects for PTSD, but the mean values on the incoherence scales used in these studies were all in the coherent as opposed to incoherent range. Halligan et al. (2003) also report a memory type by PTSD diagnosis interaction for disorganization again with all mean values in the coherent range. Moreover, in this study participants were classified into the PTSD versus the non-PTSD group based on having PTSD at any of four time periods, not necessarily at the time period when the disorganization ratings were obtained.

Of the five remaining studies, two (Berntsen, Willert, & Rubin, 2003; Rubin, Feldman, & Beckham, 2004) showed no significant differences. Two other studies showed significant differences using an ASD diagnosis (Harvey & Bryant, 1999; Salmond et al., 2011). However, the diagnosis of ASD cannot be used as empirical evidence for incoherence because the ASD diagnosis requires dissociation which includes symptoms likely to implicate incoherence. In particular, in adults, ASD requires at least three of the following five dissociation symptoms for a diagnosis: (1) numbing, which unlike the other 4 symptoms does not implicate incoherence, (2) reduced awareness, (3) depersonalization, (4) derealization, and (5) amnesia. Thus, having an

incoherent trauma narrative is required and cannot be considered an empirical finding; the incoherence issue we are arguing against in PTSD is assumed to be true for ASD. Simply put, the claim that people with ASD have incoherent memories of their stressful event is true by definition and so presenting it as an empirical finding is circular. One of these studies (Salmond et al., 2011) was for children where the diagnosis does not require the dissociation symptoms but it used supplemental dissociative questions to facilitate diagnosis.

The remaining study included "An exploratory investigation" in its title and concluded that, "Results provided only weak evidence of an association between dissociative trauma narrative themes and PTSD symptoms." (Kenardy et al., 2007, p.456). Thus, a review of the same basic issue by a distinguished senior researcher with a different theoretical perspective, in our view, uncovered little support for the claim that incoherence is especially severe in people with PTSD. Both the differences in interpretation and the small number of studies found that could possibly be seen as supporting incoherence are striking given that the issue of incoherence has existed since the beginning of the PTSD diagnosis.

The present study

The ideal study to provide evidence that might help to resolve the incoherence issue would have: 1) a variety of measures of incoherence to ensure it is not missing measures that would produce different results, a full two by two design with both 2) memories for traumatic and non-traumatic events and 3) clinically-diagnosed participants and matched control participants who 4) have experienced similar traumas, and 5) who do not vary on clinical diagnoses other than PTSD that could produce incoherence. The present study includes all 5 of these components. The coherence of narratives is affected by factors such as education and socioeconomic status and by many disorders including those that involve dissociation, though

the only aspect of dissociation that would be of importance here is incoherence (Giesbrecht, Lynn, Lilienfeld, & Merckelbach, 2008). We therefore balanced these characteristics across groups so that they would not affect the results. To ensure a variety of measures of incoherence, we measured the coherence of memories from 1) the perspective of the person who experienced and recalled the event and thus had full access to the memory beyond what they reported, 2) the perspective of trained undergraduate raters who had only the transcribed memory, and 3) the Coh-Metrix and LIWC computer programs, which also had only the transcribed memory.

Methods

Participants

Adults from the community were screened as part of a larger study by clinicians who were trained and worked regularly in a research setting. Participants were recruited via advertising for a study on memory for stressful or traumatic events and how they differ from more everyday memories. The Clinician Administered PTSD Scale (CAPS, Blake et al., 1990) was used to determine PTSD diagnostic status. Current diagnoses were determined by a 1-month time frame for PTSD. Exclusion criteria included current alcohol or other substance dependence/abuse measured by self-report and urine drug screen, neurological damage (including head trauma or disease), and current psychotic disorder or bipolar disorder with active manic symptoms based on the Structured Clinical Interview for DSM-IV Diagnosis (SCID; First, Spitzer, Gibbon, & Williams, 1996). Participants were also excluded if they were medically unstable or if they could not complete the study procedures. Nicotine dependence was allowed. To be included in the study, all participants were required to have met the DSM-IV-TR PTSD diagnostic A-criterion of having experienced, witnessed, or confronted “an event or events that involved actual or threatened death or injury, or a threat to the physical integrity of the self or

others" (American Psychiatric Association, 2000, p. 467). From a pool of 101 eligible individuals who met our inclusion and exclusion criteria, of which 41 had current PTSD and 60 had no history of current or lifetime PTSD, 30 participants with PTSD and 30 control participants were randomly selected. The two groups were then compared on their average Hollingshead score, BDI score, and education level, as well as the number of females, minorities, veterans, and percentage with major depressive disorder (MDD), other psychiatric disorders (including anxiety disorders), and histories of substance dependence/abuse diagnoses. When differences between the two groups were found, in an iterative fashion, participants with high (or low) scores on a given measure were removed and quasi-randomly replaced with another individual from the pool of eligible participants until no participants remained who could further reduce the differences between groups on these measures. Information on the PTSD group and control group is presented in Table 1.

Procedure

All administration of the protocol was done individually with each participant by trained employees in the Durham Veterans Affairs Medical Center. This staff attended regular sessions to ensure they were conforming to criteria of the study and administration of standard instruments. Participants in our sample were enrolled in a larger study for which they were compensated $500 for completing 6 total sessions. During the first session, participants were first screened for any current illicit drug use. Then, they were administered the Traumatic Life Events Questionnaire (Kubany et al., 2000), CAPS, and Structured Clinical Interview for DSM Disorders, and were queried about current medications and treatment history. During the second session, the AMQ (Rubin, Schrauf, & Greenberg, 2003, 2004) was administered for 15 cue words, and the BDI-II (Beck, Steer, & Brown, 1996), DES (Bernstein & Putnam, 1986), and

other instruments were administered. The third session is key to our current investigation. During that session, participants orally narrated their three most negative, stressful, or traumatic life events followed by their three most positive and finally their three most important life events that were not among the most traumatic or positive. After each event they completed the AMQ, Centrality of Event Scale (Berntsen & Rubin, 2006, 2007), and PCL (Weathers, Litz, Huska, & Keane, 1994) for that event. The order of narration was chosen to obtain data on the trauma memories first as these were the most crucial. The positive memories followed both to reduce any lingering mood effects of recalling the trauma memories and to prevent any of the memories of the three most positive events from being selected as among the three most important memories. We considered the positive and important memories as reasonable comparisons to the trauma memories due to their high emotional intensity but opposite valence and their similar impact on the participants' lives, respectively. They were not of interest on their own and so we accepted the loss of any advantages of counterbalancing in order to obtain narratives of the trauma memories that were not influenced by the later memories. After later sessions participants were then debriefed and referred for support if necessary. The narratives were transcribed prior to coding.

Initial Data Processing

All links to individual participants were replaced by arbitrary participant numbers to ensure confidentiality. The oral narratives were audiotaped, provided with participant numbers and then transcribed verbatim into computer files. Oral non-fluencies and fillers were then recoded for computer scoring. The transcriptions were then examined to remove any remaining information that could possibly allow the participant to be identified. Such information was changed to similar words with different referents so that the coherence coding would produce

reasonable results. For the neutral observer ratings, the individual memories of each type were given new random numbers so that the raters would not know which narratives came from the same participant. The neutral observer ratings of the narrative coherence coding scheme (NaCC) and the global coherence measures were rated by undergraduate research assistants. We used α alpha to estimate how well our raters would agree with a new set of an equal number of raters. The raters were trained on narratives that were not part of this study. Once they understood the instructions to their satisfaction, they rated the entire set of narratives from this study independently of each other. We averaged the ratings of the three narratives of each memory type from each participant for all analyses including reporting reliabilities.

Instruments used to select and describe our sample

Beck Depression Inventory (BDI-II). The BDI-II (Beck et al., 1996) is the sum of 21 items rated on a 0 to 3 scale. The measure has good internal consistency (α = .91 to .93) and .93 one-week test-retest reliability (Beck et al., 1996). Arnau, Meager, Norris, and Bramson (2001) demonstrated discriminate validity for the BDI-II in a primary care setting with significant differences in mean BDI-II scores between participants with and without a MDD diagnoses.

Clinician-Administered PTSD Scale (CAPS). The CAPS (Blake et al., 1990; Weathers, Keane, & Davidson, 2001) is a clinical structured interview that assesses the frequency and intensity of the 17 DSM-IV PTSD diagnostic symptoms. The CAPS has strong psychometric properties including high internal consistency (α = .94) and good convergent validity with other PTSD scales. The 0 to 4 severity and intensity scores for all 17 symptoms are summed to provide an overall severity score, with higher scores indicating a more severe PTSD diagnosis. Presence of each symptom was determined using the frequency ≥ 1 and intensity ≥ 2 rule (Weathers et al., 2001) which requires a symptom to be endorsed at a frequency of at least

once per month and intensity of at least moderate impairment or distress to be counted as present.

The Dissociative Experience Scale (DES). The DES (Bernstein & Putnam, 1986; Carlson & Putnam, 1993) is the average of 28-items that measure normal to pathological dissociative experiences, each rated on a 0 to 100 scale. Test-retest reliabilities are above .79 (Carlson & Putnam, 1993). The DES correlates highly with the severity of dissociative symptoms assessed in structured clinical interviews (i.e., SCID, *r*s = .58 - .78; Draijer & Boon, 1993) and by alternative measures of dissociation (e.g., Perceptual Alteration Scale, $r = .82$; Nadon, Hoyt, Register, & Kihlstrom, 1991).

Hollingshead Index of Socioeconomic Status (Hollingshead SES). On the Hollingshead SES (Hollingshead, 1975), lower scores indicate higher socioeconomic status; scores range from 11 (upper class) to 77 (lower class).

PTSD symptom severity (PCL). The PTSD Check List-Stressor Specific Version (Weathers et al., 1994) is a 17-item measure of PTSD symptoms in reference to a specific event. Using 5-point scales (1 = not at all, 5 = extremely), respondents indicate the extent to which a specific event produced each of the B, C, and D DSM-IV-TR symptoms of PTSD during the previous month. The PCL has strong psychometric properties (Blanchard, Jones-Alexander, Buckley, & Forneris, 1996) and has been shown to have high diagnostic agreement with the CAPS ($r = .93$; Blake et al., 1990). Respondents completed the PCL-S for each of their three trauma memories.

Measures of Coherence

Autobiographical Memory Questionnaire (AMQ). The AMQ (Rubin et al., 2003, 2004) includes a series of questions concerning processes involved in remembering an event.

Participants completed the AMQ for each of their 9 memories. We analyzed three items from the AMQ relevant to narrative coherence. Setting, "While remembering the event, I know the setting where it occurred" was rated on a scale from 1 (not at all) to 7 (as if it were happening now). Story, "While remembering the event, it comes to me in words or in pictures as a coherent story or episode and not as an isolated fact, observation, or scene" and pieces "My memory comes in pieces with missing bits" were rated on a scale from 1 (not at all) to 7 (completely).

Narrative Coherence Coding Scheme (NaCCs). The NaCCs (Morris, Baker-Ward, & Bauer, 2010; Reese, et al., 2011) is comprised of three coherence dimensions: context, chronology, and theme rated on a 4-point scale from 0 to 3. Context measures the degree to which the narrator provides information needed to locate the event in space and time. Chronology concerns the degree to which the narrator provides sufficient information to place the actions in the event on a time line. Theme measures the degree to which the narrator substantially develops the narrative using causal linkages, interpretations, and elaborations; describes a resolution that relates the event to other autobiographical experiences, self-concept, or identity; and describes a sense of closure. The NaCCs has been used to assess the development of these three components of narrative coherence across the lifespan for autobiographical memories (Chen, McNally, Wang, & Reese, 2012; Larkina & Bauer, 2012; Reese et al., 2011) and laboratory events (Bauer et al., 2012). Relevant to the current study, the NaCCs has also been used to quantify coherence in intensely positive and negative narratives in healthy undergraduates (Waters, Bohanek, Marin, & Fivush, 2013) and in narratives of stressful events in participants with and without a history of abuse (Greenhoot, Sun, Bunnel, & Lindboe, 2013). This rating scheme moves beyond components at the word or sentence level, and instead examines the coherence of the entire narrative. Five undergraduate raters independently

provided values for these measures; *α*s for context, chronology, and theme were .90, .72, and .70, respectively.

Global Coherence measures. These four were similar to measures used in Greenberg and Rubin (2003) and are intended to test a more abstract level of narrative skills than the measures of the NaCCs. The first two assess narrative abilities tested in neuropsychological assessments. Narrator asks "Do you understand more about who the narrator is, how they deal with the world, and how they must have felt?" Emotion asks "Does the text evoke an emotional reaction in you and/or your empathy for the narrator?" Both were rated on a scale of 1 (not at all), 2 (not really), 3 (a little bit), 4 (a moderate amount) 5 (yes, quite a bit), 6 (yes, very much so), and 7 (yes, as much as any text of about this length could). Percent irrelevant asks "How much of the writing does not add to the development of the narrative?" and was rated on a scale of 0% to 100% with markers at 10% intervals. Disorganization is based on Halligan et al. (2003). It asks "Rather than concentrating on the narrator, your emotions, or amount of irrelevant content rated in questions 1, 2, and 3, step back and look at the narrative as a story that is meant to communicate the main ideas of what happened at an event." and was rated on a scale from 0 (not at all disorganized) to 10 (extremely disorganized) with the numbers 0 through 10 listed as markers. Four coders rated the measures; *α*s for narrator, emotion, percent irrelevant, and disorganization were .76, .87, .85, and .87, respectively.

Coh-Metrix. Coh-Metrix is an automated linguistics facility that analyzes higher-level features of language and discourse, which has been widely validated (http://cohmetrix.com; Graesser, McNamara, Louwerse, & Cai, 2004; McNamara, et al., 2014; McNamara, Louwerse, McCarthy, & Graesser, 2010). Coh-Metrix includes sophisticated methods of natural language processing, such as syntactic parsing and cohesion computation, to capture deeper language

characteristics. It provides measures at multiple levels, including genre, cohesion, syntax, and words, as well as other characteristics of language and discourse. Here, we provide only a brief description of the measures of cohesion, principal component scores, and composite formality used in the current study.

Coh-Metrix principal component measures. These measures were derived from a Varimax (orthogonal) principal component analysis conducted during the development of the measures prior to this study. The orthogonal solution was preferred because it yielded a very small percentage of cross-loadings across components that exceeded |.30| and because oblique solutions added minimal increments. Eight orthogonal dimensions accounted for 67% of the variance in text variability in a large corpus of over 37,000 texts (Graesser, McNamara, & Kulikowich, 2011). Of these, the first five components, which accounted for 54% of the variance, were used and are listed below. These Coh-Metrix dimensions align with multilevel theoretical frameworks of language and discourse (Graesser & McNamara, 2011; Kintsch, 1998; Snow, 2002), which distinguish representations of meaning, structures, strategies, and cognitive processes at different levels of discourse.

Narrativity. Higher scores indicate high word familiarity, oral language, and greater ease of comprehension affiliated with everyday oral conversation.

Syntactic ease. Higher scores indicate that the sentences have fewer words and simpler, more familiar syntactic structures, which are easier to process and understand.

Word concreteness. Higher scores indicate more concrete language which evokes mental images and thus should be more meaningful to the reader.

Referential cohesion. A low referential cohesion score indicates that a text has few overlapping content words and ideas.

Deep cohesion. The extent to which the ideas in a text are connected with causal, intentional, or temporal connectives at the situation model level. A high deep cohesion score indicates that the narrator has a more complex and coherent representation of a mental model of the story.

Other Coh-Metrix measures.

Content word overlap. This measure considers the proportion of content words that overlap between pairs of sentences. It is calculated by dividing the number of content words in each pair of sentences that overlap by the total number of content words.

Latent Semantic Analysis. This measure considers the conceptual similarity between adjacent text constituents.

Connectives. These are sets of individual words that have the special function of connecting clauses and other constituents in the text base. The categories of connectives in Coh-Metrix that are most related to coherence are temporal, extended temporal, causal, adversative, logical, and additive. The incidence of each word class is computed as the number of occurrences per 1000 words. A higher incidence of connectives increases cohesion with the narrative and also indicates that the narrator's representation of events is more coherent.

Linguistic Inquiry Word Count (LIWC). The LIWC (Pennebaker et al., 2007) analyzes written or transcribed text and calculates the percentage of different types of words used. We focused on categories related to coherence (cognitive mechanisms, insight, cause, nonfluencies, filler words; Pennebaker, Mehl, & Niederhoffer, 2003; Rubin, 2011; Rude, Gortner, Pennebaker, 2004).

Relations among the measures of coherence. In the introduction, we argued that coherence is a broad concept defined in many ways in different literatures and that there has been

little agreement among PTSD researchers as to how it should be measured, in part because the PTSD diagnosis offers little guidance at a detailed level. To examine whether our measures are related empirically, we report on three correlation matrices, one for each memory type, among our 28 measures of coherence. The percent of correlations whose magnitude was greater than .255, and thus were significant at the $p = .05$ level, for the trauma, positive, and important memories were 21%, 18%, and 21%. To examine how these correlations are distributed across the five categories of coherence measures shown in Tables 2 and 3, we examined the correlations of the individual measures within each category versus the correlation between the measures in each category and the measures in the other four categories. Because the results were similar we report the average percentage of significant correlations across all three memory types. The correlations among the AMQ measures were significant 33% of the time versus being significant with the measures in the other four categories 6% of the time. For the NaCCs, Global Coherence, Coh-Metrix, and LIWC these values were 11% versus 16%, 39% versus 18%, 35% versus 17%, and 20% versus 18%. Thus, correlations exist both within categories and between categories at substantial rates, with the exception of the self-rated AMQ measures when compared to the other ratings which were all rated by observers.

Results

Group and Trauma Characteristics

As shown in Table 1, participants in the PTSD and control groups were matched on minority status, gender, combat service, major depressive disorder, past substance abuse, age, and years of education. Compared to the control group, participants in the PTSD group had lower socioeconomic status (as indicated by higher Hollingshead's SES scores) and higher scores on the BDI-II and DES. We excluded participants with clinical disorders related to

extreme scores on these scales, but not to extreme values on the scales themselves, which allowed this difference to occur. The frequencies of the wide-range of criterion A-qualifying traumas participants reported during the diagnosis are also shown in Table 1. A chi-square analysis revealed no differences in the type of trauma experienced between the two groups. Thus, the groups were matched on, or due to the exclusion criteria were lacking, many factors that could cause incoherence outside of PTSD itself.

The Narrative Coherence of Trauma, Positive, and Important Memories

Table 2 presents the main findings of our study. There are 28 measures of coherence: 3 self-report measures, 7 neutral observer measures done by raters, 13 measures from Coh-Metrix, and 5 measures from the LIWC. The multivariate MANOVA used to reduce the risk of Type I and Type II errors among these 28 measures had a main effect of memory type ($F(53, 3) = 30.93$, $p = .008$), but not a main effect of group ($F(28, 31) = 1.46$, $p = .154$), or their interaction ($F(56, 3) = 4.50$, $p = .120$). The significant main effects of memory type are indicated in Table 2. Even if we relaxed the experiment-wise risk of Type I and Type II errors by not using a MANOVA, with one exception, all the univariate effects that would be significant at $p < .01$ level or lower would be effects of memory type, and all the effects that would survive a Bonferroni correction would be main effects of memory type.

Within the memory type effects, the comparisons of trauma memories with the two kinds of control memories are the main interest. The comparison of trauma with positive but not important memories was significant for 1 of the 12 main effects of memory type: AMQ pieces, $t(59) = 2.40$, $p < .05$. The comparison of trauma memories with important but not positive memories was significant for 2 of the 12 main effects of memory type: NaCCs theme, $t(59) = 3.54$, $p < .001$, and LIWC cause, $t(59) = 3.15$, $p < .01$. For the remaining 9 significant main

effects, trauma memories were significantly different from both positive and important memories: NaCCs chronology, $t(59) = 7.05$, $p < .0001$ and $t(59) = 5.49$, $p < .0001$, respectively; Global Coherence narrator, $t(59) = 3.20$, $p < .01$ and $t(59) = 2.30$, $p < .05$; emotion, $t(59) = 14.03$, $p < .0001$ and $t(59) = 12.69$, $p < .0001$, and percent irrelevant, $t(59) = 3.42$, $p < .01$ and $t(59) = 5.01$, $p < .0001$; Coh-Metrix word concreteness, $t(59) = 2.63$, $p < .05$ and $t(59) = 3.37$, $p < .001$, deep cohesion, $t(59) = 3.98$, $p < .001$ and $t(59) = 4.75$, $p < .0001$, extended temporal connectives, $t(59) = 5.27$, $p < .0001$ and $t(59) = 3.30$, $p < .01$, causal connectives, $t(59) = 4.49$, $p < .0001$ and $t(59) = 4.38$, $p < .0001$, and logical connectives, $t(59) = 3.36$, $p < .001$ and $t(59) = 3.16$, $p < .01$.

Of these 12 measures, the significant differences were split evenly with trauma memories being less coherent in 6 measures and more coherent in 6 measures. Trauma memories are less coherent than the control memories for AMQ pieces; NaCCs theme; Coh-Metrix deep cohesion, external temporal connectives, causal connectives, and logical connectives; and more coherent for NaCCs chronology; Global Coherence narrator, emotion, and percent irrelevant; Coh-Metrix concreteness; and LIWC cause. The mixed direction of these effects does not offer strong support for trauma memories being generally more incoherent than memories of positive and important events. Because these results are about the kind of event being narrated they can be used to understand which aspects of coherence vary more for different kinds of events. However, none of the significant results pertain to the effects of PTSD or how it interacts with different measures of coherence.

There are five empirical issues that need to be addressed to ensure our conclusions are valid. The first issue is to examine the post hoc power of our results. This is especially important because based on our review of the literature, the effect we expected to find is close to

a null hypothesis finding. Therefore we need to explore what genuine statistical effects we might have missed due to limited power and whether the effects we did find were chance occurrences. The second issue is to examine the relationship among the coherence measures we used to see if they were related empirically. Although our use of the concept of coherence and the MANOVA analysis can be justified on conceptual grounds, empirical support can be evaluated based on similarities among the measures. The third issue is that narratives of the different memory types varied in length and this could affect our measures. The fourth is that either one, two, or three of the three trauma memories from each participant were of traumatic events as defined by the DSM-IV-TR PTSD diagnostic A-criterion (American Psychiatric Association, 2000) that was in use when the study was conducted. An analysis restricted to trauma memories that meet the diagnostic criteria is needed to ensure that our results are not caused by mixing events that are traumatic as defined by the diagnosis with other negative, stressful events. Similarly, an analysis that included only the trauma on which the PTSD diagnosis was made would ensure that the trauma on which we base our groups produces results that do not differ from what we have just reported. The fifth issue is that although we tried to equate our PTSD and control group on all conventional demographic and relevant clinical characteristics except for the PTSD diagnosis and symptom severity, there were some residual differences as shown in Table 1. We therefore include these characteristics as covariates in an additional analysis. We address each of these five issues in turn.

Table 3 includes the post hoc power for all measures along with the number of participants that would be needed to observe a $p < .05$ level effect with a probability of .80. Excluding the 12 significant main effects of memory type noted in Table 2, the number of participants required to observe significant effects 80% of the time is substantial, with only one

main effect of group (i.e., for LIWC cognitive mechanisms) needing less than 100 participants and only three other effects (i.e., interaction effect for AMQ story, main effect of memory type for content word overlap, main effect of group for additive connectives) needing less than 200 participants. Thus, based on our sample and measures there seems to be little else that would emerge without a much larger sample. Of the 12 significant main effects of memory type, 6 have a post hoc power of .80 or above. Of these, 3 neutral observer measures are in the direction of trauma memories being more coherent than the comparison memories and 3 computer-scored Coh-Metrix measures are in the direction of trauma memories being less coherent than comparison memories, so the basic conclusions are not affected by this result. The remaining 6 measures have post hoc power between .38 and .73 and offer a similarly mixed picture with respect to the direction of the effect.

The number of words per narrative varied across memory type, but not with diagnosis or their interaction ($F(2, 116) = 4.19$, $p < .05$) with means of 436, 331, and 348 for the positive, important, and trauma memories. Theoretically, the length of the narratives should have their biggest effect on the neutral observer NaCCs and Global Coherence ratings because the only information the raters could base their judgments on were the transcribed narratives. In contrast, the participant AMQ ratings were not based on the narratives the participants produced, and the computer scoring methods have a correction for the number of words in each narrative. Empirically, when word count was correlated with the 28 measures shown in Tables 2 and 3 for the positive, important, and trauma memories separately, 12 correlations were significant at the .05 level, and all but one of these were in the neutral observer ratings. We therefore used word count as a covariate for the neutral observer ratings.

The MANCOVA for the seven neutral observer ratings with word count as a covariate had a main effect of memory type, $F(14, 220) = 11.33$ $p < .0001$, but no main effect of group, $F(7, 51) = 1.00$, $p = .441$, or their interaction, $F(14, 220) = .95$, $p = .501$. The significant covariate effects were theme, $t(58) = 2.17$, $p = .034$, narrator, $t(58) = 4.07$ $p < .001$, percent irrelevant, $t(58) = 9.01$, $p < .0001$, and disorganization, $t(58) = 3.42$, $p = .001$. The results of the univariate ANCOVAs with word count as a covariate had only minor changes compared to the previously reported results without word count covaried. The corrected $F(2,115)$s for the significant neutral observer ratings are: chronology, 20.64, $p < .0001$; theme, 10.83, $p < .0001$; narrator, 3.53, $p = .033$; emotion, 110.05, $p < .0001$; and percent irrelevant, 9.68 $p < .0001$. Thus, there was no change in the results that would affect our overall conclusions.

We examine whether the occurrence of the traumas as indexed by the PTSD diagnosis was having a serious effect on our results. To do this we first report on how many of the three trauma memories were A-traumas for each participant and then repeat our main analysis for only A-traumas. In the PTSD group, there were 15 people with 3 A-traumas, 11 with 2, and 4 with 1 A-trauma ($M = 2.37$, $SD = .72$) compared to the non-PTSD group which had 12 with 3 A-traumas, 7 with 2, and 11 with 1 ($M = 2.03$, $SD = .89$), $\chi^2(2) = 4.49$, $p = .106$; $t(58) = 1.60$, $p = .115$. Constraining the analysis to only the A-traumas reduces the reliability of our measures because there are fewer memories being considered. Similar to the analysis using all trauma memories, the MANOVA has a significant main effect of memory type, $F(56,3) = 17.13$, $p = .019$), but not of group or their interaction, $F(28, 31) = 1.37$, $p = .193$ and $F(56,3) = .59$, $p = .823$, respectively. The individual significant univariate effects of memory type have minor changes compared to those shown in Table 2 for all memories with some F-values increasing slightly (emotion, word concreteness, extended temporal connectives, causal connectives, and cause)

while others decreased slightly (pieces, chronology, theme, narrator, percent irrelevant, deep cohesion, and logical connectives) and two new measures becoming significant at the .05 level (disorganization, $F(2,116) = 3.30$ $p = .040$; content word overlap, $F(2,116) = 4.52$, $p = .013$).

When we examine only the index traumas, none of the MANOVA effects are significant (main effect of memory type: $F(56, 3) = 2.77$, $p = .218$; main effect of group: $F(28, 31) = 1.76$, $p = .329$; interaction: $F(56, 3) = 4.33$, $p = .126$). If we nonetheless examine the univariate main effects of memory type, there are minor changes that do not affect our overall results. All but four effects that were significant in the original analyses remain significant (chronology, theme, emotion, percent irrelevant, deep cohesion, extended temporal connectives, causal connectives, and cause) and there are three new significant effects (disorganization, $F(2, 116) = 3.09$, $p = .049$; insight, $F(2, 116) = 4.10$, $p = .019$; and content word overlap, $F(2, 116) = 3.62$, $p = .030$).

Finally, we examined whether our results were affected by differences in the three measures in Table 1 that we did not completely balance across the PTSD and the control groups: the Hollingshead SES, BDI-II, and DES. Given that dysphoria and dissociation as measured by the BDI-II and DES have been associated with PTSD and that in the DSM-5 (American Psychiatric Association, 2013) dysphoria is an official symptom cluster and dissociation is implicated in one symptom (i.e., the inability to remember important parts of the trauma), correcting for them can be seen as problematic in that they are inherent parts of the disorder. Nonetheless, for completeness we examine dysphoria and dissociation as covariates to examine their effects on the results. To achieve this, we added the Hollingshead SES, BDI-II, and DES as covariates to a MANOVA identical to the one described in our initial analysis. As in the initial analyses without covariates, the main effect of memory type was significant, $F(56, 3) = 30.93$, $p = .008$, but the main effect of group, $F(28, 28) = .98$, $p = .524$, and their interaction were not,

$F(56, 3) = 4.50$, $p = .120$. However, because the three covariates are individual differences that are the same for all memory types, they do not have an effect on our main findings. For the Hollingshead SES, the significant covariate effects were theme, $t(58) = -2.07$, $p = .043$; narrativity, $t(58) = 2.84$, $p = .006$; referential cohesion, $t(58) = 2.45$, $p = .017$; adversative connectives, $t(58) = -3.31$, $p = .002$; logical connectives, $t(58) = -2.60$, $p = .012$; and cognitive mechanism, $t(58) = -2.08$, $p = .042$. For the DES they were referential cohesion, $t(58) = -2.84$ $p = .006$ and content word overlap, $t(58) = -2.42$, $p = .019$. For the BDI-II, they were deep cohesion, $t(58) = 2.03$, $p = .047$; causal connectives, $t(58) = 2.03$, $p = .047$; and filler, $t(58) = 2.22$, $p = .031$.

Discussion

We examined the coherence of trauma memories in individuals with and without PTSD. We measured the coherence of memories for events in as many ways as we could find that have been used in the literature on coherence and added newer computer scoring methods developed in educational research. To provide baselines for these measures, we compared trauma memories to the participants' most important and most positive memories. We chose this approach because these memories share many properties with traumas, including importance to the participants' lives and intense emotions. To examine the effects of PTSD, and ideally only PTSD, we compared two samples that we matched as closely as we could on factors other than PTSD. The results, in simplest terms, were that no consistent differences were observed as a function of diagnosis or the interaction of diagnosis and memory type. Several differences in coherence were observed as a function of memory type; most were small in magnitude and they were as likely to be more coherent as less coherent in trauma memories.

Given the overall abundance of mixed and null findings, we need to ensure that our results are valid. A power analysis indicated that we had sufficient power to observe differences

in coherence between trauma and comparison memories and that the differences caused by diagnosis or its interaction with memory type were small enough that a much larger sample would be needed to see effects. Moreover, the differences in memory type were as likely to show that trauma memories were more coherent as less coherent. Most importantly, our results do not reflect a failure to replicate. Our findings are consistent with results from previous studies, so increasing power is not likely to produce results pointing to incoherence in the trauma memories of people with PTSD. In particular, the data presented here parallel the literature as a whole, which as reviewed in the introduction, is characterized by the absence of empirical demonstrations of high levels of incoherence in the trauma memories of groups with PTSD and more broadly in reviews of studies of narrative in PTSD, which use a variety of measures and sample a variety of populations (Crespo & Fernández-Lansac, in press; O'Kearney & Perrott, 2006). Although claims about incoherence depend on interpretations of the anchors of the scales used (Blanton & Jaccard, 2006), it is hard to see the reported mean levels of incoherence being at high levels. On a more detailed level, the individual results reported here are consistent with recent studies of narrative that compare differences in just population or memory type (e.g., Fivush, Sales, & Bohanek, 2008; Waters, Bohanek, Marin, & Fivush, 2013) and, in our view, with studies in a review which argued that voluntarily recalled trauma memories are more incoherent in PTSD (Brewin, 2014).

Our ability to draw conclusions from these mixed and mostly null findings is further strengthened by our finding that the pattern of results was not substantively affected by variations in depressive and dissociative symptomology or SES. Although efforts were made to select the PTSD and control groups so that the groups were similar on all demographic and relevant clinical characteristics except for a PTSD diagnosis and the level of PTSD symptom

severity, residual group differences emerged for these three variables. However, results from an analysis with these three variables added as covariates were substantively unchanged compared to those from the primary analysis: the main effect of memory type was significant but the main effect of group and their interaction was not. The consistency of the results across analyses with and without covariates suggests that our findings were not due to the effects of depressive symptoms, dissociative symptoms, or SES. Similarly, analyses restricted to trauma memories that met the diagnostic criteria and to the trauma on which the PTSD diagnosis was made were conducted to ensure that our results were not caused by mixing events that are traumatic as defined by the diagnosis with other negative, stressful events. These analyses revealed only minor differences that do not affect our conclusions, suggesting that our results were not explained by variations in severity of the trauma memory as defined by DSM-IV diagnostic criteria.

Another strength of our study was our use of a broad range of coherence measures drawn from multiple disciplines and areas of research including autobiographical memory studies, developmental psychology, neuropsychological assessment research, education research, and expressive writing. Sampling measures and methods of measuring coherence from multiple literatures was necessary because the concept of incoherence is poorly defined and operationalized in the PTSD literature and diagnosis. Despite the wide variety of measures used, analyses concerning the relations among these varied measures of coherence indicated a substantial convergence both within and between categories of measures, with the exception of the self-rated AMQ ratings. The low percentage of covariance between the AMQ ratings and the other categories of ratings was not unexpected, however, given that the AMQ ratings were subjective ratings of the memory narratives and the other categories of measures were computer

scored measures of coherence or ratings made by neutral observers. Thus these results provide evidence of convergent validity both with and between the categories of coherence measures tested in the present study.

We need to note that our results and theoretical claims do not speak to increases in coherence that occur during psychotherapy. Consider an early classic study by Foa, Molnar, and Cashman (1995). Patients with PTSD who had a trauma entered exposure therapy. The therapy included seven sessions with a trained clinician in which for 45 to 60 minutes, with eyes closed, they repeatedly imagined and narrated a trauma as vividly and in as much detail as possible in the present tense as if it were happening again. The patients were asked not only to describe what was happening as they imagined the event but also how they felt and what they were thinking, which are all tasks that should increase coherence. Patients decreased their symptoms of PTSD, anxiety, and depression, and their narratives became more coherent on a number of measures, though such increases in coherence are not always observed. Although not measured, it is also likely that, due to such exposure therapy, the patients over time perceived their traumas as less fear-provoking, less damaging to them, and perhaps less likely to occur in the future. It is also clear that such a procedure, even in a milder form and in less clinically skilled hands, can change the content and evaluation of memories and can even create memories for new events (e.g., Goff & Roediger, 1998; Hyman & Pentland, 1996). But it is not clear from any studies we can find whether increases in coherence alone cause the change, are a partial contributor along with the decreases in the negative effects of the trauma memory caused by exposure therapy, or are just an index that the trauma memory has changed in the desired direction. Under any of these possibilities, increases in the coherence of the trauma memory with therapy would correlate with decreases in PTSD symptoms.

This study has limitations. First, we tested only one sample of moderate size. Second, we did not equate the PTSD and control groups completely on factors that might influence coherence. In some cases, but not all, this is justified. For instance, dysphoria is involved in symptoms of PTSD and having PTSD and control groups matched on dysphoria may leave results that are not representative of PTSD (e.g., Meehl, 1971). Third, coherence is not an easy concept to operationalize and measure (McAdams, 2006). Although we tried to include as many measures of coherence as we could, it could still be the case that there is a particular kind of incoherence central to PTSD and trauma memories that we, and the literature we surveyed to assemble our measures, have not yet articulated.

We can only speculate as to why some clinicians find the trauma memories of people with PTSD to be generally more incoherent when more controlled studies do not. One possibility is the nature of the observations on which these generalizations are based. PTSD patients in therapy spend more time trying to understand their traumas than positive events. If they were asked to explore in depth a happy event from their past with the same expectations and social situation that occurs in therapy, they might find they also cannot recall important parts of the event and the organization of the event that is not set by convention may seem fragmented. It is also possible that even a small proportion of the patients with PTSD who have a dissociative disorder could suggest a general effect of incoherence related to PTSD. Finally, over the course of therapy, the repeated attempt to narrate the traumatic event in a way that makes it easier to understand and cope with should make its narration more coherent (e.g., Foa et al., 1995). In contrast, studies like those used here that compare trauma to control memories, often exclude or match people who are likely to dissociate, and contrast the results of people with PTSD to people

without the diagnosis. All of these potential factors may eliminate possible sources of incoherence.

There is a clear clinical implication to our work. In order to have PTSD, an individual must exhibit specific symptoms that result in "clinically significant distress or impairment in social, occupational, or other important areas of functioning" (American Psychiatric Association, 2013, p. 272). Here, we argued from the existing literature and demonstrated from our data that the incoherence of the trauma memory has little support as a component of PTSD, in that incoherence is not substantially greater in trauma memories than it is in control memories nor is it substantially greater in PTSD participants than non-PTSD participants. We argued that there is no real evidence to include this symptom, but this is a null hypothesis claim. Even if some effects can be claimed to exist, they are small and thus the incoherence symptom may be less likely to cause distress that affects important areas of functioning compared to the other symptoms of the disorder. If, as we demonstrated, the trauma memory is not especially incoherent in PTSD, then during treatment more effort could be spent on changing other aspects of the memory that may lead to greater distress and impairment.

References

American Psychiatric Association. (2013). *Diagnostic and statistical manual of mental disorders* (5th ed.). Washington, DC: American Psychiatric Association.

American Psychiatric Association. (2000). *Diagnostic and statistical manual of mental disorders* (4th ed. Text Revision). Washington, DC: American Psychiatric Association.

Arnau, R. C., Meagher, M. W., Norris, M. P., & Bramson, R. (2001). Psychometric evaluation of the Beck Depression Inventory-II with primary care medical patients. *Health Psychology, 20,* 112.

Bauer, P. J., Doydum, A. O., Thanujeni, P., Larkina, M., Güler, O. E., & Burch, M. (2012). It's all about location, location, location: Children's memory for the 'where' of personally experienced events. *Journal of Experimental Child Psychology, 113*, 510-522.

Beck, A. T., Steer, R. A., & Brown, G. K. (1996). Manual for the Beck Depression Inventory, 2nd edition. San Antonio, TX: The Psychological Corporation.

Bernstein, E. M., & Putnam, F. W. (1986). Development, reliability, and validity of a dissociation scale. *The Journal of Nervous and Mental Disease, 174*, 727-735.

Berntsen, D., & Rubin, D. C. (2006). The centrality of event scale: A measure of integrating a trauma into one's identity and its relation to post-traumatic stress disorder symptoms. *Behaviour Research and Therapy, 44*, 219–231.

Berntsen, D., & Rubin, D. C. (2007). When a trauma becomes a key to identity: Enhanced integration of trauma memories predicts posttraumatic stress disorder symptoms. *Applied Cognitive Psychology, 21,* 417–431.

Berntsen, D. & Rubin, D. C. (2014). Involuntary memories and dissociative amnesia: Assessing key assumptions in posttraumatic stress disorder research. *Clinical Psychological Science, 2*, 174–186.

Berntsen, D., Willert, M., & Rubin, D. C. (2003). Splintered memories or vivid landmarks? Qualities and organization of traumatic memories with and without PTSD. *Applied Cognitive Psychology, 17,* 675–693.

Blake, D. D., Weathers, F. W., Nagy, L. M., Kaloupek, D. G., Gusman, F. D., Charney, D. S., & Keane, T. M. (1995). The development of a clinician-administered posttraumatic stress disorder scale. *Journal of Traumatic Stress, 8,* 75−80.

Blanchard, E. B., Jones-Alexander, J., Buckley, T. C., & Forneris, C. A. (1996). Psychometric properties of the PTSD Checklist (PCL). *Behaviour Research and Therapy, 34*, 669-673.

Blanton, H., & Jaccard, J. (2006). Arbitrary metrics in psychology. *American Psychologist, 61,* 27– 41.

Brewin, C. R., Dalgleish, T., & Joseph, S. (1996) A dual representation theory of posttraumatic stress disorder. *Psychological Review, 103,* 670-686.

Brewin, C. R., & Holmes, E. A. (2003). Psychological theories of posttraumatic stress disorder. *Clinical Psychology Review, 23*, 339-376.

Brewin, C. R. (2014). Episodic memory, perceptual memory, and their interaction: Foundations for a theory of Posttraumatic Stress Disorder. *Psychological Bulletin, 140*, 69-97.

Carlson, E. B., & Putnam, F. W. (1993). An update on the Dissociative Experiences Scale. *Dissociation: Progress in the Dissociative Disorders, 6,* 16-27.

Chen, Y., McNally, H. M., Wang, Q., & Reese, E. (2012). The coherence of critical event narratives and adolescents' psychological functioning. *Memory, 20,* 667-681.

Cohen, J. (1988). *Statistical power analysis for the behavioral sciences* (2nd ed.). Hillsdale, N.J.: Lawrence Erlbaum.

Crespo, M. & Fernández-Lansac, V. (in press). Memory and narrative of traumatic events: A literature review. *Psychological Trauma: Theory, Research, Practice, and Policy*.

Dalgleish, T. (2004). Cognitive approaches to posttraumatic stress disorder: The evolution of multirepresentational theorizing. *Psychological Bulletin, 130,* 228-260.

Draijer, N., & Boon, S. (1993). The validation of the Dissociative Experiences Scale against the criterion of the SCID-D, using receiver operating characteristics (ROC) analysis. *Dissociation: Progress in the Dissociative Disorders, 1,* 28-37.

Ehlers, A., & Clark, D. M. (2000). A cognitive model of posttraumatic stress disorder. *Behaviour Research and Therapy, 38*, 319-345.

First, M. B., Spitzer, R. L., Gibbon, M., & Williams, J. B. W. (1996). *Structured Clinical Interview for DSM-IV Axis I Disorders, Clinician Version (SCID-CV*). Washington, D.C.: American Psychiatric Press, Inc.

Fivush, R., Sales, J. M., & Bohanek, J. G. (2008). Meaning making in mothers' and children's narratives of emotional events. *Memory*, *16*, 579-594.

Foa, E. B., Molnar, C., & Cashman, L. (1995). Changes in rape narratives during exposure therapy for posttraumatic stress disorder. *Journal of Traumatic Stress, 8,* 675-690.

Foa, E. B., Riggs, D. S., & Gershuny, B. S. (1995). Arousal, numbing, and intrusion: Symptom structure of PTSD following assault. *American Journal of Psychiatry*, *152,* 116–120.

Gentes, E., Dennis, P. A., Kimbrel, N. A., Kirby, A. C., Hair, L. P., Beckham, J. C., & Calhoun, P. S. (in press). DSM-5 posttraumatic stress disorder: Factor structure and rates of

diagnosis, *Journal of Psychiatric Research*, http://dx.doi.org/10.1016/j.jpsychires.2014.08.014

Goff, L. M., & Roediger, H. L. (1998). Imagination inflation for action events: Repeated imaginings lead to illusory recollections. *Memory & Cognition, 26,* 20-33.

Graesser, A. C., & McNamara, D. S. (2011). Computational analyses of multilevel discourse comprehension. *Topics in Cognitive Science, 3,* 371-398.

Graesser, A. C., McNamara, D. S., & Kulikowich, J. M. (2011). Coh-Metrix: Providing multilevel analyses of text characteristics. *Educational Researcher, 40*, 223-234.

Graesser, A. C., McNamara, D. S., Louwerse, M. M., & Cai, Z. (2004). Coh-Metrix: Analysis of text on cohesion and language. *Behavior Research Methods, Instruments, & Computers*, *36*, 193-202.

Greenberg, D. L., & Rubin, D. C. (2003). The neuropsychology of autobiographical memory. *Cortex, 39,* 687-728.

Greenhoot, A. F., Sun, S., Bunnel, S. L., & Lindboe, K. (2013). Making sense of traumatic memories: Memory qualities and psychological symptoms in emerging adults with and without abuse histories. *Memory, 21*, 125-142.

Giesbrecht, T., Lynn, S. J., Lilienfeld, S. O., Merckelbach, H. (2008). Cognitive processes in dissociation: An analysis of core theoretical assumptions. *Psychological Bulletin, 134,* 617–647.

Halligan, S. L., Micheal, T., Clark, D. M., & Ehlers, A. (2003). Posttraumatic stress disorder following assault: The role of cognitive processing, trauma memory, and appraisals. *Journal of Consulting and Clinical Psychology, 71*, 419–431.

Harvey, A. G., & Bryant, R. A. (1999). A qualitative investigation of the organization of traumatic memories. *British Journal of Clinical Psychology, 38,* 401–405.

Hollingshead A. B. (1975). Four factor index of social status. Unpublished manuscript reprinted in the *Yale Journal of Sociology*, 2011, *8*, pp. 21 -52.

Horowitz, M. J. (1976). *Stress response syndromes*. New York: Jason Aronson.

Hyman, I.E., & Pentland, J. (1996). The role of mental imagery in the creation of false childhood memories, *Journal of Memory and Language, 35,* 101-117.

Jelinek, L., Randjbar, S., Seifert, D., Kellner, M., & Moritz, S. (2009). The organization of autobiographical and nonautobiographical memory in posttraumatic stress disorder (PTSD). *Journal of Abnormal Psychology, 118,* 288–298.

Jones, C., Harvey, A. G., & Brewin, C. R. (2007). The organization and content of trauma memories in survivors of road traffic accidents. *Behaviour Research and Therapy, 45,* 151–162.

Kenardy, J., Smith, A., Spence, S. H., Lilley, P.-R., Newcombe, P., Dob, R., & Robinson, S. (2007). Dissociation in children's trauma narratives: An exploratory investigation. *Journal of Anxiety Disorders, 21,* 456-466.

Kintsch, W. (1998). *Comprehension: A Paradigm for Cognition*. Cambridge, U. K.: Cambridge University Press.

Kubany, E. S., Leisen, M. B., Kaplan, A. S., Watson, S. B., Haynes, S. N., Owens, J. A., Burns, K. (2000). Development and preliminary validation of a brief broad-spectrum measure of trauma exposure: The Traumatic Life Events Questionnaire. *Psychological Assessment, 12*, 210-224.

Larkina, M., & Bauer, P. J. (2012). 'Family stories' and their implications for preschoolers' memories of personal events. *Journal of Cognition and Development, 13,* 473-504.

McAdams, D. P. (2006). The problem of narrative coherence. *Journal of Constructivist Psychology, 19,* 109-125.

McNally, R. J. (2003a). Progress and controversy in the study of posttraumatic stress disorder. *Annual Review of Psychology, 54,* 229-252.

McNally, R. J. (2003b). *Remembering trauma*. Cambridge: Harvard University Press.

McNamara, D. S., Graesser, A. C., McCarthy, P. M., & Cai, Z. (2014). *Automated evaluation of text and discourse with Coh-Metrix*. Cambridge: Cambridge University Press.

McNamara, D. S., Louwerse, M. M., McCarthy, P. M., & Graesser, A. C. (2010). Coh-Metrix: Capturing linguistic features of cohesion. *Discourse Processes, 47,* 292–330.

Meehl, P. E. (1971). High school yearbooks: A reply to Schwarz. *Journal of Abnormal Psychology, 77,* 143-148.

Morris, G., Baker-Ward, L., & Bauer, P. J. (2010). What remains of that day: The survival of children's autobiographical memories across time. *Applied Cognitive Psychology, 24,* 527-544.

Nadon, R., Hoyt, I. P., Register, P. A., & Kihlstrom, J. F. (1991). Absorption and hypnotizability: Context effects reexamined. *Journal of Personality and Social Psychology, 60,* 144.

O'Kearney, R., & Perrott, K. (2006). Trauma narratives in posttraumatic stress disorder: A review. *Journal of Traumatic Stress, 19,* 81-93.

Pennebaker, J. W., Mehl, M. R., & Niederhoffer, K. G. (2003). Psychological aspects of natural language use: Our words, our selves. *Annual review of psychology, 54,* 547-577.

Pennebaker, J. W., Booth, R. J., & Francis, M. E. (2007). Linguistic inquiry and word count: LIWC2007—Operator's manual. Retrieved from http://www.liwc.net/

Porter, S., & Birt, A. R. (2001). Is traumatic memory special? A comparison of traumatic memory characteristics with memory for other emotional life experiences. *Applied Cognitive Psychology, 15*, 101-118.

Reese, E., Haden, C. A., Baker-Ward, L., Bauer, P., Fivush, R., & Ornstein, P. A. (2011). Coherence of personal narratives across the lifespan: A multidimensional model and coding method. *Journal of Cognition and Development, 12*, 424-462.

Römisch, S., Leban, E., Habermas, T., & Döll-Hentschker, S. (2014). Evaluation, involvement, and fragmentation in narratives of distressing, angering, and happy events by traumatized and nontraumatized women. *Psychological Trauma: Theory, Research, Practice, and Policy*, 6, 465-472.

Rubin, D. C. (2011). The coherence of memories for trauma: Evidence from posttraumatic stress disorder. *Consciousness and Cognition, 20*, 857-865.

Rubin, D. C., Berntsen, D., & Bohni, M. K. (2008). A memory-based model of posttraumatic stress disorder: Evaluating basic assumptions underlying the PTSD diagnosis. *Psychological Review*, *115*, 985-1011.

Rubin, D. C., Boals, A., & Berntsen, D. (2008). Memory in posttraumatic stress disorder: Properties of voluntary and involuntary, traumatic and non-traumatic autobiographical memories in people with and without PTSD symptoms. *Journal of Experimental Psychology: General, 137*, 591-614.

Rubin, D. C., Dennis, M. F., & Beckham, J. C. (2011). Autobiographical memory for stressful events: The role of autobiographical memory in posttraumatic stress disorder. *Consciousness & Cognition, 20,* 840-856.

Rubin, D. C., Feldman, M. E., & Beckham, J. C. (2004). Reliving, emotions, and fragmentation in the autobiographical memories of veterans diagnosed with PTSD. *Applied Cognitive Psychology, 18,* 17–35.

Rubin, D. C., Schrauf, R., & Greenberg, D. (2003). Belief and recollection of autobiographical memories. *Memory & Cognition, 31*, 887-901.

Rubin, D., Schrauf, R., & Greenberg, D. (2004). Stability in autobiographical memories. *Memory, 12*, 715-721.

Rude, S., Gortner, E. M., & Pennebaker, J. (2004). Language use of depressed and depression-vulnerable college students. *Cognition & Emotion, 18*, 1121-1133.

Salmond, C. H., Meiser-Stedman, R., Glucksman, E., Thompson, P., Dalgleish, T., & Smith, P. (2011). The nature of trauma memories in acute stress disorder in children and adolescents. *Journal of Child Psychology and Psychiatry, 52,* 560–570.

Shobe, K. K., & Kihlstrom, J. F. (1997). Is traumatic memory special? *Current Directions in Psychological Science, 3,* 70-74.

Snow, C. E. (2002). *Reading for understanding: Toward a research and development program in reading comprehension*. Santa Monica, CA: Rand Corporation.

Stewart, S. H., Conrod, P. J., Pihl, R. O., & Dongier, M. (1999). Relations between posttraumatic stress symptom dimensions and substance dependence in a community-recruited sample of substance-abusing women. *Psychology of Addictive Behaviors, 13,* 78-88.

Waters, T. E., Bohanek, J. G., Marin, K., & Fivush, R. (2013). Null's the word: A comparison of memory quality for intensely negative and positive events. *Memory, 21*, 633-645.

Weathers, F. W., Keane, T. M., & Davidson, J. R. (2001). Clinician-administered PTSD scale: A review of the first ten years of research. *Depression and Anxiety*, *13*, 132–156.

Weathers, F. W., Litz, B. T., Huska, J. A., & Keane, T. M. (1994). PCL-C for DSM-IV. Boston. *National Center for PTSD-Behavioral Sciences Division*.

Yufik, T. & Simms, L. J. (2010). A meta-analytic investigation of the structure of posttraumatic stress disorder symptoms. *Journal of Abnormal Psychology, 119,* 764–776.

Table 1.

Participant Demographics

Measures	Control (n = 30)		PTSD (n = 30)		
	M	(*SD*)	*M*	(*SD*)	*t* – tests [b]
Age	36.87	(11.87)	38.83	(8.36)	.74
Years of education	13.33	(2.16)	12.77	(2.25)	1.00
Hollingshead SES[a]	48.57	(13.64)	57.13	(11.71)	2.61*
BDI-II	10.53	(6.55)	17.83	(10.94)	3.14**
DES	10.63	(7.64)	22.58	(18.43)	3.28**
PCL	30.14	(8.33)	51.61	(15.02)	6.84****
CAPS	14.93	(10.29)	60.13	(14.45)	13.96****
	%		%		χ^2
Ethnic minority	63.33		76.67		1.27
Female gender	63.33		60.00		.79
Combat service	20.00		36.67		2.05
MDD[c]	46.67		50.00		.07
Other mental illness[c]	23.33		40.00		1.93
Past substance use	53.33		73.33		2.58
Trauma Type for Diagnosis					13.21
Combat	6.67		20.00		
Childhood physical /sex abuse	6.67		13.33		
Adult physical /sex assault	3.33		13.33		
Accident	10.00		0		
Domestic violence	16.67		6.67		
Death of a loved one	30.00		13.33		
Adult violence	10.00		20.00		
Witnessed childhood violence	6.67		10.00		
Natural disaster	3.33		0		
Other	6.67		3.33		

Note. BDI = Beck Depression Inventory. DES = Dissociative Experience Scale. PCL = PTSD Checklist symptom severity scores. CAPS = Clinical-Administered PTSD Scale. MDD = Major Depressive Disorder. [a]Smaller values on Hollingshead SES indicate higher socioeconomic status. [b]Levene's tests for Equality of Variances were also conducted. Analyses with adjustment of *df* and *t*-critical did not affect the significance of any of the comparisons. [c] Participants with current or lifetime diagnosis included.*$p < .05$, **$p < .01$, *** p<.001, **** p< .0001.

Table 2.

Means (SDs) and ANOVAs for Measures of Narrative Coherence for Trauma, Positive, and Important Memories

	Trauma Memories				Positive Memories				Important Memories				ANOVAS			
	Control		PTSD		Control		PTSD		Control		PTSD		Memory Type		Group Inter-action	
Narrative Properties	*M*	(*SD*)	*M*	(*SD*)	*M*	(*SD*)	*M*	(*SD*)	*M*	(*SD*)	*M*	(*SD*)	*F*(2, 116)	η^2_p		
Participant Ratings of the Autobiographical Memory Questionnaire																
Setting	6.18	(1.06)	6.51	(.88)	6.31	(.91)	6.49	(.83)	6.21	(.85)	6.34	(.89)	-		-	-
Story	5.21	(1.61)	5.96	(1.14)	5.70	(1.66)	5.69	(1.45)	5.38	(1.58)	5.69	(1.49)	-		-	-
Pieces	2.96	(1.57)	3.18	(1.70)	2.48	(1.53)	2.80	(1.77)	2.73	(1.64)	2.80	(1.66)	3.62*	.06	-	-
Neutral Observer Ratings of the Narrative Coherence Coding Scheme																
Context	2.31	(.36)	2.28	(.39)	2.30	(.42)	2.18	(.46)	2.14	(.47)	2.19	(.43)	-		-	-
Chronology	2.75	(.21)	2.81	(.16)	2.54	(.35)	2.57	(.27)	2.57	(.30)	2.60	(.25)	24.30****	.29	-	-
Theme	2.22	(.19)	2.16	(.16)	2.21	(.18)	2.23	(.24)	2.31	(.22)	2.30	(.23)	8.36***	.12	-	-
Neutral Observer Ratings of Global Coherence																
Narrator	4.89	(.64)	4.72	(.60)	4.54	(.59)	4.55	(.61)	4.53	(.54)	4.70	(.60)	6.08**	.09	-	-
Emotion	5.25	(.62)	5.19	(.60)	4.00	(.62)	4.00	(.54)	3.96	(.61)	4.24	(.56)	122.36****	.67	-	-
% Irrelevant	2.83	(2.97)	3.94	(5.20)	5.42	(4.58)	6.39	(7.66)	5.03	(4.29)	7.83	(6.88)	9.98****	.14	-	-
Disorganization	1.27	(.65)	1.39	(.80)	1.37	(.72)	1.62	(.97)	1.48	(.69)	1.63	(.97)	-		-	-
Computer Scored Coh-Metrix Measures																
Narrativity	2.54	(.53)	2.53	(.51)	2.46	(.57)	2.48	(.49)	2.54	(.46)	2.57	(.46)	-		-	-
Syntax ease	-1.50	(.86)	-1.03	(1.07)	-1.41	(1.14)	-1.36	(1.21)	-1.57	(.92)	-1.26	(1.03)	-		-	-
Word concreteness	-.30	(.71)	-.21	(.71)	-.39	(.61)	-.61	(.56)	-.44	(.66)	-.70	(.70)	6.38**	.10	-	-
Referential cohesion	1.78	(.91)	1.51	(.80)	1.76	(1.17)	1.73	(.87)	1.88	(.93)	1.65	(.65)	-		-	-

Deep cohesion	1.06	(1.12)	.60	(.85)	1.29	(1.09)	1.46	(1.25)	1.59	(.92)	1.28	(.94)	12.88****	.17	-	-
Content word overlap	.21	(.05)	.19	(.05)	.22	(.07)	.22	(.05)	.23	(.06)	.21	(.04)	-		-	-
Latent semantic analysis	.30	(.10)	.28	(.08)	.29	(.10)	.29	(.09)	.30	(.10)	.28	(.07)	-		-	-
Temporal connectives	18.06	(5.02)	17.41	(6.23)	17.91	(6.06)	17.65	(7.30)	17.13	(5.84)	16.69	(7.18)	-		-	-
Ext temporal connectives	15.62	(6.05)	14.02	(6.01)	18.67	(5.41)	21.98	(7.57)	18.00	(6.50)	18.59	(7.43)	14.57****	.19	-	-
Causal connectives	28.90	(9.45)	25.74	(7.63)	31.47	(9.72)	34.39	(10.19)	33.43	(7.97)	32.03	(7.86)	14.17****	.19	-	-
Adversative connectives	17.80	(5.58)	16.36	(7.26)	16.75	(7.27)	14.81	(5.88)	17.94	(6.36)	15.77	(6.27)	-		-	-
Logical connectives	46.23	(13.17)	41.23	(12.01)	49.10	(14.13)	49.86	(13.93)	49.74	(10.99)	47.11	(9.40)	7.30**	.11	-	-
Additive connectives	67.27	(14.33)	64.38	(14.25)	68.99	(15.39)	59.95	(11.45)	68.93	(14.41)	61.63	(16.38)	-		-	-
Computer Scored Linguistic Inquiry Word Counts																
Cognitive mechanisms	19.26	(2.39)	18.13	(1.74)	19.30	(2.68)	17.13	(1.93)	19.09	(2.64)	18.73	(2.26)	-		-	-
Insight	2.25	(.93)	2.16	(.68)	1.90	(.62)	2.06	(.98)	2.13	(.74)	2.34	(.98)	-		-	-
Cause	1.31	(.77)	1.15	(.51)	1.22	(.67)	1.30	(.56)	1.45	(.51)	1.64	(.68)	7.51***	.11	-	-
Nonfluencies	3.51	(1.99)	3.77	(2.46)	3.59	(2.05)	4.28	(2.26)	3.55	(2.01)	4.06	(2.50)	-		-	-
Filler	1.23	(.98)	1.58	(1.59)	1.18	(.89)	1.62	(1.47)	1.19	(.70)	1.58	(1.24)	-		-	-

Note. Ext temporal connectives = Extended temporal connectives.*$p < .05$; **$p < .01$; *** $p < .001$; **** $p < .0001$

Table 3.

Post-hoc Power and Required N for Univariate ANOVAs

	Main Effect of Memory Type		Main Effect of Group		Interaction	
Narrative Properties	Power	*N*	Power	*N*	Power	*N*
Participant Ratings of the Autobiographical Memory Questionnaire						
Setting	.10	968	.20	376	.08	>999
Story	.12	690	.16	478	.45	132
Pieces	.38	158	.08	>999	.07	>999
Neutral Observer Ratings of the Narrative Coherence Coding Scheme						
Context	.24	260	.07	>999	.13	614
Chronology	1.00	26	.12	786	.06	>999
Theme	.73	70	.08	>999	.13	616
Neutral Observer Ratings of Global Coherence						
Narrator	.58	98	.05	>999	.25	256
Emotion	1.00	8	.12	734	.10	868
% Irrelevant	.81	60	.30	228	.12	668
Disorganization	.29	212	.16	510	.07	>999
Computer Scored Coh-Metrix Measures						
Narrativity	.12	634	.05	>999	.05	>999
Syntax ease	.11	774	.24	298	.15	456
Word concreteness	.60	94	.16	482	.21	312
Referential cohesion	.11	784	.15	530	.10	862
Deep cohesion	.89	48	.15	548	.28	222
Content word overlap	.31	196	.14	600	.20	330
Latent semantic analysis	.05	>999	.13	718	.08	>999
Temporal connectives	.09	>999	.07	>999	.05	>999
Ext temporal connectives	.93	42	.10	>999	.25	248
Causal connectives	.92	44	.06	>999	.30	206
Adversative connectives	.15	498	.30	226	.06	>999
Logical connectives	.67	82	.14	594	.17	388
Additive connectives	.07	>999	.55	108	.18	372
Computer Scored Linguistic Inquiry Word Count						
Cognitive mechanisms	.22	288	.80	60	.34	176
Insight	.28	218	.09	>999	.11	732
Cause	.68	80	.06	>999	.20	334
Nonfluencies	.15	498	.15	540	.10	914
Filler	.05	>999	.28	242	.06	>999

Note. Ext temporal connectives = Extended temporal connectives. Power is post-hoc power calculated in G*Power according to Cohen (1988). Req. *N* is the number of subjects required given an *a priori* power of .8.