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The Effect of Post-Incident Discussion on Psychological Wellbeing and Memory Integrity

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BA (Psych Honours)

2015

The University of Sydney
School of Psychology
Faculty of Science

A thesis submitted in fulfillment of the requirements for the degree of
Doctor of Clinical Psychology/Master of Science
Statement of Originality

I, Leila Bukalo, declare that the work contained within this thesis entitled “The Effect of Post-Incident Discussion on Psychological Wellbeing and Memory Integrity” is, to the best of my knowledge and belief, original except as acknowledged in the text. I hereby declare that I have not submitted this material, either in full or in part, for a degree at this or any other institution.

………………………………..  …………………………………..

Leila Bukalo  Date
Acknowledgements

There are a number of people I would like to acknowledge who have helped me over the course of this degree. Firstly, I would like to thank my primary and associate supervisors, Dr Helen Paterson and Prof. Louise Sharpe, for their knowledge, guidance and expertise over the course of this project. Thank you for all the time, effort and support you both have provided me with - I could not have done all of this without your help. Also, thank you for putting up with me along the way.

To my family: Mum, thank you for your unconditional love and support over the last few years and the course of my life more generally. Thank you for always believing in me and helping me believe in myself. Without you I would not have been able to come as far as I have. Dad, thank you for always encouraging and believing in me – I know you would be very proud if you were here today and I dedicate this work to you. Adem, thank you for being a great brother and friend.

To my ever-supportive friends, there are too many of you to name, but you know who you are. Thanks for all the laughs and for helping me stay sane over the past few years! You all bring so much joy to my life for which I am endlessly grateful. I have to give a special shout-out though to the Sugarbabes - Lily and Pav, you guys are amazing in every way.

Finally, a special thank you to Mark. I can’t thank you enough for all your love, patience, support, humor and friendship over the past few years. You are the best friend and partner in life that I could ever hope for.
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Abstract

As a consequence of their work, emergency service personnel, such as fire-fighters, police officers and paramedics are frequently exposed to traumatic events. This can result in an elevated risk of developing Post Traumatic Stress Disorder (PTSD). To date, studies have indicated that post-trauma early interventions, such as Critical Incident Stress Debriefing, offer no benefit to individuals in terms of recovery from trauma-type symptomatology and may even be harmful. Furthermore, recent studies indicate that these procedures may also have negative effects on individuals’ memory for the event. While research to date has largely examined the efficacy of debriefing procedures as a whole, the present analogue study aimed to investigate what aspects of currently used debriefing procedures may contribute to the negative effects on psychological adjustment and memory integrity observed in the literature. Specifically, the current study examined the effects of differently-focused discussions (emotion, fact, and non-event related) on psychological adjustment and memory integrity, and investigated whether the outcome of these discussions was influenced by the mode of administration (individual, group discussion format). Participants were 177 undergraduate students who viewed a video of a forensic autopsy. Psychological reactions were tested at baseline, immediately after discussions and one week later. Memory for the video was assessed immediately following discussions and one week later. Psychological distress was found to decrease for all participants over time, with no significant differences in symptom recovery between conditions. Although those assigned to the group condition reported fewer symptoms of anxiety, depression and stress across time periods than the individual group, there was no indication that reduced symptomatology was an effect of the intervention, per se. Those who discussed factual information in a group setting incorporated more misinformation into memory; however, these participants reported the most confidence
in the accuracy of their memories. Overall, these results suggest that, irrespective of the model of administration, post-trauma discussions in which emotional reactions or factual information are reviewed do not facilitate psychological adjustment compared to non-event related discussions. Furthermore, this study provides a greater understanding of the specific aspects of debriefing that may account for misinformation effects. These findings can be used to inform the development of more effective post-trauma interventions in future.
Chapter 1

LITERATURE REVIEW

1.1 Overview of Thesis

Many employees, such as emergency service personnel, are exposed to highly distressing situations by virtue of their occupations. Nevertheless, these individuals are not immune to the psychological effects of these events (Regehr & Hill, 2001). Indeed, high levels of distress and posttraumatic symptomatology have been identified in individuals employed in invaluable help-providing roles after traumatic events, including nurses (e.g., Matthews, 1998), paramedics (e.g., Thompson, 1993), fire-fighters (e.g., Harris, Bololu & Stacks, 2002), police officers (e.g., Leonard & Alison, 1999), and military personnel (e.g., Wu et al., 2011). This is particularly concerning when considering the negative effects associated with posttraumatic stress disorder (PTSD) including elevated rates of suicide, substance abuse, comorbid mental health problems, physical health problems, and social and occupational impairment (see Kessler, 2000; Tanielian & Jaycox, 2008, for reviews). Indeed, when applied to an emergency services context, these impairments can impact on the provision of high-quality services to the public and potentially place employees at risk of additional work-related trauma.

In response to these issues, a number of brief crisis interventions, or psychological debriefing (PD) models, have been developed and used with trauma-exposed individuals to mitigate the negative effects of traumatic events. Of these, Critical Incident Stress Debriefing
(CISD) has been the most widely used and researched protocol. This procedure was specifically developed by Mitchell (1983) for emergency service employees following exposure to traumatic events and has been incorporated into a number of alternative PD models (e.g., Adler et al., 2009, Armstrong, O’Callahan & Marmar, 1991; Dyregrov, 1989; Everly, Flannery & Mitchell, 2000; Wu et al., 2011). Generally, these interventions involve single-session, semi-structured discussions which give participants the opportunity to discuss the nature of the event as well as their associated emotional and behavioural reactions (Devilly, Gist, & Cotton, 2006).

Despite the widespread use of PD protocols, research has accumulated indicating that these procedures may have little or no benefit for psychological wellbeing (see Rose, Bisson, Churchill & Wessely, 2002), and may be iatrogenic when administered individually (e.g., Bisson, Jenkins, Alexander & Bannister, 1997). However, the impact of group debriefing procedures is largely unknown and has only recently attracted research attention (see Devilly & Annab, 2008; Tuckey & Scott, 2014). Nevertheless, the limited studies of group debriefing that have been conducted have identified memory contamination effects associated with this mode of debriefing (e.g., Devilly, Varker, Hansen & Gist, 2007). Thus, in addition to potentially negative effects on psychological health, it appears that debriefing may also have negative consequences for eyewitness memory. Furthermore, it seems that each of these outcomes may be associated with different modes of administering PD (i.e., individual debriefing impeding psychological adjustment and group debriefing increasing risk for memory contamination). This raises questions about how post-trauma interventions should be administered, if at all.

Indeed, a number of reviews have recommended that psychological debriefing should no longer be used with victims of trauma due to the potentially detrimental effects of this
intervention (see Rose et al., 2002). However, a key question arises about what should be provided in place. The development of effective post-trauma interventions is both valuable and necessary for a number of reasons. Firstly, given the personal and professional impairment associated with PTSD, it is important to attend to the needs of individuals exposed to traumatic events to facilitate their return to pre-trauma levels of functioning. Furthermore, the development of effective interventions is necessary to help organisations meet their duty of care requirements under workplace health and safety laws. In fact, due to research findings supporting both arguments, organisations are currently placed in a precarious position whereby both providing and not providing PD may lead to litigation.

Interestingly, while whole models have been examined, the individual components of PD protocols have not attracted much research attention. Thus, it has not been established whether specific debriefing procedures may be more responsible for the negative outcomes that have been identified in the literature. If certain elements can be identified as problematic, this could inform the development of new interventions in future which mitigate the psychological effects of trauma more effectively and minimise the risk for memory contamination. In addition, if different modes of administering these procedures (i.e., in an individual versus group-based format) can be identified as potentially compounding these negative effects, this will further aid the development of more informed models in future. Thus, the present thesis will examine whether specific PD procedures and methods of administration may contribute to the negative outcomes identified in the literature to date.

This chapter will begin by discussing the prevalence of trauma exposure, followed by an examination of posttraumatic stress disorder (including clinical definition, prevalence, course, associated impairment and risk factors). Subsequently, theoretical models of PTSD will be outlined to highlight the mechanisms underlying this disorder. A review of evidence-
based treatment and commonly used preventive interventions will be presented, followed by an examination of the empirical evidence for psychological debriefing. The results of a systematic review investigating the effects of group debriefing on psychological wellbeing will then be discussed. Finally, the justification and hypotheses for the current empirical study will be presented.

1.2 Trauma Exposure

Research has shown that exposure to traumatic events is not a rare occurrence. Indeed, a recent survey of 2,953 U.S. adults found that up to 89.7% of participants had been exposed to at least one traumatic event in their lifetime (Kilpatrick et al., 2013). This indicates that trauma exposure is, in fact, highly prevalent in the general population – a finding which has been corroborated by a number of large epidemiological surveys (for reviews, see Kessler, 2000; Mills et al., 2011; Ozer, Best, Lipsey & Weiss, 2008).

Among emergency service personnel (e.g., paramedics, firefighters and police officers), the rate of trauma exposure is much higher than the general population (Kessler et al., 1995; Langan-Fox & Cooper, 2011). Indeed, due to the nature of their work, emergency personnel may be directly exposed to several potentially traumatic events per day. The most frequently experienced include death (i.e., of children, patients, or colleagues), violence (i.e., directed at self or others), and situations in which the worker’s safety is at risk (e.g., receiving threats; Langan-Fox & Cooper, 2011). For example, Regehr, Goldberg and Hughes (2002) found that 85% of paramedics had been exposed to the death of a child, 85% had had a patient die in their care, 70% had experienced violence committed against them, and 28% had lost a colleague in the line of duty. Similarly, Regehr, Hill and Glancy (2000) found that 59% of firefighters had been exposed to the death of a child, 32% had had a patient die in their care, 36% had lost a colleague in the line of duty, and 17% had had violence committed
against them by a member of the public. Finally, among police recruits in the early stages of their career, Regehr et al. (2007) found that 80% had been exposed to at least one potentially traumatic event (e.g., being seriously injured, witnessing a colleague being seriously injured, witnessing death, being threatened with a weapon, or receiving death threats against their family members).

While most individuals report psychological distress following exposure to traumatic events (for reviews, see Bryant, 2003; Litz, Grey, Bryant & Adler, 2002), the majority are able to successfully return to their pre-trauma level of functioning within one year (Breslau, 2009; Chapman et al., 2012; Kessler, Somnnega, Bromet, & Nelson, 1995). Nevertheless, a minority of trauma survivors fail to recover and may go on to develop diagnosable psychiatric disorders. In particular, posttraumatic stress disorder has been well established as a predominant disorder following exposure to traumatic events among the general population and the emergency services.

1.3 Posttraumatic Stress Disorder (PTSD)

1.3.1 Clinical Definition

The fifth edition of the Diagnostic and Statistical Manual of Mental Disorders (DSM-5; APA, 2013) defines PTSD as a disorder which is characterised by a cluster of symptoms that develop following exposure to a traumatic stressor. The diagnostic criteria describes a traumatic event as one that must involve actual or threatened death, serious injury or sexual violence which an individual has directly experienced, witnessed or learned of (Criterion A). Psychological reactions to these events are grouped into four clusters: intrusion symptoms, characterised by persistent, vivid and involuntary re-experiencing of the trauma (e.g., via images, thoughts, dreams, flashbacks; Criterion B); avoidance of internal or external
reminders of the traumatic event (e.g., memories, people; Criterion C); negative mood and cognitions (e.g., amnesia for parts of the event; Criterion D); and hyperarousal symptoms (e.g., irritability, sleep disturbance; Criterion E). To meet diagnostic criteria, symptoms must persist for more than one month following the traumatic event (Criterion F) and cause significant impairment in some domain of daily functioning (Criterion G). Finally, symptoms must not be attributable to substance use or a medical condition (Criterion H).

### 1.3.2 DSM-5 versus DSM-IV Diagnostic Criteria

The DSM-5 conceptualisation of PTSD differs in a number of ways from the DSM-IV (APA, 2000) nomenclature. Most noteworthy is the new category within which PTSD is categorised. Specifically, while classed as an anxiety disorder in DSM-IV, PTSD is included in a new ‘Trauma- and Stressor-Related Disorders’ chapter in DSM-5. The rationale for this change is based on the long-standing acknowledgement that PTSD is a disorder which is precipitated by exposure to a traumatic event (Friedman et al., 2011b). Indeed, while PTSD shares symptoms with other anxiety disorders (e.g., hypervigilance); it is distinct in that trauma exposure is required to be a precipitating factor in the diagnostic criteria.

A number of significant changes to the DSM-IV diagnostic criteria for PTSD were introduced in DSM-5. With respect to the stressor criterion (Criterion A), sexual assault has been specifically included as a type of traumatic event that may precipitate PTSD. Further, trauma exposure is more clearly defined in DSM-5 than it was previously. That is, while trauma exposure was described as experiencing, witnessing or being confronted by a traumatic event in DSM-IV (APA, 2000), DSM-5 provides a more detailed definition whereby exposure may include directly experiencing, witnessing, learning of trauma experienced by a significant other (e.g., friend, family member), or repeated exposure to
aversive details related to a traumatic event. Finally, the DSM-IV criterion requiring individual responses characterised by “fear, helplessness and horror” (Criterion A2; APA, 2000) has been eliminated in DSM-5 due to its questionable diagnostic utility (for a review, see Friedman, Resick, Bryant & Brewin, 2011).

Despite these differences, all 17 symptoms listed in the DSM-IV diagnostic criteria for PTSD were retained in DSM-5, albeit with minor modifications to some for the purpose of clarification. In addition, a further three symptoms were introduced (i.e., persistent, distorted cognitions related to the traumatic event which leads the individual to blame themselves or others (Criterions D3), persistent negative emotional state involving fear, horror, anger, guilt or shame (Criterion D4) and, reckless or destructive behaviour following the event (Criterion E2). While PTSD symptoms were grouped into three clusters in DSM-IV (i.e., Intrusion, Avoidance/Numbing, and Arousal), four symptom clusters have been included in the DSM-5 diagnostic criteria (i.e., Intrusion, Avoidance, Negative Alterations in Cognition and Mood, and Hyperarousal and Reactivity). This decision came as the result of confirmatory factor analyses which supported the splitting of the DSM-IV Avoidance/Numbing cluster into two separate symptom groups; ‘Avoidance Behaviour’ and ‘Negative Alterations in Cognitions and Mood’ (for a review, see Friedman et al., 2011a).

While there have been a number of changes to the diagnostic criteria from DSM-IV to DSM-5, no changes were made to the required duration of disturbance (Criterion F). Thus, symptoms must persist for a minimum period of one month in order to meet criteria. Further in line with DSM-IV, these symptoms must cause significant distress or functional impairment (Criterion G), and must not be attributable to other factors (e.g., substance use or medical condition; Criterion H).
1.3.3 Prevalence and Course of PTSD

Lifetime PTSD prevalence estimates vary across epidemiological studies. Nevertheless, extant research indicates that PTSD is not a rare disorder in the general population. For example, the 2007 National Survey of Mental Health and Wellbeing estimated a lifetime prevalence rate of 12.2%, representing approximately two million Australians (Australian Bureau of Statistics, 2007). This rate was second only to harmful use of alcohol, reinforcing the widespread nature of this disorder among the general population.

Such high rates are concerning, particularly when the chronic nature of PTSD is taken into consideration. Indeed, a study based on data from the 2007 National Survey of Mental Health and Wellbeing found that while most Australians diagnosed with PTSD will remit, more than one third continue to experience symptoms decades after the traumatic event (Chapman et al., 2012). Similarly, Kessler et al. (1995) found that over one third of U.S. adults diagnosed with PTSD reported experiencing symptoms six years after exposure, irrespective of whether they received treatment. Thus, not only is PTSD a relatively prevalent disorder among the general population, but for a significant proportion of those affected, the disorder will follow a chronic course.

High prevalence rates of PTSD have also been confirmed in research with emergency service workers. Indeed, a recent meta-analysis of epidemiological studies investigating the prevalence of PTSD in rescue workers (including paramedics, police officers and firefighters) revealed a pooled worldwide prevalence rate of 10% for this population (Berger et al., 2012). In line with findings from the general population, persistent symptoms have also been reported by emergency service personnel. For example, a study conducted by Marmar et al. (1999) found that some workers were still experiencing PTSD symptoms five years post-
trauma. Thus, these research findings indicate that emergency service personnel, who are often exposed to traumatic events by virtue of their work (Donnelly & Siebert, 2009), are not immune to the negative effects of these events, and in fact, are at a high risk for developing PTSD.

In addition to being a highly prevalent and persistent disorder, PTSD has also been found to commonly co-occur with other psychiatric disorders (Kessler, 2000). Specifically, high rates of mood and anxiety disorders, substance use disorders, suicidal ideation and suicide attempts have been identified among individuals diagnosed with PTSD compared to those without PTSD (see Kessler, 2000, for a review). High comorbidity rates have also been identified among emergency service personnel, For example, McFarlane and Papay (1992) found that up to 77% of Australian firefighters diagnosed with PTSD also had a comorbid diagnosis of depression, panic disorder or phobic disorder. Such high rates are particularly problematic given that the functional impairment associated with PTSD has been found to increase significantly with psychiatric comorbidity (North et al. 1999).

1.3.4 Impairment Associated with PTSD

Indeed, PTSD has been associated with significant impairment; particularly in the domains of social and occupational functioning. For example, following the 1995 Oklahoma City bombing, North et al. (1999) found that 44.4% of individuals diagnosed with PTSD had experienced negative changes to their relationships with relatives or friends, and 9.1% reported worsened relationships with their spouse, significant other or other household member. Elevated rates of marital instability have also been reported in studies of individuals diagnosed with PTSD (for a review, see Kessler, 2000), reinforcing the significant negative impact of this disorder on individuals’ interpersonal relationships.
High rates of work impairment and unemployment have also been identified in the PTSD literature (see Kessler, 2000, for a review). Specifically, research suggests that this disorder is associated with disruption to job performance, elevated rates of burnout, absenteeism, sick leave, work cutback days, resignation rates, and early retirement (Baunach et al., 2002; Fisher & Etches, 2003; Kessler & Frank, 1997; Robinson & Mitchell, 1994). Furthermore, North et al. (1999) found that 52.6% of participants reported being dissatisfied with their work performance following trauma exposure. Thus, PTSD appears to be associated with significant interpersonal, occupational and financial costs to individuals, which in turn compounds their difficulties.

1.3.5 Risk Factors for PTSD

A number of variables have been associated with increased risk for PTSD. These relate to factors operating prior to, during, and following the trauma. Indeed, Ozer et al. (2008) conducted a meta-analysis of 68 studies, identifying a number of factors that were associated with the subsequent development of PTSD following trauma exposure. With respect to pre-trauma variables, history of prior trauma was found to be associated with an increased risk for PTSD. In fact, the cumulative effect of trauma exposure on PTSD caseness and severity has been well established in the literature (see Brewin, Andrews & Valentine, 2000). Further, prior history of psychiatric disorders was also found to be predictive of PTSD (Ozer et al., 2008). Indeed, Breslau, Davis, Andreski and Peterson (1991) found that pre-trauma anxiety and mood disorders, as well as a family history of these disorders, was associated with an elevated risk for PTSD. Interestingly, Ozer et al. (2008) proposed that demographic factors including gender, education and ethnicity are not plausibly implicated in the development of PTSD. This conclusion was based on the findings of a meta-analysis conducted by Brewin et al. (2000) which found that the effects of these factors were small
and did not lead to a greater understanding of the development of PTSD compared to other factors.

During or immediately following traumatic events, the intensity of negative emotional responses has been associated with subsequent symptom severity (McNally, Bryant & Ehlers, 2003; Ozer et al., 2008). These reactions include fear, helplessness, horror, guilt and shame. Furthermore, individuals who perceive their life as being in danger during the traumatic event are also at a higher risk for developing PTSD (Ozer et al., 2008). Moreover, dissociative experiences during the traumatic event have also been linked to an increased risk for PTSD (Ozer et al., 2008).

With respect to post-trauma variables, lack of social support has been consistently identified as a significant predictor of PTSD (Brewin et al., 2000; Hyman, 2004; Ozer et al., 2008). Similarly, negative social support (e.g., minimising problems or needs, invalidating messages) has also been linked to long-term post-trauma distress (see Hobfoll et al., 2007). However, when social support is perceived to be helpful and available, this serves as an important protective factor against PTSD (e.g., Schumm, Briggs & Hobfoll, 2006). Indeed, Arnberg, Hultman, Michel and Lundin (2012) investigated the effect of perceived social support from friends and relatives on posttraumatic stress in a sample of 4,600 survivors of the 2004 Indian Ocean tsunami. Their results revealed a significant buffering effect, which ranged from small for those with low levels of exposure to moderate for those with high levels of exposure. Thus, social support appears to be a particularly influential factor in moderating post-trauma psychological adjustment.

Based on the research described above, it appears that a significant proportion of those diagnosed with PTSD experience distress several years after the traumatic event. This leads to considerable impairment, which is further compounded by high psychiatric
comorbidity. As a result, a number of studies have attempted to identify risk factors for the development of PTSD, with several variables being highlighted in the literature to date. This information is important in facilitating the development of sound theoretical models and effective interventions for the treatment and prevention of PTSD. In order to better understand the psychological mechanisms that drive trauma-type symptomatology, a review of current theoretical models of PTSD will be presented in the next section.

1.4 Theoretical Models of PTSD

A number of theoretical models have been developed to highlight the mechanisms underpinning the development of PTSD. These have been used to inform treatment and improve the effectiveness of PTSD interventions. Two theories will be examined in detail in this review; Foa and Kozak’s (1986) Emotional Processing Theory and the Cognitive Model of PTSD developed by Ehlers and Clark (2000).

1.4.1 Emotional Processing Theory

Emotional processing theory was originally advanced as an explanation for the efficacy of exposure techniques used in behavior therapy (Foa & Kozak, 1986). The theory was based on Lang’s (1977) *bioinformational* conceptualization of fear which proposes that a ‘fear network’ is represented in memory and used as a program to avoid or escape from danger. The network includes three kinds of information: (a) information about the feared situation; (b) information about cognitive, behavioural and physiological responses to the situation; and (c) meanings and interpretations about the stimulus situation and its associated responses (e.g., ‘this situation is dangerous’). While fear networks can be valuable in protecting an individual from harm, there are occasions when they may become pathological and unhelpful.
According to Foa and Kozak (1986), when inaccurate information is incorporated into the fear network, this can lead to similar yet non-harmful stimuli being erroneously interpreted as dangerous. For example, if an individual is confronted with a threatening situation, such as witnessing gunfire, the fear network would support adaptive escape behavior (e.g., running away to safety). However, if the meaning attributed to a similar yet non-harmful stimulus is inaccurate, the sound of fireworks, for example, may be associated with extreme danger, resulting in pathological responses.

Foa and Kozak (1986) suggest that two conditions must be met by any intervention in order to successfully modify such pathological fear networks. First, the fear memory must be activated, and secondly, information that is incompatible with the fear network must be available so that it may be integrated into the fear structure. The activation of the fear structure is seen as essential in that it allows for the reorganization of affective responses (Lang, 1977). In addition, the availability of information that is inconsistent with the fear structure is necessary since exposure to fear-confirming information would only serve to strengthen a fear response.

According to Foa and Kozak (1986), emotional processing (i.e., the incorporation of new, corrective information into an existing fear structure to allow for a reduction in emotional responding) can be achieved through habituation. That is, through prolonged and repeated exposure to a feared stimulus in a non-threatening environment, new representations may be formed in the fear structure, leading to a gradual decrease in fear. The authors propose that two types of habituation facilitate emotional processing. First, short-term (within session) habituation challenges the concept that anxiety will persist until escape is realised. Next, long-term (across session) habituation allows for corrective information to replace the
exaggerated probability for harm that is associated with the feared stimulus, causing initial reactions to the feared stimulus to decrease across exposure sessions.

With respect to PTSD, exposure to the trauma memory generates new information whereby the memory is experienced without the physiological arousal that accompanied the traumatic event. Over time, the individual habituates to the previously anxiety-provoking memories as new representations are stored in memory. According to Foa and Cahill (2001), emotional engagement with the trauma memory is an important mechanism underlying the reduction in PTSD severity in treatment. That is, the absence of emotional engagement with the trauma memory is suggested to hinder emotional processing and subsequently recovery.

1.4.2 Cognitive Model of PTSD

The cognitive model that was developed by Ehlers and Clark (2000) draws heavily on emotional processing theory (Foa & Kozak, 1986) and the writings of other theorists (e.g., Brewin, Dalgleish & Joseph, 1996; Horowitz, 1997) to explain the persistent nature of PTSD and provide a cognitive-behavioural framework for treatment. The authors propose that persistent PTSD will only occur when an individual develops a sense of current threat upon processing the traumatic event. Specifically, the model suggests that when a current sense of threat is triggered, this leads to a stereotypical pattern of emotional, cognitive and behavioural responses which, despite being intended to alleviate distress, maintain the disorder over time (Ehlers & Clark, 2000). Two mechanisms are identified in the model as contributing to this sense of threat associated with chronic PTSD: 1) negative appraisals of the trauma and/or its sequelae, and; 2) the nature of the trauma memory. A schematic representation of Ehlers & Clark’s (2000) models is shown below in Figure 1.1.
Ehlers and Clark (2000) identify a number of different types of negative appraisals that influence the processing of trauma information, producing a sense of current threat. These relate to an inability to view the traumatic event as time-limited. Firstly, the authors identify a tendency for individuals with persistent PTSD to overgeneralise the fear that is associated with the traumatic event. That is, the likelihood of danger associated with non-harmful situations becomes exaggerated, leading to fear and avoidance behaviours. The second type of negative appraisal raised by the authors relates to the individual’s feelings or behaviour during the traumatic event. For example, experiencing arousal during a sexual assault may be interpreted by an individual as a sign that they harbour desires which do not align with their values. The third type of negative appraisal pertains to trauma sequelae. That is, negative interpretations associated with one’s own psychological response to the trauma, of others’ reactions following the event, and of the functional impairment caused by the
The University of Sydney

trauma. These different types of negative appraisals are proposed to lead to negative emotions, such as fear, shame, guilt, sadness and anger, and encourage the use of unhelpful coping strategies which paradoxically maintain PTSD symptomatology (Ehlers & Clark, 2000).

The discrepant nature of the trauma memory is the second important feature of Ehlers and Clark’s (2000) cognitive model of persistent PTSD. While involuntary intrusive memories are frequently experienced, individuals with PTSD often experience difficulty in intentionally retrieving a complete and organised account of the traumatic event. The authors suggest that this discrepancy can be attributed to the way the trauma is encoded and laid down in memory. Specifically, they propose that the trauma memory is poorly elaborated, not considered within its context, and is inadequately integrated into other autobiographical memories. This causes problems in perceptual priming, whereby strong associations between the trauma material and other temporally associated stimuli are developed, leading to poor stimulus discrimination. Consistent with conditioning theory, these strong associations result in a reduced perceptual threshold for temporally associated stimuli, thereby increasing the likelihood of intrusive memories being triggered.

According to Ehlers and Clark (2000), negative appraisals of the trauma and the nature of the trauma memory are themselves influenced by a third variable: cognitive processing during trauma. Various types of cognitive processing are identified as correlates of chronic PTSD, including mental defeat and conceptual versus data-driven processing. Mental defeat is defined as the perceived loss of all psychological autonomy, as well as the sense of no longer being human (Dunmore, Clark, & Ehlers, 2001). Conceptual and data-driven processing is considered an important dimension of encoding information into memory (Roedigger III, 1988). Specifically, conceptual processing involves processing the
meaning of the traumatic situation in an organized way and placing it into context. This form of cognitive processing enables intentional recall of the trauma memory. In contrast, data-driven processing, or sensory-driven processing, is proposed to interfere with the ability to intentionally retrieve a complete an organized trauma memory due to strong perceptual priming for associated stimuli. This leads to poor discrimination between stimuli present during the trauma and harmless stimuli which may bear some similarity (leading to a sense of perceived threat).

The cognitive model of persistent PTSD proposed by Ehlers and Clark (2000) highlights the importance of an individual’s cognitive appraisal of the trauma and its sequelae, and the way the trauma is laid down in memory. As will be discussed, aspects of both emotional processing theory and the cognitive model of persistent PTSD are helpful in proposing potential intervention strategies for those exposed to traumatic events.

1.5 Treatment and Prevention of Posttraumatic Stress Disorder

1.5.1 Treatment of PTSD

The Australian Guidelines for the Treatment of Acute Stress Disorder (ASD) and PTSD (Australian Centre for Posttraumatic Mental Health, 2013) provides a framework of best practice around which PTSD treatment should be structured. This is based on the findings of a systematic review of the available evidence for a range of current interventions used for people with PTSD. The guidelines recommend trauma-focused interventions, such as Cognitive Behavioural Therapy (CBT) or eye movement desensitisation and reprocessing (EMDR), as the ‘first line’ treatment for PTSD. In addition, in-vivo exposure (i.e., graded exposure to the feared stimulus) is recommended as an adjunct to these interventions.
While EMDR is recommended as a first line treatment, this approach incorporates many key elements of trauma-focused CBT such as engagement with the trauma memory, cognitive processing and rehearsal of coping and mastery responses (Australian Centre for Posttraumatic Mental Health, 2013). Indeed, the unique feature of EMDR is the use of eye movements as a fundamental component of treatment. However, the absence of evidence pointing to the utility of eye movements in treatment success suggests that treatment gains are most likely due to those components which are shared with standard trauma-focused CBT. This would indicate that trauma-focused CBT, in addition to in-vivo exposure, should be considered first line treatment.

Trauma-Focused CBT (TF-CBT) incorporates both exposure and cognitive restructuring in addition to psychoeducation and symptom management strategies. This is in line with Foa and Kozak’s (1986) emotional processing theory, which supports the use of gradual exposure to the trauma memory in order to facilitate habituation. Further, Ehlers and Clark’s (2000) cognitive model of PTSD highlights the importance of cognitive restructuring in the modification of erroneous or unhelpful thoughts associated with the traumatic event or its sequelae. Indeed, Ehlers, Clark, Hackmann, McManus and Fennell (2005) developed a variant of TF-CBT which was evaluated in a randomised controlled trial against a waitlist condition. The treatment consisted of three core components: 1) the modification of negative appraisals of the trauma and its sequelae; 2) elimination of maladaptive behaviour and cognitive strategies (e.g., avoidance), and; 3) imaginal and in-vivo exposure. Their results indicated that TF-CBT was effective in reducing posttraumatic symptomatology, disability, depression and anxiety when compared to waitlist which showed no significant improvement. Further, these gains were maintained after a six month follow-up (Ehlers et al., 2005).
findings have also been obtained by other randomised controlled trials comparing TF-CBT to waitlist (for a review, see Bisson et al., 2007).

While empirically-supported treatment interventions have been identified in the literature for those who meet criteria for PTSD, the recommended *early* interventions are not as clear for adults who have been exposed to trauma but do not yet meet criteria for PTSD. Rather than treating the symptoms of an already developed PTSD, these interventions are intended to prevent the onset of this disorder by fostering adaptive functioning and coping. Given that emergency service personnel are frequently exposed to potentially traumatic events by virtue of their occupations, this places them at high risk for the development of PTSD. Thus, the availability of empirically-supported early interventions is invaluable for this population and will be the focus of this thesis.

1.5.2 Psychological Debriefing

The effect of traumatic events on emergency service workers began to attract research attention in the 1980s. This resulted in a number of early intervention models being developed which aimed to mitigate the adverse psychological effects that were associated with these events (Rose, Bisson & Wessely, 2003). For many decades, psychological debriefing (PD) was the most prominent early intervention method used with emergency service personnel (e.g., firefighters, police officers, paramedics, military personnel). While many variants have been developed, most PD models consist of single-session, semi-structured discussions, which are held shortly after exposure to a traumatic event (i.e., usually between 24 and 72 hours; Devilly et al., 2006). These discussions are intended to provide victims of trauma the opportunity to review the event and its sequelae in a supportive environment (Bisson, McFarlane & Rose, 2000), which is considered to be essential in order to achieve emotional catharsis.
Of the many PD models available, Mitchell’s (1983) Critical Incident Stress Debriefing (CISD) model has been the most widely used and researched. Indeed, for many years, CISD was considered to be the optimal post-trauma intervention for emergency service personnel. As a result, many alternative PD models have incorporated elements of the CISD procedure within their protocols (e.g., Adler, Bliese, McGurk, Hoge & Castro, 2009; Armstrong et al., 1991; Dyregrov, 1989; Raphael, 1986; Wu et al., 2011). In addition, the CISD procedure has also been included as a whole in a commonly used alternative multicomponent crisis intervention called Critical Incident Stress Management (CISM; see Everly, Flannery & Mitchell, 2000). Thus, given that many post-trauma early interventions share features with Mitchell’s (1983) CISD model, particular emphasis will be placed on this protocol when evaluating the efficacy of PD.

1.6 Critical Incident Stress Debriefing (CISD)

1.6.1 Theoretical Background

Mitchell’s (1983) CISD model was developed based on the principles of crisis theory. This theory rests on a number of primary assumptions that have been outlined by Roberts (2005). That is, according to crisis theory: a) stressful life events commonly occur over the course of a normal life; b) these events disturb stability in psychological functioning, placing individuals in a psychologically vulnerable state; c) following these events, individuals normally strive to regain equilibrium by mobilizing internal or external resources and supports; d) typically, equilibrium will be restored in four to six weeks following the stressful event (Caplan, 1964), however, if attempts to achieve equilibrium are unsuccessful, this may lead to significant long-term distress and functional impairment, and; e) in order to prevent
such maladaptive outcomes, individuals may benefit from crisis intervention during the first four to six weeks post-event (Roberts, 2005).

Indeed, Caplan (1964), who provided much of the foundation for crisis theory, suggested that individuals desire help from others and are more amenable to intervention during crises than at more stable times. Accordingly, the primary objective of crisis intervention is to intervene shortly after trauma exposure in a time-limited and action-focused manner (Aguilera, 1998; Pollin, 1995). To facilitate the development of appropriate crisis interventions, Aguilera, Messick and Farrell (1970) outlined a number of strategies that should be included in these models. Specifically, crisis interventions should: assist individuals in developing a cognitive understanding of the event; promote emotional ventilation; include a review of current and alternative methods of coping, and; involve the development of a plan of action to help individuals prepare for any future crises (Aguilera et al., 1970).

1.6.2 The CISD Procedure

In accordance with the primary objectives of crisis intervention, Mitchell’s (1983) CISD model is a time-limited procedure (requiring approximately 2-3 hours to complete) which is recommended to be administered to individuals soon after trauma exposure. The complete procedure (presented in Table 1.1) consists of seven phases that are proposed to equally contribute to the cathartic process (Everly & Mitchell, 1995). According to Mitchell and Bray (1990), there are two main goals of this intervention: 1) to mitigate the impact of traumatic events, and; 2) to accelerate psychological recovery. In line with Aguilera et al.’s (1970) crisis intervention recommendations, the procedure begins with a cognitive examination of the event (i.e., fact and thought phases) before reviewing emotional responses
(i.e., reaction phase) and symptoms of stress (i.e., symptom phase). Subsequently, current and alternative coping strategies are examined (i.e., teaching phase) before a final review and conclusion of the intervention.

Table 1.1 *CISD stages as outlined by Everly & Mitchell (1995)*

<table>
<thead>
<tr>
<th>Phase</th>
<th>Objective</th>
</tr>
</thead>
<tbody>
<tr>
<td>Introduction</td>
<td>To explain the debriefing process to participants, establish ground rules for the debriefing session, and address confidentiality</td>
</tr>
<tr>
<td>Fact</td>
<td>Participants recount the factual details of the traumatic event</td>
</tr>
<tr>
<td>Thought</td>
<td>To discuss participants’ thoughts and attributions related to the event</td>
</tr>
<tr>
<td>Reaction</td>
<td>To discuss the emotional reactions participants experienced during the height of the event</td>
</tr>
<tr>
<td>Symptom</td>
<td>To identify and discuss the symptoms of stress that participants are currently experiencing</td>
</tr>
<tr>
<td>Teaching</td>
<td>Facilitators provide participants with information about normal stress reactions and strategies to effectively manage stress</td>
</tr>
<tr>
<td>Re-entry</td>
<td>Any remaining issues or questions are addressed with participants</td>
</tr>
</tbody>
</table>

While CISD was originally developed for use with emergency service personnel, the procedure has also been adapted for use with primary victims (i.e., not emergency personnel). Indeed, the debriefing procedure has been provided to victims of motor vehicle accidents (e.g., Mayou et al., 2000), violent crime (e.g., Rose et al., 1999) and physical injury (e.g., Bisson et al., 1997), among others. Furthermore, although traditionally recommended to be delivered to groups of individuals to provide the opportunity for experiences to be normalised
in a supportive environment (Everly & Mitchell, 1995), CISD has also been administered individually to victims of trauma (e.g., Lee, Slade & Lygo, 1996; Marchand et al., 2006).

1.6.3 The Efficacy of CISD

Due to its widespread application, CISD has been subject to many empirical evaluations of its efficacy in preventing and ameliorating posttraumatic sequelae. This has resulted in an abundance of research indicating that while the majority of recipients describe the intervention as helpful (Devilly et al., 2006), CISD provides no real benefit for psychological health (for a review, see McNally et al., 2003). Indeed, a number of systematic reviews have investigated the efficacy of PD (e.g., Australian Centre for Posttraumatic Mental Health, 2013; National Institute for Clinical Excellence (NICE), 2005; Rose & Bisson, 1998; Rose et al., 2002; Rose, Bisson & Wessely, 2003; van Emmerik, Kamphuis, Hulsbosch & Emmelkamp, 2002). These have unanimously concluded that the effect of PD is equivocal to no intervention in reducing the risk of developing PTSD, and may even be detrimental to long-term adjustment.

The discovery of negative outcomes in particular has led to recommendations that the routine use of PD should be ceased with victims of trauma (e.g., NICE, 2005; Rose et al., 2002). Two studies have become influential in PD research due to such findings; that is, those of Bisson et al. (1997) and Mayou et al. (2000). In the first of these studies, Bisson et al. (1997) randomly assigned a sample of adult burn victims to a debriefing (CISD) or no debriefing condition. To evaluate the effectiveness of the debriefing, PTSD symptomatology was assessed prior to the intervention, and at three and 13 months post-intervention. Results indicated that those who received debriefing were more likely to have PTSD at 13 month follow-up than controls. Similarly, Mayou et al. (2000) examined the effectiveness of PD for survivors of motor vehicle accidents by randomly assigning participants to a debriefing or no
debriefing condition. PTSD symptomatology was assessed pre-intervention and at four months and three years post-intervention. Their results indicated that debriefed participants who initially reported high levels of distress were still symptomatic at follow-up, whereas those who were not debriefed managed to recover.

While these iatrogenic effects are concerning, a number of methodological flaws have been identified in both of the abovementioned studies. As a result, the validity of their findings has been questioned (Dyregrov & Regel, 2012; Hawker, Durkin, & Hawker, 2011; Rose et al., 2002; Yule, 2001). Indeed, treatment and control groups were non-comparable at baseline in both studies. Specifically, debriefed participants were more severely injured than controls. Interestingly, when this was controlled for, Mayou et al. (2000) found that the negative effect of debriefing at follow-up was reduced to marginal significance ($p < 0.07$). Furthermore, debriefed participants in Bisson et al.’s (1997) study reported higher psychological sequelae at baseline than the control group. This is noteworthy given that baseline symptomatology was the only variable that predicted posttraumatic symptomatology at follow-up (Bisson et al., 1997). Other methodological concerns that have been identified relate to the debriefing procedures that were employed. Specifically, debriefing procedures were not standardized in these studies, raising concerns about internal validity. Furthermore, both studies administered debriefing individually, precluding generalisation of the negative results to group-based debriefing procedures.

Indeed, conclusions about the ineffectiveness and potentially harmful effects of PD have been largely based on the findings of studies that have administered debriefing individually. This has been attributed to a paucity of methodologically sound field studies utilizing group debriefing procedures (Devilly & Annab, 2008; Tuckey, 2007). Thus, while reviewers such as Rose et al. (2002) have recommended that "compulsory debriefing of
victims of trauma should cease" (p. 2), they have been "unable to comment on the use of group debriefing" (p. 11). This has prompted calls for more randomised trials of group debriefing, leading to an increase in studies investigating this debriefing format (e.g., Devilly & Annab, 2008; Tuckey & Scott, 2014). As a result, a review of the literature investigating the efficacy of group debriefing is warranted. Indeed, it is plausible that this mode of debriefing may lead to more favourable outcomes than individual debriefing due to the inclusion of social support, which has been consistently associated with positive outcomes in PTSD research (for a review, see Hobfoll et al., 2007). Therefore, the efficacy of group debriefing has been investigated here in a systematic review of the literature (see Appendix A for complete review).

1.6.4 A Systematic Review of Group Debriefing

A systematic review of the literature was conducted to investigate the effectiveness of single-session group debriefing in mitigating post-traumatic and/or associated psychopathology in adults following exposure to trauma. Studies were identified by searching the following electronic databases (see Appendix A for search terms used): PsycINFO (1806-February, 2014), Medline (1946- February, 2014), CINAHL (1982- February, 2014) and EMBASE (1947- February, 2014). Additionally, the author conducted a manual search of articles published in the Journal of Traumatic Stress since 2010. Finally, the reference lists of included studies were hand-searched for additional relevant trials. The last search was performed on 26 February, 2014.

The combined search resulted in a total of 2,746 articles which were screened by the author. To be eligible for inclusion in the current review, studies were required to satisfy the following criteria:
1. A group debriefing intervention had been provided to participants within four weeks of exposure to trauma (real or analogue). The rationale for this timeframe was based on the DSM-5 (APA, 2013) diagnostic criteria for PTSD which requires symptoms to be present for at least four weeks to meet caseness. Thus, interventions provided within the first month after exposure would be considered ‘preventative’, whereas interventions provided after this time would be considered ‘curative’ (van Emmerik et al., 2002);

2. Debriefing procedures contained key elements of Mitchell’s (1983) CISD protocol, although not necessarily adhering to the procedure. That is, discussions involved reviewing factual information related to the traumatic event, examining emotional reactions, normalization of these reactions, and a discussion of coping strategies;

3. The effects of group debriefing were examined in relation to posttraumatic or associated psychopathology;

   a. For the purposes of this review, posttraumatic or associated psychopathology referred to:

      i. Anxiety;
      ii. Mood;
      iii. Negative emotion;
      iv. Trauma-like symptomatology;

4. Outcomes were assessed both pre- and post-intervention using valid and reliable measures;
5. At least one comparison/control group was included consisting of individuals who were exposed to the same trauma;

6. Participants were aged 16 years or older.

Nine studies were identified that met the eligibility criteria. Six of these were randomised controlled trials (RCTs; i.e., Adler et al., 2008; Adler et al., 2009; Devilly & Annab, 2008; Devilly & Varker, 2008; Tuckey & Scott, 2014; Wu et al., 2011), two employed a self-selecting approach during group allocation (i.e., De Gaglia, 2006; Ruck, Bowes & Tehrani, 2013), and the remaining study, despite being described as an RCT, assigned participants to groups based on individual availability and other commitments (Deahl et al., 2000). A data extraction table was developed outlining information pertaining to sample characteristics, time since trauma exposure, experimental conditions, group size, debriefing duration, outcome measures used, results, and the nature of follow-up assessments (see Appendix A).

Two raters assessed risk of bias for each study using three separate scales which have been used in previous reviews of debriefing (e.g., Rose et al., 2002): the Cochrane Collaboration Quality Assessment Tool (as outlined in the Cochrane Collaboration Handbook; Higgins & Altman, 2008); the CCDAN quality rating scale (Moncrieff, Churchill, Drummond & McGuire, 2001), and; a quality assessment measure specifically developed for studies of debriefing (Kenardy & Carr, 1996). Since it was specifically designed to evaluate debriefing studies, ratings based on the Kenardy and Carr (1996) scale were used to compare the quality of studies. There were no discrepancies in quality ratings between raters, thus, these ratings informed the subsequent quality scores.
Overall, methodological quality of the included studies was relatively uniform, at an intermediate level. Most studies gave adequate information regarding the source of the sample, thorough descriptions of debriefing procedures, and used reliable and valid self-report outcome measures. However, common methodological shortcomings related to a lack of objective measures being used, inadequate randomization and allocation concealment, a lack of information obtained regarding pre-morbid vulnerability factors, and not reporting specific trauma information.

**Description of studies included in the review**

All of the included studies involved a single, group-debriefing session. These ranged from 40 to 180 minutes in duration. Seven studies specifically used CISD as their debriefing intervention (Adler et al., 2008; Deahl et al., 2000; Devilly & Annab, 2008; Devilly & Varker, 2008; Ruck et al., 2013; Tuckey & Scott, 2014; Wu et al., 2011). The two remaining studies (Adler et al., 2009; De Gaglia, 2006) used interventions that were based on the principles of PD as outlined by Mitchell (1983). All studies included a no intervention (control) condition and/or an alternative active condition. Alternative active conditions varied across studies and included stress management education classes, an intervention called 512 PIM (which comprised of all CISD stages plus an additional cohesion training stage, during which participants play games to encourage team cooperation), and another intervention called Battlemind Training which is used with soldiers and aimed at facilitating transition to civilian life post-deployment. This intervention emphasizes group cohesion, peer support and adapting combat skills to enable a successful transition to civilian life.

Participants were recruited from a variety of populations. As a result, the types of traumatic experiences varied across studies. Four studies recruited military personnel who were exposed to war-time environments (Adler et al., 2008; Adler et al., 2009; Deahl et al.,
2000) or involved in disaster rescue efforts (Wu et al., 2011). Two studies evaluated the
efficacy of debriefing following exposure to work-related trauma in a sample of fire-fighters
(De Gaglia, 2006; Tuckey & Scott, 2014), and one in a sample of prison staff (Ruck et al.,
2013). Two studies used analogue trauma stimuli (i.e., a distressing video) with university-
recruited participants (Devilly & Annab, 2008; Devilly & Varker, 2008).

The timing of interventions varied. Some studies provided debriefing immediately
(Devilly & Annab, 2008; Devilly & Varker, 2008), or soon after exposure (within 24 hours,
De Gaglia, 2006; timing not specified, Ruck et al., 2013; within three days, Tuckey & Scott,
2014). Military-based studies generally involved longer time intervals (up to one month,
Adler et al., 2008; within 14 days upon return, Adler et al., 2009; median 25 days, Wu et al.,
2011), with only one of these studies providing debriefing immediately upon return from duty
(Deahl et al., 2000). The number of participants attending each debriefing session ranged
from 4 to 32, although one study did not report group sizes (Wu et al., 2011).

A variety of outcome measures were used across studies, however, all were
standardised and had good psychometric properties. Seven studies used self-report measures
of post-traumatic stress symptomatology (Adler et al., 2008; Adler et al., 2009; Deahl et al.,
2000; Devilly & Annab, 2008; Devilly & Varker, 2008; Ruck et al., 2013; Tuckey & Scott,
2014). These included the Posttraumatic Stress Disorder Checklist (Weathers, Litz, Herman,
Huska, & Keane, 1993), Posttraumatic Stress Diagnostic Scale (Foa, 1995), PTSD Symptom
Scale-Self-report (Foa, Riggs, Dancu, & Rothbaum, 1993), the Impact of Events Scale
(Horowitz et al., 1979), Impact of Event Scale-Revised (Weiss, 2004), and Impact of Event
Scale-Extended (Tehrani et al., 2002), PTSS-10 (Holen, Sund & Weisaeth, 1989), and the
Post-traumatic Stress Scale (Foa et al., 1993). Only one study (i.e., Wu et al., 2011) measured
PTSD symptomatology via clinical interview (using the Structured Interview for PTSD;
Davidson, Smith & Kudler, 1989). One study did not examine post-traumatic symptomatology (De Gaglia, 2006), but used a state version of the Multiple Affect Adjective Checklist – Revised (Zuckerman, Lubin & Rinck, 1983) as the main outcome measure to investigate negative affect pre- and post-debriefing.

A variety of outcome measures were used across studies to assess general psychopathology. Seven studies measured depression and/or anxiety symptoms (Adler et al., 2008; Adler et al., 2009; Deahl et al., 2000; Devilly & Annab, 2008; Devilly & Varker, 2008; Ruck et al., 2013; Wu et al., 2011). Measures that were used included the Depression, Anxiety, Stress Scales (Lovibond & Lovibond, 1995), General Health Questionnaire (Goldberg & Hillier, 1979), Generalised Anxiety and Depression scale (Goldberg, 1971), Hospital Anxiety and Depression Scale (Zigmond & Snaith, 1983), Patient Health Questionnaire for Depression (Spitzer, Kroenke, Williams, & Patient Health Questionnaire Primary Case Study Group, 1999), and Center for Epidemiological Studies – Depression Scale (Radloff, 1977). Two studies (i.e., Deahl et al., 2000; Tuckey & Scott, 2014) assessed general distress using the Symptom Checklist-90-Revised (Derogatis, 1996) and the Kessler-10 (Andrews & Slade, 2001). Two studies (i.e., Adler et al., 2008; Deahl et al., 2000) assessed alcohol misuse using the Alcohol Use Disorder Identification Test (Babor, Higgins-Biddle, Saunders, & Monteiro, 2001) and the CAGE questionnaire (Ewing, 1984). Finally, only one study (i.e., Adler et al., 2008) assessed aggressive behavior using the Conflict Tactics Scale (Straus, 1979) and used an objective measure of distress (i.e., an OMRON wrist blood pressure monitor) to measure participants’ heart rates.
Results

Debriefing versus no intervention

Seven studies investigated the impact of group debriefing on PTSD symptoms compared to a control condition. Six of these found no impact of group debriefing (Adler et al., 2008; Deahl et al., 2000; Devilly & Annab, 2008; Devilly & Varker, 2008; Tuckey & Scott, 2014; Wu et al., 2011). One study found that debriefing led to a significant reduction in PTSD symptomatology, whereas no significant change was observed for the control group over time (Ruck et al., 2013).

With respect to associated pathology, seven studies investigated the impact of group debriefing on symptoms of anxiety and depression. Of these, five found no significant difference between debriefed and non-debriefed participants (Adler et al., 2008; Devilly & Annab, 2008; Devilly & Varker, 2008; Ruck et al., 2013; Wu et al., 2011), while two studies found a positive effect of group debriefing. Specifically, De Gaglia (2006) found that debriefed individuals reported significantly lower levels of negative affect than non-debriefed individuals. Further, Deahl et al. (2000) found that debriefed participants reported fewer symptoms of anxiety and depression at six-month follow-up compared to non-debriefed participants. Two studies which compared group debriefing to a no intervention control condition examined alcohol misuse. One obtained favourable outcomes for PD, in that debriefed participants reported significantly lower rates of alcohol consumption than controls (Deahl et al., 2000), whereas the second study found that CISD participants reported increased alcohol use over time compared to controls (Adler et al., 2008).

Debriefing versus alternative intervention

Four studies investigated the impact of group debriefing on PTSD symptomatology compared to an alternative intervention. Three studies compared debriefing to a stress
management class. Of those, two found that group debriefing was more effective in reducing symptoms for those with higher degrees of trauma exposure (Adler et al., 2008; Adler et al., 2009) while the final study found no difference (Tuckey & Scott, 2014). One study compared group-based CISD to 512 PIM (i.e., a group-based intervention comprised of CISD plus a cohesion training stage) and found that 512 PIM was more effective in reducing PTSD symptomatology than CISD alone (Wu et al., 2011).

Four studies investigated the impact of group debriefing on associated psychopathology compared to an alternative intervention. One study investigated alcohol use and compared group debriefing to a stress management class (Adler et al., 2008). Results indicated that group debriefing led to increased alcohol use over time. Two studies investigated the impact of group PD on symptoms of depression compared to a stress management class. One study found that PD was more effective for those with high levels of combat exposure in particular (Adler et al., 2009), while the second study found no difference (Adler et al., 2008). Finally, one study compared group-based CISD to 512 PIM when investigating the effect of PD on symptoms of anxiety and depression, with 512 PIM found to be more effective in reducing symptoms than CISD (Wu et al., 2011).

**Conclusion of systematic review**

While previous reviews have concluded that single-session individual debriefing is ineffective and may even be harmful (e.g., Rose et al., 2002); the efficacy of group debriefing has been largely unknown. This review of group-based PD largely replicates the individual PD literature. That is, there is little evidence of benefit for group debriefing on PTSD symptoms. Indeed, only one out of nine studies found a benefit. Interestingly, the results of two studies suggest that group PD may be more beneficial for individuals exposed to higher levels of trauma; however, further studies are required to replicate these results before
confident inferences can be made. With respect to associated psychopathology, there was also relatively equivocal evidence for benefits on depression and anxiety. Finally, while no study showed a negative impact of group debriefing on symptoms of PTSD, anxiety and depression, one out of two studies did find increased alcohol use.

When compared to alternative interventions, the current review indicates that stress education classes, in which symptoms of stress and adaptive coping strategies are reviewed, appear to be no more efficacious than group debriefing. However, given that only three studies included such interventions, future systematic reviews should investigate the effectiveness of this intervention more specifically. Interestingly, one study found that an alternative group intervention (i.e., 512 PIM), which contained all of the CISD stages as well as a cohesion training stage, led to a greater reduction in PTSD, anxiety and depression symptoms in a sample of Chinese soldiers than CISD alone (Wu et al., 2011). The researchers attributed the effectiveness of the alternative intervention to the additional cohesion training element which was proposed to have developed participants’ sense of belonging and perceived levels of social support. While this finding is promising, further studies are required to replicate these results before confident inferences can be made about the effectiveness of this intervention.

Thus, in summary, the majority of studies in the current review of group debriefing indicate that when compared to natural recovery over time, this early intervention procedure results in equivocal outcomes (with respect to PTSD symptomatology and associated psychopathology). As a result, group debriefing interventions based on Mitchell’s (1983) CISD model cannot be recommended for use with victims of trauma on the basis of the available evidence.
1.6.5 Psychological Debriefing and Memory Integrity

As evidenced above, much empirical attention has focused on the negative effects of debriefing on psychological wellbeing. More recently, however, the impact of debriefing on memory for the traumatic event has attracted research attention. Specifically, during group debriefing sessions, emergency service personnel may encounter erroneous information about the event from other witnesses and this misinformation may become integrated into the original memory store. Given that these personnel are often required to provide eyewitness testimony during legal proceedings, the potential ramifications of inaccurate memories for the event raise considerable concern. Indeed, eyewitness testimony is still regarded as one of the most influential forms of evidence (Wells et al., 1998). Thus, contaminated memories may compromise the reliability of this evidence and consequently the justice and fairness of the legal system. As a result, in investigating the effects of post-trauma debriefing, ensuring that the accuracy and integrity of memories is protected is also essential.

Indeed, extant literature indicates that individuals can unknowingly incorporate erroneous post-event information into memory, leading to distorted accounts (see Loftus, 2005, for a review). This phenomenon is referred to as the ‘misinformation effect’, and has been demonstrated in victims following exposure to analogue trauma stimuli and true traumatic events. For example, Nourkova, Bernstein and Loftus (2004) found that a minority of participants (12.5%) who had witnessed the 1999 Moscow bombing reported that they had seen a wounded animal at the scene after this was suggested to them. That is, a false memory was able to be planted for an event for which participants felt strong emotion.

Further research has also demonstrated that complete, elaborate memories and not simply minor details can be implanted into childhood memories, such as experiencing an overnight hospitalisation (Hymen, Husband & Billings, 1995) and being lost in a shopping
centre (Loftus & Pickrell, 1995). More often than not, once incorporated into memory, individuals confidently believe that these inaccurate memories are accurate and they experience difficulty when this is challenged (Loftus & Pickrell, 1995). This emphasises the relative ease with which misinformation can contaminate memory, and further, how easily witnesses may come to believe these accounts are accurate.

Loftus (2005) outlined three main ways that misinformation can be incorporated into memory. The first of these is via leading questions. That is, by being asked suggestive questions that contain erroneous information, individuals have been found to incorporate the false information into their memory for the event (e.g. Cole & Loftus, 1979; Hilgard & Loftus, 1979; Loftus, 1975; Loftus, Miller & Burns, 1978; Loftus & Palmer, 1974). The second way is through exposure to inaccurate or misleading media reports or post-event narratives (e.g., Allen & Lindsay, 1998; Belli, Lindsay, Gales & McCarthy, 1994; Searcy, Barlett, & Memon, 2000), and the third is via discussion with others. The final means of transferring misinformation is most relevant to group debriefing procedures and is well documented among co-witnesses (e.g., Allan & Gabbert, 2008; Gabbert, Memon & Allan, 2003; Hope, Ost, Gabbert, Healey & Lenton, 2008; Meade & Roediger, 2002; Mudd & Govern, 2004; Pickel, 2004; Paterson, Kemp & Ng, 2011; Roediger, Meade & Bergman, 2001; Wright, Self & Justice, 2000).

Indeed, of the three abovementioned avenues, Paterson and Kemp (2006) found that discussion among co-witnesses had the strongest impact on memory for the event. This reinforces findings that misinformation is more misleading when presented socially (i.e., during discussion with others), than non-socially (i.e., in narrative form; Gabbert, Memon, Allan & Wright, 2004). Furthermore, and of most concern, is that this effect appears to be resilient to attempts to prevent it by warning individuals that they may have been exposed to
misinformation (see Paterson et al., 2011). Accordingly, police officers often separate co-witnesses to prevent post-event discussion and preserve the validity of testimonies (Paterson & Kemp, 2005). Nevertheless, the same standards are not applied to emergency service personnel, as debriefing procedures (e.g., CISD) promote co-witness discussion of critical incidents. A valid question to ask then is whether these double standards are justified.

In fact, research suggests that police officers are no more accurate than civilians in their memories of events (Gabbert et al., 2003; Ost, Green & Cherryman, 2004; Wright et al., 2000). Further, it appears that they are also susceptible to post-event misinformation effects (Granhag, Memon, Gabbert, & Allwood, 2004). Thus, emergency service personnel may be susceptible to misinformation effects during the process of psychological debriefing. Indeed, initial investigations using analogue trauma stimuli have demonstrated that the process of group debriefing can compromise recall when erroneous information is provided by one participant (e.g., Devilly et al., 2007). In this study, university students were shown a video of a car accident and randomly assigned to one of three conditions: (1) debriefing (in the form of CISD), (2) debriefing with a confederate supplying misinformation, (3) no debriefing. Participants who had been given misinformation by a confederate were more likely to recall this information as fact than other participants. Moreover, these findings were maintained at follow-up, suggesting that once false memories are implanted, they are fixed over time. Nevertheless, the underlying mechanisms responsible for susceptibility to misinformation effects during debriefing procedures have not been identified in research to date and require investigation (Devilly et al., 2007).

Not only can receipt of misinformation lead to inaccurate memories, but evidence also suggests that these memories are held more confidently than accurate memories (Devilly et al., 2007; Loftus & Pickrell, 1995; Ryan & Geiselman, 1991; Weingardt, Leonesio & Loftus,
1994). This has been attributed to the notion that false memories are based upon stable ideas whereas accurate memories are based on unstable narratives (Devilly et al., 2007). This further reinforces the need to investigate whether specific group debriefing procedures, which promote “collective remembering” (Basden, Basden & Henry, 2000, p. 497), may lead to confidently held, inaccurate memories of a distressing event.

### 1.7 The Present Investigation

Thus, the available research investigating the efficacy of PD has raised concerns about the utility of this intervention in protecting individuals exposed to trauma from subsequent psychopathology. Specifically, evidence suggests that individuals who undergo debriefing may have an increased risk of developing PTSD (e.g., Bisson et al., 1997) and may also be susceptible to the misinformation effect (e.g., Devilly et al., 2007). These findings indicate that debriefing practices, as they are currently performed, may do more harm than good, particularly in an emergency services context. This has led to recommendations that PD should no longer be used with victims of trauma (e.g., Rose et al., 2002); however, such recommendations do not address the need for organisations to replace this intervention with something more effective.

To aid in the development of new post-trauma interventions which mitigate these negative effects, it would be worthwhile to build upon research in both areas by pinpointing specific stages common to many debriefing protocols that may be responsible for these outcomes. Furthermore, it would be helpful to ascertain whether particular means of conducting these discussions, namely whether these stages are administered in an individual or group format, may also contribute to negative outcomes. Indeed, Dyregrov and Regel (2012) argue that the key question to address is *how* post-trauma early interventions should be conducted to minimise adverse effects, as opposed to the question of whether or not to
intervene at all. Currently, the literature lacks such efforts to examine what, if any, aspects of post-trauma intervention procedures are unhelpful (Devilly & Annab, 2008). Instead, whole models are being dismissed as ineffective, resulting in an unproductive and polarizing debate. Thus, the current study aims to address this gap in the literature by examining specific components that are common to many debriefing procedures, and their mode of administration, to identify possible PD procedures that may be hindering recovery and contributing to the negative effects observed in the literature.

1.7.1 Discussion of Emotions

Everly and Mitchell (1995) claim that emotional ventilation is critical for effective post-trauma recovery as this provides individuals with the opportunity to achieve catharsis. As a result, the discussion of emotional reactions has been a common feature of many PD models (e.g. CISD’s reaction phase). Nevertheless, it has been argued that encouraging emotional ventilation soon after a traumatic event may be too overwhelming for some individuals and may actually impede recovery following trauma exposure as a result (see McNally et al., 2003; Ursano et al., 2000). Indeed, some research has provided support for this claim. For example, an RCT conducted by Sijbrandij, Olff, Reitsma, Carlier and Gersons (2006) investigated the effect of what they termed ‘emotional debriefing’ on post-trauma psychological adjustment and found that those who participated in this form of debriefing reported more PTSD symptoms at follow-up than non-debriefed participants. It is important to note, however, that all CISD stages but the teaching phase were administered in the emotional debriefing intervention used in this study, thus, the negative effects obtained cannot be confidently and exclusively linked to the discussion of emotions alone. Nevertheless, Seery, Silver, Holman, Ence and Chu (2008) conducted a study following the traumatic events of September 11, 2001 and found that participants who did not express their
emotional reactions to the event (via written responses) reported better mental and physical health outcomes (i.e., levels of general distress, PTSD symptoms and physical health ailments) compared to those who did express their emotional reactions.

A number of explanations have been proposed outlining why emotional ventilation, in the context of debriefing, may be unhelpful for post-trauma psychological adjustment. Some suggest that the process of habituation cannot be adequately achieved with a short, one-off procedure involving the vivid re-experiencing of emotions associated with a traumatic event (Devilly & Varker, 2008; Devilly et al., 2006; Hobfoll et al., 2007). Indeed, many PTSD theories (e.g., cognitive theory, Ehlers & Clark, 2000; emotional processing theory, Foa & Kozak, 1986; dual processing theory, Brewin et al., 1996) advocate for gradual and prolonged exposure to trauma stimuli in treatment to provide individuals with ample opportunity to successfully modify their fear responses and return to a calm state. Furthermore, repeated exposure sessions are recommended to facilitate long-term adjustment. Thus, the intense re-living of emotional responses in the context of debriefing, which does not allow for such exposure experiences, may impede habituation (Devilly et al., 2007). Furthermore, this procedure may reinforce or exacerbate distress (Devilly et al., 2007), placing individuals at greater risk for the development of PTSD. Indeed, Brewin et al. (1999) found that the best predictor of the development of PTSD was the amount of re-experiencing and arousal symptoms experienced by individuals shortly following trauma. Similarly, other studies have found that high levels of arousal immediately following trauma were associated with increased risk for the development of PTSD (Bryant et al., 2000; Carlier et al., 1997; Schell, Marshall & Jaycox, 2004). Thus, these findings provide some support for the suggestion that encouraging the vivid re-experiencing of emotional reactions by reviewing them in a one-off discussion held shortly after a traumatic event may actually be unhelpful.
Furthermore, Sijbrandij et al. (2006) propose that interventions which demand emotional ventilation shortly after a traumatic event may not allow the individual time to master his or her own emotional response, leading to negative appraisal of one’s sense of mastery. This is proposed to reinforce helplessness and keep individuals in a high state of arousal (Raphael & Wilson, 2000). Consequently, Sijbrandij et al. (2006) suggest that a more adaptive response would involve a period of rest during which emotional discussions about the event are reduced.

Given that recounting and re-experiencing traumatic reactions may be unhelpful for post-trauma psychological adjustment, this raises questions as to why debriefing protocols, which aim to mitigate the effects of trauma and accelerate recovery, contain stages that require participants to discuss and re-live their emotional reactions to a traumatic event (e.g., the reaction phase in CISD). Indeed, it is plausible that this practice may contribute to the negative effects of debriefing on psychological wellbeing which have been described in the literature. Thus, the first aim of the current empirical study is to investigate whether emotional discussion about an analogue trauma can lead to poor psychological health outcomes. If this link can be made, then this can inform the development of more effective post-trauma interventions which omit such procedures.

1.7.2 Discussion of Factual Information

Dyregrov and Regel (2012) suggest that the discussion of factual information may be more helpful than emotional expression in facilitating psychological adjustment post-trauma. They attribute this to the idea that fact-focused discussions would aid the development of a more organised and integrated trauma memory. This is in line with the cognitive model of PTSD proposed by Ehlers and Clark (2000), which highlights the nature of the trauma memory as an important factor in the development of this disorder. Indeed, studies have
found that disorganised trauma memories are predictive of ASD and PTSD (e.g., Amir, Stafford, Freshman, & Foa, 1998; Halligan, Clark, & Ehlers, 2002; Harvey & Bryant, 1999; Murray, Ehlers, & Mayou, 2002), whereas a more organised and integrated trauma memory is associated with a reduction in PTSD symptomatology (e.g., Krans, Naring, Holmes, & Becker, 2009; Foa, Molnar & Cashman, 1995). Given that fact-based discussions are a common feature of many psychological debriefing models (e.g., CISD’s fact phase), and facilitate the development of a more complete narrative and understanding of the event, an examination of the efficacy of this practice in facilitating post-trauma psychological adjustment is warranted.

Upon further examination, however, it would appear that the process of discussing factual details about the traumatic event may be problematic in terms of preserving accurate memories. Given that discussion among co-witnesses has been shown to be a highly powerful way in which misinformation can be incorporated into memory (e.g., Paterson & Kemp, 2005), and that misinformation given during group debriefing can contaminate participants’ memories (e.g., Devilly et al., 2007), it seems plausible that discussion of factual information during these procedures more specifically may allow for the transfer of misinformation during debriefing. If this process can be identified as the means by which misinformation is transferred during PD, this will assist with the development of more effective post-trauma interventions. Therefore, the second aim of the current empirical study is to build on the findings of Devilly et al. (2007) by attempting to identify whether discussing factual information, which requires participants to collaboratively recall factual details of a shared traumatic event, contributes to the negative effects of debriefing on eyewitness memory.
1.7.3 Format of Discussion

The present study will also examine whether the effects of emotion and fact-focused discussions on psychological wellbeing and memory integrity differ depending on the format within which discussions are held (i.e., individual versus group setting). While systematic reviews indicate that both individual (e.g., Rose et al., 2002) and group (see Appendix A) administrations of psychological debriefing are mostly ineffective and may be iatrogenic, it is plausible that the effect of specific components may differ depending on the format of these discussions. Thus, in order to facilitate the development of more efficacious post-trauma early interventions, it would be helpful to elucidate whether the format of discussions may moderate outcomes.

Given that social support has been consistently associated with positive outcomes for psychological wellbeing in PTSD research (for a review, see Hobfoll et al., 2007), it is plausible that discussions may lead to more favourable outcomes for psychological wellbeing when delivered in a group setting. Indeed, unlike individual discussions, group discussions provide participants with the opportunity to experience empathy, normalisation and validation of their reactions by others who have been exposed to the same stressful experience (Galinsky & Schloper, 1994). This form of social support has been found to be particularly helpful in aiding post-trauma psychological adjustment (e.g. Jacobson, 1986). For example, Cutrona and Russell (1990) found that uncontrollable situations, such as traumatic events, require greater levels of emotional support than controllable situations which require more practical support. Thus, the absence of such support during individual discussions may impede post-trauma psychological adjustment.

While group discussions may facilitate psychological adjustment via the availability of social support, it is unlikely that they would be helpful in terms of preserving accurate
memories for the event. Indeed, co-witness discussion is associated with poorer outcomes in terms of memory integrity (Paterson & Kemp, 2005). Thus, it is likely that group-based discussions, which require the collaborative recall of factual information from all participants, will pose the greatest risk for memory contamination compared to individually-administered discussions. Furthermore, inaccurate memories that develop from these discussions may also be held more confidently (e.g., Devilly et al. 2007). Given that currently used psychological debriefing procedures often involve stages during which participants are required to discuss factual information in a group setting, it is important to investigate whether this procedure may be problematic and impact on the quality and reliability of eyewitness testimony.

1.7.4 Hypotheses

In this experimental study, participants viewed a short clip of a forensic autopsy which was used as analogue trauma stimuli. We then examined whether three differently focused discussions (emotion-focused, fact-focused, non-event focused control) had different effects on participants’ psychological wellbeing, memory for an event, and confidence in their memories. The differently focused discussions were administered to participants individually or in a group format (4-8 individuals per group) to further examine whether the format within which discussions were administered had different effects on the outcomes of interest. An ecologically valid misinformation paradigm was used (such as that used in Gabbert et al., 2003) in which group members were unknowingly shown slightly different sources of information and then naturally introduced conflicting information during their discussion of the event. A one-week follow-up was utilized to examine whether immediate effects were maintained over time.
It is hypothesised that, after viewing the mildly stressful video:

**Psychological wellbeing:**

1. Participants who discuss their emotional reactions will report higher levels of trauma-type symptomatology both immediately and one-week later compared to those who discuss factual or non-event related information, due to having to re-experience their negative emotions (Brewin et al., 1999).

2. Those who discuss their emotional reactions in an individual setting will report the highest levels of trauma-type symptomatology both immediately and one week later, due to having to re-experience their negative emotions in the absence of social support which may prevent the opportunity for empathy, normalisation and validation of reactions (Galinsky & Schloper, 1994).

**Memory integrity:**

1. Due to the promotion of collaborative recall (Gabbert et al., 2003), participants who discuss factual details of the video in a group setting will be more likely to report information from the alternative version of the video both immediately and one week later than participants in other conditions. That is, these participants will be most susceptible to the misinformation effect.

2. In line with previous findings (e.g., Devilly et al., 2007), these participants will also report higher confidence ratings for inaccurate memories compared to accurate memories both immediately and one week later.
Chapter 2

METHOD

2.1 Design

The present study employed a 3 x 2 between subjects design investigating whether there was a difference in the effect of post-incident discussion content (fact-focused, emotion-focused, unrelated) and discussion format (individual, group-based) on participants’ psychological wellbeing and memory integrity. Further, some variables were assessed at multiple time points and thus time was a repeated measures variable.

2.2 Participants

The sample consisted of 177 first-year undergraduate students (35 male, 142 female) at the University of Sydney, with a mean age of 20.54 (SD = 5.19), who participated in exchange for course credit. Individuals with a history or current diagnosis of Posttraumatic Stress Disorder were excluded from participating, due to the potentially distressing nature of the analogue trauma stimuli. This was achieved by both outlining the exclusion criteria in the study’s advertisement and by asking participants prior to the commencement of the experiment. The study was approved by the Human Research Ethics Committee of the University of Sydney (Appendix B). Data collection was undertaken between August 2012 and October 2013. When conducting analyses for the current study, there were no significant
interactions between gender and any of the independent variables, thus they will not be considered further (all \( p > .09 \)).

2.3 Materials

All visual stimuli were presented on computer and all outcome measures administered on computer via an online survey tool (LimeSurvey).

2.3.1 Visual Stimuli

Two slightly different versions of an autopsy video were used as analogue trauma stimuli (Version A and B). The two five-minute videos presented the same visual footage depicting the removal of a brain from a cadaver. Key differences related to the content of the voiceover describing the procedure as a forensic autopsy of the victim of a serial killer (see Appendix C.1 for video commentaries). The videos were selected for use as they have been found to significantly increase participant stress responses and lead to a misinformation effect when the differing content is discussed among participants (Monds, Paterson, & Whittle, 2013; Paterson, Whittle, & Kemp, 2014).

Each five-minute video is split into two sections. First, a woman in a white lab coat provides details about the victim and the hypothesized nature of the crime. Subsequently, the video depicts the autopsy being carried out by two pathologists, while a voiceover describes the procedures being performed as well as other important details. Voiceovers differed between videos on a total of 9 items of information (see Table 1.3). The use of headphones ensured that participants did not hear the commentary of other participants’ videos.
Table 2.1

*Differences between information presented in version A and B of the forensic autopsy video.*

<table>
<thead>
<tr>
<th>Video Stimuli</th>
<th>Version A</th>
<th>Version B</th>
</tr>
</thead>
<tbody>
<tr>
<td>Location of Body</td>
<td>Car Park</td>
<td>Park</td>
</tr>
<tr>
<td>Date Body Discovered</td>
<td>25th January, 2005</td>
<td>25th February, 2005</td>
</tr>
<tr>
<td>Location of trauma</td>
<td>Head and neck</td>
<td>Head and legs</td>
</tr>
<tr>
<td>Length of time serial killer at large</td>
<td>Three years</td>
<td>Two years</td>
</tr>
<tr>
<td>Number of bodies found at murder scene</td>
<td>Two</td>
<td>Information not mentioned</td>
</tr>
<tr>
<td>Details about differences between murder victims</td>
<td>Information not mentioned</td>
<td>The victim described in the video differed as he was much older than other victims of the serial killer</td>
</tr>
<tr>
<td>Suggestion of how trauma to the head occurred</td>
<td>Forceful bodily violence on the part of the serial killer</td>
<td>Slamming the victims head against an object: perhaps the side of a car</td>
</tr>
<tr>
<td>Disease clusters on brain surface</td>
<td>Identified as meningitis</td>
<td>Only identified as disease clusters, not meningitis</td>
</tr>
<tr>
<td>Time needed for brain to harden in fixing solution</td>
<td>Information not mentioned</td>
<td>Five to six weeks</td>
</tr>
</tbody>
</table>
2.4 Measures

Psychological Wellbeing

2.4.1 State-Trait Anxiety Inventory

The state version of the State-Trait Anxiety Inventory (STAI-S; Spielberger et al., 1983; Appendix C.2) is a 20-item self-report measure that assesses temporary symptoms of anxiety which develop in response to different situations. Participants are required to rate how well positive (10 items, e.g., “calm”) and negative (10 items, e.g., “anxious”) emotions apply to them in a given situation using a four-point Likert scale (1 = Not at all, 4 = Very much so). Responses relating to positive emotions are reverse-scored. All scores are then summed to provide a Total Score that may range between 20 and 80, with higher scores indicating greater anxiety. This measure was selected as it has been shown to be highly reliable and valid (see Spielberger et al., 1983). It was administered at four time points throughout the experiment: at baseline, after viewing the video, immediately after discussion, and at one week follow-up. Analysis of internal consistency in the current study revealed Cronbach’s alpha coefficient of α=.90.

2.4.2 Depression, Anxiety, Stress Scales

The Depression, Anxiety, and Stress Scale (DASS-21; Lovibond & Lovibond, 1995; Appendix C.3) is a 21-item self-report questionnaire which consists of three subscales measuring symptoms of depression, anxiety and stress. Each subscale consists of seven items, including: ‘I couldn't seem to experience any positive feeling at all’ (Depression scale); ‘I felt scared without any good reason’ (Anxiety scale); and ‘I found it hard to wind down’ (Stress scale). Participants rate how much a particular statement applied to them over the past week.
on a four-point Likert scale (0 = *Did not apply to me at all*; 3 = *Applied to me very much, or most of the time*). Separate scores are generated for each of the subscales, with higher scores reflecting greater symptomatology. The DASS-21 has been shown to be a reliable measure of Depression (α = .88), Anxiety (α = .82) and Stress (α = .90), and demonstrates good validity, correlating positively with other measures of anxiety and depression (Henry & Crawford, 2005). The measure was administered at three time points; at baseline, immediately after discussion, and at one-week follow-up. Analysis of internal consistency in the current experiment revealed Cronbach’s alpha coefficient of: α=.81 for the depression subscale, α=.71 for the anxiety subscale, and α=.83 for the stress subscale.

### 2.4.3 Positive and Negative Affect Scales

The Positive and Negative Affect Scales (PANAS; Watson, Clark, & Tellegen, 1988; Appendix C.4) is a 20-item self-report questionnaire consisting of two subscales that measure positive affect (PA; 10 items) and negative affect (NA; 10 items). Participants are required to rate the extent to which each item (a single adjective representing an emotion state, e.g., ‘enthusiastic’) applied to them during a specified time period on a five-point Likert scale (1 = *Very slightly or not at all*, 5 = *Extremely*). In this experiment, the PANAS was administered at three time points: at baseline (measuring PA and NA experienced during the past week), immediately after discussions (measuring PA and NA experienced since viewing the video), and at one-week follow-up (measuring PA and NA experienced in the seven days post-discussion). The measure has demonstrated high internal consistency for each of the PA (α = 0.89) and NA (α = 0.85) subscales, and good construct validity (Crawford & Henry, 2004). In this study, analysis of internal consistency revealed Cronbach’s alpha coefficients of α=.87 for the PA subscale and α=.77 for the NA subscale.
2.4.4 The Impact of Event Scale – Revised

The Impact of Event Scale – Revised (IES-R; Weiss & Marmar, 1997; Appendix C.5) is a 22-item self-report questionnaire that assesses subjective distress related to a specific traumatic event. In the current study, instructions were amended so participants indicated distress in relation to the visual stimulus (autopsy video). The IES-R consists of three subscales related to DSM-IV diagnostic criteria for PTSD: Avoidance (e.g., ‘I stayed away from reminders of it’), Intrusion (e.g., ‘Any reminder brought back feelings about it’), and Hyperarousal (e.g., ‘I felt irritable and angry’). Participants are required to rate how distressing each symptom had been over the past seven days on a five-point Likert scale (0=Not at all, 4=Extremely). The measure has been used extensively with clinical and non-clinical populations and its subscales have demonstrated good psychometric properties with high internal consistency (α=0.96; Creamer, Bell, & Failla, 2003), and good construct validity (Weiss & Marmar, 1997). The IES-R was administered during the second session of the experiment only (i.e., one week follow-up) as participants are required to rate their distress “during the past seven days”. Analysis of internal consistency in the current experiment revealed Cronbach’s alpha coefficient of α=.90.

Memory

Participants’ memories for the video were assessed using a free recall task and a true/false recognition questionnaire.

2.4.5 Free Recall Task

The free recall task (see Appendix C.6) required participants to write down as much information as they could remember about the video. Two guiding questions were used in order to stimulate participants’ memories. Firstly, participants were required to recall as much
information as possible conveyed to them by the presenter in the white lab coat before the autopsy had begun. Secondly, participants were required to recall as much information conveyed to them about the procedures used and the discoveries made after the autopsy had begun. Participants were instructed to be as specific as possible in their recount of the video.

A scoring key was used (see Appendix C.7) to code participant responses based on the presence of: (a) correct items (one point was allocated for any item of information present in the video that was correctly recalled by participants), (b) confabulation (one point was allocated for each item of information that was reported but not present in either version of the video), and (c) misinformation (one point was allocated for each item of information recalled that was present in the alternative version of the video and thus not viewed by the participant). Free recall responses were obtained at two time points, post-discussion and one-week follow-up.

2.4.6 Recognition Task

The recognition task (see Appendix C.8) was a 28-item true/false questionnaire relating to specific information within the video (e.g., item 1: “The victim’s name was John”). Participants were asked to indicate whether the statement was true or false, and rate the confidence with which they gave their answer using a five-point Likert scale (1 = Extremely unsure to 5 = Extremely confident). Of the 28 items, nine related to information that differed between the two video versions (e.g., item 7: “The body was discovered on the 25th January, 2005”). For these items, whether the answer participants gave was correct or incorrect depended on the version of the video that they viewed. The remaining 19 items had stable correct and incorrect answers, which did not vary depending on the version of the video the participant had viewed. Specifically, nine had correct “true” answers, and 10 had correct “false” answers.
2.5 Procedure

Participants were recruited through an online research participation system (Sona Systems; see Appendix D for recruitment advertisement). Due to the nature of the Sona Systems program, participants could view the number of individuals that could attend each testing session (i.e., either one or eight per timeslot). Therefore, this enabled a self-selecting approach to discussion format condition whereby individuals could have selected or avoided specific timeslots based on the number of participants who could attend. A random number generator program (www.random.org) was used to randomise the content of discussions (i.e., fact, emotion, non-event related).

The experiment consisted of two sessions (time one and one-week follow-up). The first experimental session, which lasted approximately 60 minutes, was held at a University of Sydney laboratory with participants attending either individually or in groups of 4-8. Prior to beginning the experiment, participants attending in groups were asked whether or not they knew their group members, as a prior relationship may have affected data collection. No participants reported knowing each other.

Participants completed all measures individually online using the program LimeSurvey (Version 1.91+; Schmitz & LimeSurvey Project Team, 2012). Identical computer workstations were set up next to each other in the laboratory in a way that ensured each participant would not be able to view other participants’ screens. Prior to the commencement of the experiment, participants were seated in front of a computer and warned by the experimenter that they would be watching a short recording of a forensic autopsy. They were informed that the purpose of the study was to examine psychological and cognitive reactions to forensic procedures. Thus, whilst active deception was not employed, the true purpose of the study was withheld as knowledge of this may have influenced
participants’ behaviour. Participants read an information sheet describing the nature of the study (Appendix E.1), completed their consent forms (Appendix E.2) and were informed of their right to withdraw at any stage without penalty. The experimenter then answered questions regarding participation.

Once participants agreed to continue, they were asked to complete the first battery of questionnaires on demographic information and psychological wellbeing (including the STAI-S, DASS-21, and PANAS). Subsequently, participants were informed that they would now watch the autopsy video and were asked to pay close attention to the information given about the situation and the procedures described in the video. Next, the experimenter asked participants to put on a set of headphones and subsequently started the videos. Once participants had viewed the video, they were asked to complete the second administration of the STAI-S, followed by a filler task (Appendix C.9). This task consisted of reading a passage and circling words where the letter “N” appeared.

After completing the filler task, participants were subjected to one of three experimental protocols, depending on the content condition to which they had been randomly assigned. All participants were told that they would be tested on the content of the video they had just viewed, but would be required to participate in a discussion with the experimenter (undertaken individually, or in groups, depending on the discussion format condition). Participants in the control condition discussed information that was unrelated to the content of the autopsy video. To ensure standardisation, they were asked to describe their shoes, talk about what they had for breakfast, discuss their university studies and talk about what they did during the previous weekend.

Participants in the emotion-focused discussion condition were told that they were only allowed to discuss the emotional reactions they experienced while watching the video and the
intensity of these emotions. In addition, participants were instructed that they were not to speak about any factual content contained within the video. These instructions were given as they correspond to the instructions given during the reaction phase of CISD (Everly & Mitchell, 1995; see Appendix C.10 for a transcript of instructions). Participants in the fact-focused discussion condition were told that they were only allowed to discuss the factual content of the video. Participants were instructed that they were not to discuss any emotional reactions they had to the video, and to only focus on factual details that the video contained. These instructions were given as they correspond with the instructions given during the fact phase in CISD (Everly & Mitchell, 1995; see Appendix C.10 for transcript of instructions).

To guide participants in the emotion- and fact-focused discussion conditions, it was suggested that they discuss the information presented to them in two sections: before the autopsy began and while the autopsy was being performed. Since CISD and other debriefing protocols do not stipulate that discussions should be ceased after a certain period of time, the experimenter did not terminate discussions until participants indicated that they had no more to add. Accordingly, the average length of discussions was five minutes. Following the discussion, participants completed the third battery of questionnaires which assessed psychological wellbeing (including the STAI-S, DASS-21, and PANAS) and memory for the video (free recall and recognition tasks).

Once participants completed the first time point of the experiment, they were thanked for their participation thus far and informed that they would receive an email seven days later which would include the link to the follow-up questionnaire. Participants were informed that they were required to complete the follow-up questionnaire within 24 hours of receipt of the email. This could be completed online at any location. The follow-up questionnaire battery included measures of psychological wellbeing (including the STAI-S, DASS-21, PANAS and
IES-R) and memory (free recall and recognition tasks). Upon completion of the follow-up questionnaire, a debriefing form was presented to participants on screen explaining the true purpose of the experiment and the reasons that deception was used (Appendix E.3). Furthermore, the contact details for the university Psychology Clinic were provided in case participants needed to discuss the video with anyone after the experiment had concluded.

A diagrammatic representation of the procedure is provided in Figure 2.1 below.

---

**Figure 2.1.** Experimental flow chart showing the procedure of the study from pre-assessment to one-week follow-up.
2.6 Discussion Characteristics

There were 15 group discussions conducted: 5 fact-focused \((n = 33)\), 6 emotion-focused \((n = 36)\), and 4 unrelated to the event \((n = 25)\). Group sizes ranged from 4 to 8 participants per group. The average length of group discussions was 5.55 minutes (6.36 minutes for fact-focused; 6 minutes for emotion-focused, and 4.56 minutes for unrelated group discussions). The remaining participants \((n = 83)\) were involved in individual discussions. Of these, 28 participated in fact-focused discussions, 27 in emotion-focused discussions, and 28 in unrelated discussions. The average length of individual discussions was 3 minutes (4 minutes for fact-focused, 3.30 minutes for emotion-focused and 2 minutes for unrelated).

2.7 Statistical Analyses

Statistical analyses were conducted using IBM SPSS 19.0. For all analyses, an alpha level of .05 was used (unless otherwise stated).

2.7.1 Preliminary Analyses

A series of 2 x 3 ANOVAs were used to determine whether there were any pre-existing differences between discussion format (individual, group) and discussion content (fact, emotion, non-event related) conditions on measures of psychological wellbeing at baseline. Where significant differences occurred, variables were included as covariates in subsequent analyses.

In order to determine whether the video produced stress in all participants, a mixed-model 2 (format: group by individual) x 3 (content: fact by emotion by non-event related) x 2 (time: before and after the video) ANCOVA was performed. It was anticipated that stress
would be experienced by all participants (a main effect for time), but that differences would not yet have emerged between the group and format conditions since this was prior to the administration of the experimental manipulation.

2.7.2 Primary Analyses

Psychological Wellbeing Measures

To assess the effects of discussion format and content on trauma-related symptomatology (i.e., as measured by the STAI-S, DASS-21, and PANAS) over time (baseline, immediately post-discussion, one-week follow-up), a series of 2 (format) x 3 (content) x 3 (time) repeated measures ANCOVAs were used. Since the Impact of Event Scale-Revised was only administered at one time-point (one-week follow-up), a series of univariate ANCOVAs were conducted, controlling for covariates, to analyse the effects of discussion format and content on intrusions, avoidance, hyperarousal and total PTSD score. Where assumption of sphericity was not met, the Greenhouse-Geisser adjustment was made to the degrees of freedom.

Memory Tasks

A series of 2 (format) x 3 (content) x 2 (time: post-discussion, one-week follow-up) mixed repeated measures ANOVAs were used to assess the impact of the experimental manipulations on memory accuracy over time. Where significant interactions were found, post-hoc ANOVAs were employed, as necessary, to analyse between group differences. Simple effect analyses were also conducted to analyse within group differences, with a Bonferroni adjusted alpha of .025.
Chapter 3

RESULTS

This chapter will begin with preliminary analyses, including baseline comparisons and a manipulation check. Primary analyses are then presented in two parts. First, analyses will examine the effects of discussion format and content on psychological wellbeing. Then, the effects of format and content conditions on memory accuracy will be presented.

3.1 Preliminary Analysis

3.1.1 Baseline Comparisons

Differences in state anxiety (as measured by the STAI-S), depression, anxiety and stress (as measured by the DASS21), and positive and negative affect (measured by the PANAS) were analysed via a series of 2 x 3 ANOVAs. Means and standard deviations for all baseline measures are presented in Table 3.1. Results revealed a significant main effect of format for baseline depression scores, $F(1, 171) = 7.48, p < .01$. That is, those in the group format condition reported higher depression scores at baseline ($M = 3.10, SD = 3.45$) compared to those in the individual discussion format condition ($M = 1.95, SD = 1.98$). As a result, baseline depression was entered as a covariate in subsequent psychological wellbeing analyses. The main effect of content and the format by content interaction were both non-significant ($ps > .23$). The main effects and interactions were all non-significant for state anxiety, stress, positive affect and negative affect (all $ps > .32$).
Table 3.1

Raw means and standard deviations for baseline measures across format and content conditions

<table>
<thead>
<tr>
<th>Measure</th>
<th>Individual</th>
<th></th>
<th></th>
<th>Group</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Fact</td>
<td>Emotion</td>
<td>Unrelated</td>
<td>Fact</td>
<td>Emotion</td>
<td>Unrelated</td>
</tr>
<tr>
<td></td>
<td>(n = 28)</td>
<td>(n = 27)</td>
<td>(n = 28)</td>
<td>(n = 33)</td>
<td>(n = 36)</td>
<td>(n = 25)</td>
</tr>
<tr>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
</tr>
<tr>
<td>STAI-S</td>
<td>32.82 (9.51)</td>
<td>33.70 (8.19)</td>
<td>33.04 (8.62)</td>
<td>33.06 (8.79)</td>
<td>32.36 (8.46)</td>
<td>32.80 (7.36)</td>
</tr>
<tr>
<td>DASS-D</td>
<td>1.96 (1.97)</td>
<td>2.04 (1.74)</td>
<td>1.86 (2.24)</td>
<td>3.91 (3.84)</td>
<td>2.28 (2.54)</td>
<td>3.20 (3.87)</td>
</tr>
<tr>
<td>DASS-A</td>
<td>1.75 (2.19)</td>
<td>2.19 (2.00)</td>
<td>1.71 (2.28)</td>
<td>2.61 (2.85)</td>
<td>2.03 (2.44)</td>
<td>1.88 (1.96)</td>
</tr>
<tr>
<td>DASS-S</td>
<td>5.07 (4.23)</td>
<td>5.33 (3.31)</td>
<td>4.46 (4.18)</td>
<td>5.73 (4.18)</td>
<td>3.94 (3.55)</td>
<td>5.20 (4.22)</td>
</tr>
<tr>
<td>PANAS-PA</td>
<td>34.18 (7.45)</td>
<td>32.81 (6.65)</td>
<td>32.61 (6.53)</td>
<td>33.70 (7.89)</td>
<td>31.36 (7.65)</td>
<td>33.04 (8.07)</td>
</tr>
<tr>
<td>PANAS-NA</td>
<td>16.25 (5.09)</td>
<td>16.26 (3.57)</td>
<td>17.46 (5.12)</td>
<td>17.39 (4.78)</td>
<td>15.97 (4.77)</td>
<td>17.06 (5.00)</td>
</tr>
</tbody>
</table>

Note: STAI-S: State-Trait Anxiety Inventory – State scale; DASS-D: DASS21 Depression scale; DASS-A: DASS21 Anxiety scale; DASS-S: DASS21 Stress scale; PANAS-PA: Positive Affect scale; PANAS-NA: Negative Affect scale.

3.1.2 Manipulation Check

A mixed repeated measures ANCOVA was conducted, controlling for depression, to test whether the video stimuli successfully increased participants’ state anxiety levels.

Participants’ baseline STAI-S scores (pre-video) were compared to those obtained immediately after viewing the stimuli (post-video). The analysis revealed a significant main effect of time, $F(1, 170) = 49.70, p < .01$. That is, on average, participant state anxiety levels increased significantly from pre- ($M = 32.94, SD = 8.43$) to post-video exposure ($M = 40.19, SD = 10.93$). The main effect of content was non-significant, $F(2, 170) = .71, p = .49$, as was the main effect of format, $F(1, 170) = .28, p = .10$. Furthermore, there were no significant interactions, all $ps > .17$. 
3.2 Primary Analyses - Psychological Wellbeing

3.2.1 State Anxiety

A 2 x 3 x (3) ANCOVA (controlling for depression) revealed a significant main effect of time, $F(1.87, 317.91) = 15.05, p < .01, \eta_p^2 = .08$. Contrasts revealed that state anxiety increased significantly from baseline to post-discussion $F(1, 170) = 37.06, p < .01, \eta_p^2 = .18$, and then decreased significantly from post-discussion to follow-up $F(1, 170) = 14.45, p < .01, \eta_p^2 = .08$. There was also a significant main effect of discussion format, $F(1, 170) = 7.60, p < .01, \eta_p^2 = .04$, indicating that participants involved in group discussions reported lower state anxiety, on average, than those in the individual discussion format condition. The main effect of content was non-significant, $F(2, 170) = .50, p = .61, \eta_p^2 = .01$. Furthermore, there were no significant interactions, all $p$s > .28. Means and standard deviations across format and content conditions over all three time points are presented in Table 3.2.

Table 3.2

*Mean STAI-S scores over time points, across discussion format and content conditions.*

<table>
<thead>
<tr>
<th>Stage</th>
<th>Individual</th>
<th></th>
<th>Group</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Fact</td>
<td>Emotion</td>
<td>Unrelated</td>
<td>Fact</td>
</tr>
<tr>
<td></td>
<td>$(n = 28)$</td>
<td>$(n = 27)$</td>
<td>$(n = 28)$</td>
<td>$(n = 33)$</td>
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<tr>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
</tr>
<tr>
<td>Baseline</td>
<td>32.82 (9.51)</td>
<td>33.70 (8.19)</td>
<td>33.04 (8.62)</td>
<td>33.06 (8.79)</td>
</tr>
<tr>
<td>Post-disc.</td>
<td>38.61 (11.46)</td>
<td>40.57 (11.07)</td>
<td>40.29 (9.70)</td>
<td>39.15 (10.75)</td>
</tr>
<tr>
<td>Follow-up</td>
<td>35.39 (13.05)</td>
<td>36.00 (8.21)</td>
<td>36.57 (11.74)</td>
<td>35.61 (10.81)</td>
</tr>
</tbody>
</table>
3.2.2 Depression, Anxiety and Stress

Depression

Due to the significant difference in depression scores at baseline, a 2 x 3 x (2) ANCOVA was conducted, with baseline depression included as a covariate, to assess the impact of discussion format and content on depression over time (post-discussion to one-week follow-up). Results revealed a significant main effect of format, $F(1, 170) = 4.39, p < .05, \eta_p^2 = .03$, indicating that those who participated in group discussions had lower depression scores on average than those involved in individual discussions (See Table 3.3 for means and standard deviations). The main effect of content was non-significant, $F(2, 170) = .28, p = .75, \eta_p^2 = .00$, as was the main effect of time, $F(1, 170) = .58, p = .45, \eta_p^2 = .00$. Furthermore, all interactions were non-significant, all $ps > .55$.

Table 3.3

Mean depression scores over time, across discussion format and content conditions.

<table>
<thead>
<tr>
<th>Stage</th>
<th>Individual</th>
<th>Group</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Fact</td>
<td>Emotion</td>
</tr>
<tr>
<td></td>
<td>(n = 28)</td>
<td>(n = 27)</td>
</tr>
<tr>
<td>Post-disc.</td>
<td>M (SD)</td>
<td>M (SD)</td>
</tr>
<tr>
<td>Follow-up</td>
<td>2.50 (2.74)</td>
<td>2.41 (2.28)</td>
</tr>
<tr>
<td></td>
<td>1.89 (2.51)</td>
<td>1.89 (2.52)</td>
</tr>
</tbody>
</table>

Anxiety

A 2 x 3 x (3) ANCOVA, with baseline depression included as a covariate, indicated that the main effect of time was non-significant, $F(1.68, 285.31) = 2.51, p = .09, \eta_p^2 = .02$. The main effect of format was non-significant, $F(1, 170) = 1.96, p = .16, \eta_p^2 = .01$, as was
the main effect of content, $F(2, 170) = .93, p = .40, \eta_p^2 = .01$. Furthermore, all interactions were non-significant, all $ps > .15$.

**Stress**

A 2 x 3 x (3) ANCOVA revealed a significant main effect of time, $F(1.56, 265.75) = 5.64, p < .01, \eta_p^2 = .03$. Contrasts indicated that stress scores decreased significantly from post-discussion to follow-up $F(1, 170) = 9.40, p < .01, \eta_p^2 = .05$, however, there was no significant difference in stress scores between baseline and post-discussion, $F(1, 170) = 2.20, p = .14, \eta_p^2 = .01$. The main effect of format was significant, $F(1, 170) = 7.16, p < .01, \eta_p^2 = .04$. That is, participants in the group format condition reported lower levels of stress overall than those in the individual format condition (see Table 3.4 for means and standard deviations). Furthermore, the main effect of content and all interactions were non-significant, all $ps > .10$.

Table 3.4

*Mean stress scores over time points, across discussion format and content conditions.*

<table>
<thead>
<tr>
<th>Stage</th>
<th>Individual</th>
<th></th>
<th></th>
<th>Group</th>
<th></th>
<th></th>
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</thead>
<tbody>
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<td></td>
<td>Fact</td>
<td>Emotion</td>
<td>Unrelated</td>
<td>Fact</td>
<td>Emotion</td>
<td>Unrelated</td>
</tr>
<tr>
<td></td>
<td>($n = 28$)</td>
<td>($n = 27$)</td>
<td>($n = 28$)</td>
<td>($n = 33$)</td>
<td>($n = 36$)</td>
<td>($n = 25$)</td>
</tr>
<tr>
<td></td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
</tr>
<tr>
<td>Baseline</td>
<td>5.07 (4.23)</td>
<td>5.33 (3.31)</td>
<td>4.46 (4.18)</td>
<td>5.73 (4.18)</td>
<td>3.94 (3.55)</td>
<td>5.20 (4.22)</td>
</tr>
<tr>
<td>Post-disc.</td>
<td>5.54 (4.33)</td>
<td>6.22 (4.17)</td>
<td>5.25 (4.27)</td>
<td>5.27 (4.59)</td>
<td>4.56 (3.76)</td>
<td>5.12 (4.65)</td>
</tr>
<tr>
<td>Follow-up</td>
<td>3.82 (4.09)</td>
<td>3.96 (3.45)</td>
<td>5.00 (5.49)</td>
<td>3.45 (3.79)</td>
<td>3.28 (3.70)</td>
<td>3.84 (4.47)</td>
</tr>
</tbody>
</table>

**3.2.3 Positive and Negative Affect**

**Positive Affect**

Results of the 2 x 3 x (3) ANCOVA revealed a significant main effect of time, $F(1.80, 305.49) = 77.64, p < .01, \eta_p^2 = .31$. Contrasts revealed that positive affect decreased
The University of Sydney

significantly from baseline to post-discussion $F(1, 170) = 147.56, p < .01, \eta^2_p = .47$, and then increased significantly from post-discussion to follow-up $F(1, 170) = 56.33, p < .01, \eta^2_p = .25$ (see Table 3.5 for means and standard deviations). The main effect of content revealed a non-significant trend, $F(2, 170) = 2.59, p = .08, \eta^2_p = .03$, as did the time by content interaction, $F(3.59, 305.49) = 2.14, p = .08, \eta^2_p = .03$. The main effect of format and all other interactions were non-significant, all $ps > .35$.

Table 3.5

*Mean positive affect scores over time, across discussion format and content conditions.*

<table>
<thead>
<tr>
<th>Stage</th>
<th>Individual</th>
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<th></th>
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<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
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<td>Fact (n = 28)</td>
<td>Emotion (n = 27)</td>
<td>Unrelated (n = 28)</td>
<td>Fact (n = 33)</td>
<td>Emotion (n = 36)</td>
<td>Unrelated (n = 25)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Baseline</td>
<td>34.18 (7.45)</td>
<td>32.81 (6.65)</td>
<td>32.61 (6.53)</td>
<td>33.70 (7.89)</td>
<td>31.36 (7.65)</td>
<td>33.04 (8.07)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post-disc.</td>
<td>26.93 (7.54)</td>
<td>25.00 (7.50)</td>
<td>22.61 (6.64)</td>
<td>25.73 (8.28)</td>
<td>27.06 (7.27)</td>
<td>24.28 (8.33)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Follow-up</td>
<td>32.96 (9.69)</td>
<td>29.19 (7.86)</td>
<td>28.89 (9.04)</td>
<td>30.73 (8.91)</td>
<td>29.11 (7.88)</td>
<td>31.04 (9.05)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Negative Affect**

The analysis indicated a significant main effect of time, $F(1.56, 265.65) = 3.78, p < .05, \eta^2_p = .02$. Contrasts revealed that there was no significant difference in negative affect between baseline and post-discussion, $F(1, 170) = 1.79, p = .18, \eta^2_p = .01$, however, negative affect decreased significantly from post-discussion to follow-up $F(1, 170) = 5.80, p < .05, \eta^2_p = .03$. The main effect of format was significant, $F(1, 170) = 5.94, p < .05, \eta^2_p = .03$, indicating that negative affect was lower for those involved in group discussions than those in individual discussions, averaged across time and content conditions (see Table 3.6). The main effect of content and all interactions were non-significant, all $ps > .16$. 
Table 3.6

Mean negative affect scores over time, across discussion format and content conditions.

<table>
<thead>
<tr>
<th>Stage</th>
<th>Individual</th>
<th>Group</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Fact (n = 28)</td>
<td>Emotion (n = 27)</td>
</tr>
<tr>
<td></td>
<td>M (SD)</td>
<td>M (SD)</td>
</tr>
<tr>
<td>Baseline</td>
<td>16.25 (5.09)</td>
<td>16.26 (3.57)</td>
</tr>
<tr>
<td>Post-disc.</td>
<td>15.21 (6.68)</td>
<td>18.26 (6.47)</td>
</tr>
<tr>
<td>Follow-up</td>
<td>15.93 (6.16)</td>
<td>16.22 (5.24)</td>
</tr>
</tbody>
</table>

3.2.4 Intrusion, Avoidance and Hyperarousal

A series of univariate ANCOVAs, controlling for depression, revealed no significant main effects or interactions for any of the intrusion, avoidance, hyperarousal or total IES outcome variables (all ps > .22). Means and standard deviations are presented in Table 3.7.

Table 3.7

Mean scores for IES-R outcome variables across discussion format and content groups.

<table>
<thead>
<tr>
<th>Measure</th>
<th>Individual</th>
<th>Group</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Fact (n = 28)</td>
<td>Emotion (n = 27)</td>
</tr>
<tr>
<td></td>
<td>M (SD)</td>
<td>M (SD)</td>
</tr>
<tr>
<td>IES-R-I</td>
<td>.17 (.21)</td>
<td>.25 (.34)</td>
</tr>
<tr>
<td>IES-R-A</td>
<td>.21 (.40)</td>
<td>.36 (.65)</td>
</tr>
<tr>
<td>IES-R-H</td>
<td>.03 (.09)</td>
<td>.21 (.34)</td>
</tr>
<tr>
<td>IES-R-T</td>
<td>.41 (.55)</td>
<td>.82 (1.21)</td>
</tr>
</tbody>
</table>

3.3 Primary Analyses – Memory Integrity

3.3.1 Free Recall

The free recall task was coded by two raters who were blind to the experimental condition to which participants had been allocated. Separate total scores were calculated for correct items, confabulation items and misinformation items at both time points and these formed the basis for the subsequent statistical analysis.

**Inter-rater reliability**

To assess inter-rater reliability, ten free recall responses were scored by two raters who were blind to the experimental condition to which participants had been allocated. Pearson product-moment correlation coefficients were used to calculate the degree of consistency between raters. Results indicated significant correlations between raters for correct items \((r = .97, p < .01)\), confabulation items \((r = .77, p < .05)\) and misinformation items \((r = .89, p < .01)\). Since correlation coefficients above .70 indicate strong, positive relationships between variables (Field, 2009), this suggests that there was a high level of agreement among raters in the coding of free response items.

**Correct items**

A mixed repeated measures ANOVA was conducted including correct item ratings at both time points (post-discussion and one-week follow-up). Results revealed a significant main effect of time, \(F (1, 171) = 112.21, p < .01, \eta_p^2 = .40\). That is, the number of correct items reported by participants decreased from post-discussion \((M = 30.60, SD = 8.56)\) to one-week follow-up \((M = 24.75, SD = 9.65)\). The main effect of format was non-significant, \(F (1, 171) = 1.61, p = .21, \eta_p^2 = .01\), as was the main effect of content, \(F (2, 171) = 1.78, p = .17, \eta_p^2 = .02\). Furthermore, all interactions were non-significant, all \(ps > .18\).
Confabulation items

A mixed repeated measures ANOVA including confabulation items at both time points (post-discussion and one-week follow-up) revealed a significant main effect of time, $F(1, 171) = 10.37, p < .01, \eta_p^2 = .06$. That is, the number of confabulation items reported decreased significantly from post-discussion ($M = 1.69, SD = 1.45$) to one-week follow-up ($M = 1.32, SD = 1.32$). The main effect of format was non-significant, $F(1, 171) = .44, p = .51, \eta_p^2 = .00$, as was the main effect of content, $F(2, 171) = .98, p = .38, \eta_p^2 = .01$. Furthermore, all interactions were non-significant, all $ps > .13$.

Misinformation items

The effect of discussion format and content on number of misinformation items reported over time (post-discussion, one-week follow-up) was analysed using a mixed repeated measures ANOVA. Table 3.8 reports means and standard deviations for content and format conditions over time. Results revealed a significant main effect of time, $F(1, 171) = 8.42, p < .01, \eta_p^2 = .05$. That is, the number of misinformation items reported by participants increased significantly from post-discussion ($M = .29, SD = .76$) to follow-up ($M = .47, SD = .83$). The main effect of format was significant, $F(1, 171) = 42.13, p < .01, \eta_p^2 = .20$, indicating that those in the group format condition reported more misinformation items in their free recall responses, on average, than those in the individual discussion format condition. The main effect of content was also significant, $F(2, 171) = 32.50, p < .01, \eta_p^2 = .27$. Contrasts revealed that those who discussed factual information reported more misinformation items, on average, than those who discussed emotional ($p < .01$) or unrelated ($p < .01$) content. There was no significant difference between emotion and unrelated discussion conditions ($p > .05$).
Table 3.8

Means and standard deviations for number of misinformation items reported across format and content conditions over time

<table>
<thead>
<tr>
<th>Stage</th>
<th>Fact (n = 28)</th>
<th>Emotion (n = 27)</th>
<th>Unrelated (n = 28)</th>
<th>Fact (n = 33)</th>
<th>Emotion (n = 36)</th>
<th>Unrelated (n = 25)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
</tr>
<tr>
<td>Post-disc.</td>
<td>.11 (.42)</td>
<td>.07 (.27)</td>
<td>.11 (.32)</td>
<td>1.18 (1.29)</td>
<td>.06 (.23)</td>
<td>.08 (.40)</td>
</tr>
<tr>
<td>Follow-up</td>
<td>.14 (.36)</td>
<td>.11 (.32)</td>
<td>.11 (.32)</td>
<td>1.64 (1.11)</td>
<td>.33 (.59)</td>
<td>.28 (.46)</td>
</tr>
</tbody>
</table>

The time by content interaction was non-significant, $F(2, 171) = .53, p = .59, \eta^2_p = .01$, as was the time by format by content interaction, $F(2, 171) = .34, p = .71, \eta^2_p = .00$. There was a significant time by format interaction, $F(1, 171) = 6.16, p < .05, \eta^2_p = .04$. Simple effect analyses were performed separately for individual and group discussion format conditions. The simple effect for the group discussion format condition was significant, $F(1, 91) = 9.23, p < .01, \eta^2_p = .09$, indicating that number of misinformation items reported were significantly higher at follow-up compared to post-discussion. The simple effect for the individual discussion format condition was non-significant, $F(1, 80) = .33, p = .57, \eta^2_p = .00$, suggesting that the number of misinformation items reported by participants in individual discussions was, in general, the same across time points. See Figure 3.1 for graphical representation of the interaction.
The format by content interaction was significant, $F(2, 171) = 28.97, p < .01, \eta^2_p = .25$. Simple effect analyses revealed a significant difference in the number of misinformation items reported across content conditions for those in the group format condition, $F(2, 91) = 40.55, p < .01, \eta^2_p = .47$, but no significant difference across content conditions for those in the individual format condition, $F(2, 80) = 10, p = .91, \eta^2_p = .00$. Simple contrasts revealed that those in the group condition who discussed factual information reported a significantly higher number of misinformation items compared to those who discussed emotional ($p < .01$) or unrelated ($p < .01$) content. There was no significant difference in the number of misinformation items reported between the emotion-focused and unrelated discussion conditions ($p = .93$). See Figure 3.2 for a graphical representation of the interaction.
3.3.2 Recognition Memory

Memory Accuracy

Of the 28 items contained in the recognition questionnaire, nine were directly related to information that differed between the two versions of the video. These items are hereafter referred to as “target” items as they are used to assess for the misinformation effect. The remaining 19 items related to information that was consistent across both versions of the video. These are hereafter referred to as “neutral” items as they assess participants’ knowledge for factual information contained within the stimuli. Target item accuracy was determined depending on the version of the video that participants viewed. That is, participants’ responses were classed as incorrect if the option selected corresponded with the alternate version of the video and not their own. Neutral item accuracy did not differ.
depending on the version of the video that was viewed. Aggregate scores were created and converted into percentages for target item and neutral item accuracy. Thus, higher scores indicated greater recognition accuracy. A mixed repeated measures ANOVA was used to assess the effect of discussion format and discussion content on recognition accuracy over time (post-discussion and one-week follow-up).

**Target items.** Mean and standard deviations for target item recognition accuracy are reported in Table 3.9. Results revealed a significant main effect of time, \( F (1, 171) = 18.04, p < .01, \eta^2_p = .10 \). That is, recognition accuracy decreased from post-discussion to one-week follow-up, on average. There was a significant main effect of format, \( F (1, 171) = 17.53, p < .01, \eta^2_p = .09 \), indicating those in the group format condition reported lower recognition accuracy compared to those in the individual format condition. There was a significant main effect of content, \( F (2, 171) = 7.03, p < .01, \eta^2_p = .08 \). Contrasts revealed that those in the fact-focused content condition had poorer recognition accuracy than those in the emotion-focused \((p < .01)\) and unrelated \((p < .01)\) content conditions. There was no significant difference in recognition accuracy between emotion and unrelated content conditions \((p = .53)\).

**Table 3.9**

*Means and standard deviations for target item recognition accuracy across format and content conditions over time*

<table>
<thead>
<tr>
<th>Stage</th>
<th>Individual</th>
<th></th>
<th></th>
<th>Group</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Fact ( (n = 28) )</td>
<td>Emotion ( (n = 27) )</td>
<td>Unrelated ( (n = 28) )</td>
<td>Fact ( (n = 33) )</td>
<td>Emotion ( (n = 36) )</td>
<td>Unrelated ( (n = 25) )</td>
</tr>
<tr>
<td>Post-disc.</td>
<td>73.02 (16.12)</td>
<td>75.72 (12.72)</td>
<td>77.78 (14.18)</td>
<td>57.24 (14.73)</td>
<td>73.15 (17.29)</td>
<td>75.56 (11.56)</td>
</tr>
<tr>
<td>Follow-up</td>
<td>71.82 (12.64)</td>
<td>74.49 (12.26)</td>
<td>68.65 (12.49)</td>
<td>58.25 (10.03)</td>
<td>67.90 (17.17)</td>
<td>63.56 (13.03)</td>
</tr>
</tbody>
</table>
The time by format interaction was non-significant, $F(1, 171) = .51, p = .48, \eta^2_p = .00$, as was the time by format by content interaction, $F(2, 171) = .80, p = .45, \eta^2_p = .01$. The time by content interaction was significant, $F(2, 171) = 7.76, p < .01, \eta^2_p = .08$. Post hoc analyses performed separately for fact, emotion and unrelated content conditions revealed a significant simple effect for the unrelated content condition, $F(1, 51) = 29.72, p < .01, \eta^2_p = .37$, indicating a significant decrease in recognition accuracy over time points. There was no significant change in recognition accuracy over time points for those in the fact, $F(1, 59) = .00, p = .96, \eta^2_p = .00$, and emotion, $F(1, 61) = 2.77, p = .10, \eta^2_p = .04$, content conditions (see Figure 3.3 for graphical representation).

![Figure 3.3](image_url)

*Figure 3.3.* Mean target item recognition accuracy (%) across discussion content conditions over time points.
Results also revealed a significant format by content interaction, $F(2, 171) = 3.80, p < .05, \eta^2_p = .04$. Post-hoc analyses indicated that target item recognition accuracy differed significantly across content conditions for those in the group format condition, $F(2, 91) = 10.96, p < .01, \eta^2_p = .19$. There was no significant difference in target item recognition accuracy across content conditions for those in the individual format condition, $F(2, 80) = .37, p = .69, \eta^2_p = .01$. Simple contrasts indicated that target item recognition accuracy was significantly lower for those in the group format condition who discussed factual content compared to those who discussed emotional ($p < .01$) and unrelated ($p < .01$) content. There was no significant difference in target item recognition accuracy between those who discussed emotional versus unrelated content in a group format ($p = .76$). See Figure 3.4 for graphical representation of the interaction.

*Figure 3.4. Target item recognition accuracy (%) across content and format conditions.*
Neutral items. Results indicated a significant main effect of time, $F (1, 171) = 7.28$, $p < .01$, $\eta^2_p = .04$. That is, neutral item recognition accuracy decreased significantly from post-discussion ($M = 86.50$, $SD = 7.63$) to follow-up ($M = 84.78$, $SD = 9.35$), irrespective of experimental condition. The main effect of format was non-significant, $F (1, 171) = 2.44$, $p = .12$, $\eta^2_p = .01$, as was the main effect of content, $F (2, 171) = .01$, $p = .99$, $\eta^2_p = .00$. Furthermore, all interactions were non-significant, all $p$s $> .15$.

Confidence ratings

Participants provided confidence ratings for their responses to each recognition questionnaire item on a five-point scale (1 = lowest level of confidence).

Confidence Ratings for Correct and Incorrect Responses to Target Items

Separate confidence ratings were calculated for correct and incorrect responses to target items on the recognition questionnaire. This was conducted to determine whether there was a difference in participants' confidence for misinformation which had been recalled. Mean confidence scores were calculated for correct and incorrect target items at two time points (post-discussion and one-week follow-up) and analysed using a mixed repeated measures ANOVA.

Correct responses. Descriptive statistics are presented in Table 3.11. Results revealed a significant main effect of time, $F (1, 171) = 6.29$, $p = .01$, $\eta^2_p = .04$, suggesting that confidence in correct responses to target items decreased from post-discussion ($M = 3.93$, $SD = .67$) to follow-up ($M = 3.81$, $SD = .76$). Further, the main effect of format was significant, $F (1, 171) = 8.64$, $p < .01$, $\eta^2_p = .05$. The main effect of content was non-significant, $F (2, 171) = 1.36$, $p = .26$, $\eta^2_p = .02$. The time by format interaction was non-significant, $F (1, 171) = 2.61$, $p = .11$, $\eta^2_p = .02$, as was the time by content interaction, $F (2, 171) = .16$, $p = .85$, $\eta^2_p$
Further, the time by format by content interaction was non-significant, $F(2, 171) = .17, p = .85, \eta^2_p = .00$.

There was a significant format by content interaction, $F(2, 171) = 4.58, p = .01, \eta^2_p = .05$. Post-hoc analyses revealed a significant difference in confidence ratings across content conditions for those in the group format condition, $F(2, 91) = 5.80, p < .01, \eta^2_p = .11$. There was no significant difference in confidence ratings across content conditions for those in the individual discussion format condition, $F(2, 80) = .53, p = .59, \eta^2_p = .01$. Simple effect contrasts indicated that those in the group format condition who discussed factual information reported significantly higher confidence levels than those who discussed emotional ($p < .01$) or unrelated ($p < .05$) information. There was no significant difference in confidence ratings between those who discussed emotional versus unrelated information ($p = .52$).

Table 3.10

Means and standard deviations for correct target item response confidence ratings over time, across discussion format and content conditions.

| Stage      | Individual | | | | | | Group | | | |
|------------|------------|------------|------------|------------|------------|------------|------------|------------|------------|------------|------------|
|            | Fact (n = 28) | Emotion (n = 27) | Unrelated (n = 28) | Fact (n = 33) | Emotion (n = 36) | Unrelated (n = 25) |
| Post-disc. | M (.SD) | M (.SD) | M (.SD) | M (.SD) | M (.SD) | M (.SD) |
|            | 3.74 (.72) | 3.95 (.56) | 3.79 (.69) | 4.33 (.56) | 3.82 (.68) | 3.91 (.63) |
| Follow-up  | 3.56 (.74) | 3.68 (.80) | 3.64 (.63) | 4.25 (.68) | 3.78 (.67) | 3.90 (.92) |

Incorrect responses. Descriptive statistics are presented in Table 3.12. The main effect of time was non-significant, $F (1, 162) = .12, p = .73, \eta^2_p = .00$. There was a significant main effect of format, $F (1, 162) = 21.42, p < .01, \eta^2_p = .12$. The main effect of content was significant, $F (2, 162) = 4.04, p < .05, \eta^2_p = .05$. The time by format interaction was non-significant, $F (1, 162) = .86, p = .36, \eta^2_p = .01$, as was the time by content interaction, $F (2,
Further, the time by format by content interaction was non-significant, $F(2, 162) = .24, p = .79, \eta^2_p = .00$.

The format by content interaction was significant, $F(2, 162) = 4.70, p = .01, \eta^2_p = .06$. Post-hoc analyses revealed a significant difference across content conditions for those in the group discussion format condition, $F(2, 89) = 7.09, p < .01, \eta^2_p = .14$, but not for those in the individual discussion format condition, $F(2, 73) = 2.27, p = .11, \eta^2_p = .06$. Simple effect contrasts examining differences across content conditions for those in the group format condition indicated that confidence ratings for incorrect responses to target items were significantly higher overall for those who discussed factual information compared to those who discussed emotional ($p < .01$) or unrelated ($p < .01$) content. There was no significant difference in confidence ratings for those in the group format condition who discussed emotional content compared to those who discussed unrelated content ($p = .82$).

Table 3.11

<table>
<thead>
<tr>
<th></th>
<th>Individual</th>
<th></th>
<th>Group</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Stage</td>
<td>Fact</td>
<td>Emotion</td>
<td>Unrelated</td>
<td>Fact</td>
</tr>
<tr>
<td></td>
<td>(n = 28)</td>
<td>(n = 27)</td>
<td>(n = 28)</td>
<td>(n = 33)</td>
</tr>
<tr>
<td></td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
<td>M (SD)</td>
</tr>
<tr>
<td>Post-disc.</td>
<td>2.99 (.89)</td>
<td>3.34 (.94)</td>
<td>2.74 (1.21)</td>
<td>3.92 (.84)</td>
</tr>
<tr>
<td>Follow-up</td>
<td>2.94 (.80)</td>
<td>3.15 (1.04)</td>
<td>2.84 (.78)</td>
<td>4.08 (.84)</td>
</tr>
</tbody>
</table>
Chapter 4

DISCUSSION

The aim of the current study was to dismantle various components of psychological debriefing procedures to investigate the effect of fact versus emotion focused discussions on psychological adjustment and memory integrity following exposure to analogue trauma. In addition, the study sought to investigate whether the effects of these discussions differed depending on the format within which they were administered. In order to achieve these objectives, an experimental design was employed where participants engaged in one of three differently-focused discussions (i.e., fact-focused, emotion-focused or non-event related) which was administered individually or in a group setting. To assess the effects of these discussions, outcomes were assessed immediately following the discussions and at one-week follow-up.

We predicted that emotion-focused discussions would impede recovery, resulting in more trauma-type symptoms (PTSD symptoms, depression, anxiety, stress and negative affect), particularly when these discussions were conducted individually. With respect to memory integrity, it was hypothesised that those involved in group-based, fact-focused discussions would be most likely to report misinformation compared to other participants, and that these participants would be more confident in their inaccurate memories. Finally, all hypothesised effects were expected to be maintained over time. Whilst the hypotheses for memory were supported by the current results, those for psychological wellbeing were not. These findings will be discussed in further detail below.
4.1 Summary of Major Findings

4.1.1 Psychological Wellbeing

_Hypothesis One_

The hypothesized negative effect of emotion-focused discussion on post-trauma psychological recovery was not supported. Indeed, there was no significant difference in recovery across discussion content conditions. These results indicate that neither fact-focused nor emotion-focused discussions either facilitated or worsened the normal recovery which was evident over time for all participants. These results are not consistent with some findings that suggest early re-experiencing of the trauma is predictive of PTSD (e.g., Brewin et al., 1999). Neither are they consistent with some experimental studies that have shown that reviewing emotional information has a negative impact on post-trauma psychological adjustment (e.g., Seery et al., 2008; Sijbrandij et al., 2006). Nevertheless, our findings are consistent with an abundance of systematic reviews and meta-analyses which have indicated no effect of debriefing procedures as a whole over controls (e.g., Australian Centre for Posttraumatic Mental Health, 2013; National Institute for Clinical Excellence (NICE), 2005; Rose & Bisson, 1998; Rose et al., 2002; Rose, Bisson & Wessely, 2003; van Emmerik, Kamphuis, Hulsbosch & Emmelkamp, 2002). Indeed, while reviews have suggested that debriefing may be iatrogenic in some instances, on the whole they have concluded that the effect of these procedures is comparable to no intervention.

Our findings are consistent with a number of studies that have found no association between emotional expression and post-trauma symptom recovery. For example, Sloan, Marx and Greenberg (2011) investigated the effects of written emotional disclosure on PTSD symptomatology compared to a control writing condition. All participants were found to
display a significant reduction in PTSD symptom severity, with no significant difference in symptom recovery between groups. Further, Halpern, Maunder, Schwartz and Gurevich (2012) examined the efficacy of emotional expression in a sample of paramedics following a critical incident and found that, despite its perceived helpfulness, emotional expression was unrelated to PTSD symptomatology and general distress. When combined with the results of our study, these findings suggest that emotional expression is neither beneficial nor harmful for post-trauma psychological adjustment, irrespective of the mode of administration.

The comparable effects of emotion- and fact-focused discussions on psychological adjustment that we obtained is inconsistent with Dyregrov and Regel’s (2012) suggestion that reviewing factual information may be more helpful for post-trauma recovery than reviewing emotional information. Specifically, the authors proposed that fact-focused discussions may aid recovery by facilitating the development of an organized and integrated trauma memory. While various theoretical models of PTSD (e.g., Ehlers & Clark, 2000) have emphasized the importance of a coherent trauma narrative for post-trauma recovery, we found no benefit of fact-focused discussions compared to emotion-focused or non-event related discussions for psychological adjustment. Indeed, effects were comparable across groups irrespective of the format of discussion. One explanation for this finding may be that individuals with PTSD already have coherent trauma memories. Indeed, this has been proposed by Bernsten, Willert and Rubin (2003) who found that trauma memories of individuals with PTSD were no more fragmented than those of trauma-exposed individuals without PTSD. This finding has been repeatedly demonstrated in other studies (e.g., Kihlstrom, 2006; Porter & Birt, 2001; Porter & Peace, 2007; Rubin, Feldman, & Beckham, 2003). Thus, these findings, combined with the results of our study, raise questions about the utility of fact-focused discussions in early interventions following trauma. Specifically, this procedure, which aims to facilitate the
development of a coherent trauma memory, may be unnecessary if trauma narratives are already organised.

**Hypothesis two**

The hypothesized negative effect of individually-administered emotion-focused discussions was also not supported. Indeed, the effect of emotion-focused discussion on psychological adjustment was comparable to fact-focused and non-event related discussions, and this did not differ across discussion format conditions. Thus, while a lack of social support has been identified as a risk factor for PTSD development (see Hobfoll et al., 2007); there was no significant difference in patterns of symptom recovery between discussion format conditions in the current study.

A possible explanation for this finding may relate to the source of social support that was available to participants in our study. Indeed, Laffaye, Cavella, Drescher and Rose (2008) suggest that different sources of social support (i.e., listeners to whom individuals disclose) may have differing effects on PTSD recovery. Although this is an under-examined area of PTSD research, there are some studies available which provide support for this suggestion. For example, Hoyt et al. (2010) found that when soldiers and first responders disclosed emotional information to individuals who had *not* shared their traumatic combat experience (i.e., family, friends and spouses); this was associated with less PTSD symptomatology than disclosing to peers who had a common experience. Similarly, Deahl, Gillham, Thomas, Searle, & Srinivasan (1994) found that disclosure among veterans (who had a shared traumatic experience) had no significant benefit for psychological adjustment. Thus, taken together, these results suggest that disclosing to individuals who do *not* have a shared trauma experience may be more beneficial than disclosing to those who do have a shared experience. Given that participants in the current study were exposed to the same
trauma, this may be why a buffering effect of group discussion (reflecting the presence of social support) was not identified in the current study.

It is notable, however, that participants involved in individual discussions reported significantly higher levels of anxiety, depression, stress and negative affect overall, compared to those involved in group-based discussions. Nevertheless, given that the discussion format by time interaction did not reach significance, we cannot conclude that one discussion format is more helpful for psychological adjustment than another. Indeed, whilst the only significant difference found between groups at baseline was on a measure of depression, it may be the case that other pre-existing differences were not large enough at baseline to meet significance; however, over multiple assessments, the pattern continued across time and these consistent patterns were sufficiently large to reach significance.

In summary, the present findings suggest that two stages which are featured in many early intervention models (e.g., Adler et al., 2008; Adler et al., 2009; Everly et al., 2000; Ruck et al., 2013; Wu et al., 2011) are ineffective in facilitating post-trauma psychological adjustment compared to the effect of time, and this effect holds irrespective of the format of discussions. Indeed, while research to date has examined the effects of individual or group-based PD procedures as a whole, our findings provide a unique contribution to the PTSD literature by highlighting the fact that when used in isolation neither the fact-focused nor the emotion-focused components provide any benefit to psychological recovery. This raises questions about the inclusion of these procedures in post-trauma early interventions.
4.1.2 Memory Integrity

Hypothesis One

Although there was no evidence to suggest any detrimental impact of fact versus emotion focused discussions on psychopathology, group-based, fact-focused discussions did result in detrimental effects on eyewitness memory, as predicted. Specifically, compared to participants in other conditions, those who discussed factual information in a group setting were more likely to incorporate misinformation (i.e., information contained in the version of the video they did not view) into their responses on both free recall and recognition memory tasks. These findings are consistent with research indicating that discussion among individuals can lead to the transfer of misinformation (e.g., Gabbert et al., 2003; Paterson & Kemp 2006; Paterson et al., 2011). Furthermore, the findings build on research by Devilly et al. (2007) who were the first to identify a misinformation effect in the context of psychological debriefing. Specifically, while Devilly et al. (2007) concluded that psychological debriefing as a complete procedure was problematic for eyewitness memory, the current study has been able to identify a specific component of commonly-used PD procedures, and mode of delivery, as contributing to this outcome.

An important finding was that despite memory accuracy and confidence ratings decreasing over time for all participants, the number of confabulation items that were reported also decreased. Thus, while individuals may not have been able to recall many aspects of the event confidently or correctly, they did not attempt to ‘fill in the blanks’ with erroneous details. This finding is consistent with previous research which suggests that memory failure and ‘gap-filling’ does not account for confabulation (for a review, see Schnider, 2008).
An unexpected finding was that participants who discussed non-event related information demonstrated a significant decrease in recognition accuracy for misinformation items over time compared to those in fact or emotion-focused conditions. Given that misinformation was not relayed during non-event related discussions, this outcome cannot be attributed to the misinformation effect. Rather, this result may be the product of a lack of rehearsal of detailed information related to the video coupled with the forced choice nature of the recognition questionnaire. That is, since participants who discussed non-event related content did not have the opportunity to rehearse detailed information related to the video during discussions, this may have prevented them from confirming their memories for certain details (see Basden et al., 2000). Therefore, responses on the recognition questionnaire, which required individuals to select whether a statement related to the video was true or false, may simply be reflective of decreased memory accuracy over time for these items.

_Hypothesis Two_

It was hypothesized that participants who discussed factual information in a group setting would report more confidence in their inaccurate memories (reflecting the incorporation of misinformation into memory) than others. This prediction was partially supported. Indeed, unlike the findings of Devilly et al. (2007), we found that participants were more confident in their responses, irrespective of the accuracy of these responses. This reinforces previous research indicating that the relationship between eyewitness confidence and accuracy is relatively weak (Sporer, Penrod, Read, & Cutler, 1995). It may be the case that during collective remembering, reaching a consensus on whether or not the information was accurate may have heightened participants’ sense of confidence in their responses on the recognition task. It could also be that if a participant mentioned an item that was included in the video, and appeared confident in their recollection, that higher confidence ratings may be
explained by individuals readily accepting a seemingly confident and logical co-witness testimony.

In summary, our findings related to memory integrity suggest that the discussion of factual information among co-witness’s, which is a common feature of group-based psychological debriefing procedures (e.g., CISD), is particularly problematic in terms of preserving accurate memories. Specifically, not only does this procedure increase susceptibility to the misinformation effect, but it also results in increased confidence in memories, irrespective of their accuracy. This adds to existing research and supports claims that eyewitness testimony provided by individuals who have been involved in group-based post-incident debriefing procedures should be considered with caution as they may be contaminated by misinformation (see Devilly et al., 2007). Thus, in order to preserve the integrity of eyewitness memory, interventions involving group-based, fact-focused discussions should be avoided with those who may be required to provide such testimony (e.g., emergency service personnel).

4.2 Study Limitations and Strengths

While a number of potentially problematic aspects of psychological debriefing have been identified, the current study is not without limitations. In particular, we used analogue trauma stimuli to examine the effect of discussions on post-trauma psychological adjustment. Although trauma-related symptoms have been proposed to exist along on a continuum (Holmes, 2003), the video used in the present study did not elicit the same level of psychological reactivity as real world trauma. As a result, caution should be exercised in the generalization of the current results to real world situations.
Indeed, while research has shown that handling deceased bodies (e.g., by assisting with autopsies) is a risk factor for PTSD symptomatology (e.g., Ursano, Fullerton, Kao & Bhartiya, 1995), participants in the current analogue study reported relatively low levels of trauma-related symptoms after viewing the forensic autopsy video. Furthermore, there appeared to be a floor effect, with minimal ratings reported on the IES-R (Weiss & Marmar, 1997), on average, across the groups. Thus, while there may have been significant differences between groups following real trauma, the measure may not have been sensitive enough to detect PTSD symptomatology one week after exposure to analogue trauma. Hence, the lack of differences between groups could be attributable to the fact that everyone recovered well from viewing the video and hence, no intervention could improve on the passage of time.

Furthermore, it is also plausible that discussions were too short to significantly influence psychological recovery. Indeed, whilst the complete debriefing procedure is said to require 2-3 hours to complete (Mitchell, 1983), discussions in the current study lasted five minutes on average. This is reflective of the restricted nature of the discussions, in that only factual, emotional or non-event related information regarding a relatively short (five minute) video could be reviewed. Thus, it is possible that the combination of analogue trauma stimuli, a lack of sensitivity of measures and low dose of intervention may have resulted in much smaller between-group differences than were anticipated, and hence the study may be underpowered to detect these differences. Indeed, the largest partial eta-square for the between group psychological wellbeing analyses (none of which reached statistical significance) was 0.021. In order to achieve a significant effect if this was real, many more participants would have been required. Hence, the effect is likely to be so small that its clinical significance would be questionable.
Despite these limitations, the trauma film paradigm has been used extensively and successfully in PTSD research (for a review, see Holmes & Bourne, 2008). Indeed, given the ethical and logistical issues associated with conducting research following traumatic events, this methodology is particularly useful when attempting to address important theoretical and clinical questions in this area. Given that the trauma film used in the current study had been used successfully in previous research (e.g., Monds et al., 2013), the use of analogue trauma stimuli was considered appropriate to address the aims of the current study.

A second limitation in the study is the significant difference in baseline depression scores between discussion format conditions, which indicates a failure of randomization. This is consistent with the fact that individuals could self-select to be involved in individual or group-based discussions, which was a clear limitation. While it is interesting that those with higher depression scores elected to participate in group discussions, it is important to note that depression scores were relatively low overall in this non-clinical sample. This may also explain why overall, those involved in group discussions were ultimately found to report less anxiety, stress, depression and negative affect than those in individual discussions.

Given that the majority of emergency service workers are male, a third limitation of our study is the predominantly female sample. However, given the absence of significant gender interactions, our results suggest that this variable does not influence recovery over time. This finding supports claims made by Ozer et al. (2008) that gender is not plausibly implicated in the development of PTSD. Our results also suggest that gender does not influence memory accuracy. Therefore, the current findings can be generalised to both males and females.

A final limitation is that the current study only examined two stages common to many currently-used psychological debriefing models (e.g., CISD). Nevertheless, there is clear
theoretical evidence to suggest that the discussion of emotional and factual information would be particularly problematic for post-trauma psychological wellbeing and memory integrity. Indeed, research investigating the effectiveness of psycho-education, which is another component featured in many PD protocols (e.g., CISD’s teaching stage), indicates that education per se does not result in better post-trauma psychological adjustment (e.g., Sijbrandij et al., 2006; for a review, see Wessely et al., 2008). Thus, combined with our results, a number of components of PD protocols have been found to be no more helpful or harmful for psychological adjustment than no intervention, with one component and mode of delivery (i.e., group-based, fact-focused discussions) being identified as problematic for memory integrity.

A clear strength of our study was that we examined the effect of specific components of currently used debriefing procedures, and their mode of delivery, on both psychological adjustment and memory integrity over time. Furthermore, the controlled experimental design we used to isolate the effects of each independent variable was another strength. Until now, the PD literature has largely examined the effects of debriefing procedures as a whole, in either an individual or group context. Thus, while these procedures have been found to be ineffective or iatrogenic overall, the effectiveness (or ineffectiveness) of individual components has been largely unknown (Devilly & Annab, 2008). Given that numerous clinical guidelines have recommended that PD should no longer be used with individuals following exposure to trauma (e.g., Australian Centre for Posttraumatic Mental Health, 2013), this is an important issue to address. Indeed, the development of new, more effective early interventions and procedures cannot progress without an understanding of what practices of existing interventions may be helpful, harmful or ineffective.
4.3 Clinical Implications and Directions for Future Research

The present study, in conjunction with previous research, suggests that high risk organisations such as the emergency services would benefit from the development of a new post-trauma early intervention which successfully mitigates the negative psychological effects of trauma and preserves the accuracy of participants’ memories for the event. Unfortunately, however, there are no empirically-supported interventions aimed at preventing PTSD that have been identified in research to replace debriefing (see Australian Centre for Posttraumatic Mental Health, 2013; Forneris et al., 2013). Indeed, while effective treatment interventions have been identified (i.e., Trauma-Focused-CBT and EMDR); these are used with individuals who already meet diagnostic criteria for PTSD. Thus, despite recommendations that the routine use of PD should be ceased (e.g., Rose et al., 2002); the absence of an evidence-based alternative places organizations such as the emergency services at risk.

Specifically, the continued use of PD may leave organisations vulnerable to litigation should research be used as evidence to prove the negligence of employers in compromising the occupational health and safety of their workers (Devilly & Cotton, 2003). However, doing nothing (given that there is no clear empirically-supported alternative) may also increase risk of litigation for violating duty of care requirements. Furthermore, doing nothing may also increase employees’ risk for developing PTSD due to a perceived lack of social and organizational support which this practice may promote. Indeed, Barnes, Nickerson, Adler and Litz (2013) investigated the relationship between perceived organizational support (POS) and PTSD symptoms among military personnel on a peacekeeping mission in Kosovo and found that higher levels of POS were related to lower PTSD symptoms.
Therefore, there is a clear need for future research to focus on the development of a new, empirically-supported early intervention to be used with this population for two reasons: 1) to help organisations develop appropriate post-trauma policies and procedures that satisfy their duty of care requirements, and; 2) to ensure that high risk employees feel adequately supported by their organisations following exposure to traumatic events, which may minimize the risk for developing PTSD. Indeed, the development of effective post-trauma early interventions has been a focus of much clinical and research attention. This has resulted in a number of recommendations being made about which practices may be helpful to implement in the immediate aftermath of trauma.

### 4.3.1 Early Intervention Alternatives

*Psychological First Aid*

One early intervention alternative which has been increasingly used and promoted in the PTSD literature is Psychological First Aid (PFA). This intervention is administered during the immediate trauma impact phase (i.e., within four weeks of the traumatic event) with the aim of alleviating distress and fostering adaptive functioning (Brymer et al., 2006). The Psychological First Aid Field Operations Guide (Brymer et al., 2006) lists eight core components of the PFA model. These include: 1) establishing contact and engagement, 2) providing safety and comfort, 3) promoting a sense of calm, 4) identifying immediate needs and concerns, 5) offering practical assistance to address needs and concerns, 6) facilitating contact with social supports, 7) providing information about stress reactions and coping, and 8) linking individuals with available services. These components have been largely underpinned by research highlighting the essential elements that should be included in immediate and mid-term interventions following trauma-exposure (see Hobfoll et al., 2007).
Despite being evidence-informed, PFA lacks proof of effectiveness. Indeed, a number of reviews have noted the absence of solid empirical evidence for the effectiveness of this early intervention model in promoting recovery and preventing the development of PTSD (see Bisson & Lewis, 2009; Fox et al., 2012; Shultz & Forbes, 2013). Given that PFA has been widely used and propagated as a replacement for debriefing with trauma-exposed individuals (including emergency service personnel; e.g., Nash, Westphal, Watson, & Litz, 2010); this highlights the urgent need for research to address this significant gap in the literature. Importantly, not only should this intervention be found to effectively mitigate the psychological effects of trauma, but research should also ensure that PFA effectively minimises the chance for memory contamination to occur between participants.

Forbes et al. (2011) identified a number of factors that complicate effectiveness trials of PFA and may account for the paucity of research. Firstly, the use of a standardized protocol is difficult given the flexible nature of PFA. Indeed, unlike PD protocols, PFA tailors care in accordance with individual needs. Secondly, given that most individuals do not go on to develop a diagnosable disorder, large sample sizes are required to demonstrate the effectiveness of the intervention. Third, fidelity assessments are difficult to conduct in the field. Fourth, it is difficult to assess other factors (e.g., environmental, organizational, cultural) that may influence receptivity. With these challenges in mind, Forbes et al. (2011) developed a framework for the implementation and evaluation of PFA within an organizational setting, which they suggest provides an ideal context for the development and evaluation of this intervention. Their suggested model contains four phases; two pre-trauma phases that are implemented prior to a traumatic event (i.e., Phases 1 and 2), and two post-trauma phases (Phase 3 and 4).
The first phase of Forbes et al.’s (2011) PFA model focuses on addressing organizational policies and procedures to establish the legitimacy of PFA and facilitate a supportive environment. This involves the development of policies and procedures that: recognize the incidence of traumatic events and their potentially negative psychological effects; clearly delineate the roles and responsibilities of all staff in providing support following such events, and; demonstrate a commitment to providing training to ensure appropriate implementation of the intervention. The second phase of the model focuses on the promotion of such policies within the organization and training of staff in the provision of PFA in accordance with their roles and responsibilities. The third phase involves the delivery of PFA following trauma based on the eight core components previously outlined (see Brymer et al., 2006). Finally, phase four involves the ongoing monitoring and follow-up of staff. During each phase of the model, Forbes et al. (2011) detail methods of evaluating effectiveness. Thus, the researchers offer a clear framework to facilitate controlled trials of PFA elements within an organizational setting. Given that an evidence-based early intervention is urgently required for high-risk organizations such as the emergency services, future research should focus on testing the effectiveness of this phased PFA model.

**Skills for Psychological Recovery**

Where PFA has not been sufficient in addressing the needs of trauma-exposed individuals, the Australian Centre for Posttraumatic Mental Health (ACPMH) suggests that Skills for Psychological Recovery (SPR) may be a useful next step. This intervention involves providing skills-training to individuals in order to facilitate psychological recovery following exposure to trauma (see Berkowitz et al., 2010). The skills provided during this intervention are informed by research evidence and include: problem-solving, activity-scheduling, thought challenging, distress tolerance and arousal reduction. Similar to PFA,
SPR is tailored to the needs of the recipient, allowing for a more resource and time-efficient intervention (Berkowitz et al., 2010). That is, the intervention can be delivered in a single 45 minute session, or over a number of sessions. Furthermore, the types of skills that would be delivered to individuals would depend on their particular needs (Berkowitz et al., 2010). Whilst SPR is a promising intervention, there is currently a lack of research evidence into its effectiveness. Thus, similar to PFA, further research is required before this intervention can be recommended as an effective post-trauma intervention.

**A Stepped Care Approach**

Following a systematic review of the available literature, the Australian Centre for Posttraumatic Mental Health identified a number of practices that have been found to be helpful for post-trauma psychological adjustment. These are listed in The Australian Guidelines for the Treatment of ASD and PTSD (Australian Centre for Posttraumatic Mental Health, 2013), which provides recommendations for post-trauma interventions. Specifically, the Guidelines recommend a ‘stepped care’ approach with adults following exposure to a potentially traumatic event. This involves tailoring care by monitoring those who are at high risk for developing psychopathology and providing increasingly intensive interventions as required. Underpinning this approach is the well-established recognition that not everyone who is exposed to a traumatic event will develop a diagnosable disorder (Breslau, 2009; Chapman et al., 2012; Kessler et al., 1995). Therefore, unlike the one-size-fits-all approach of PD (Litz et al., 2002), the level of care that is provided from a stepped care approach is commensurate with individual needs and as such also allows for a more cost-effective intervention (see Katon, Roy-Byrne, Russo, & Cowley, 2002).

In an attempt to present a coherent model for health system application, O’Donnell, Bryant, Creamer and Carty (2008) developed a three-stepped approach to early intervention.
This involved 1) screening injury survivors to identify those at risk of developing psychopathology, 2) reassessing those who were high risk at a later point in time, and 3) early intervention for those experiencing persistent symptoms four weeks post injury (i.e., CBT). The effectiveness of each stage of the model was subsequently assessed by O’Donnell et al. (2012) with a sample of 683 hospitalised injury patients. Participants were screened during hospitalisation and high risk patients were followed up at four weeks post injury. Those with elevated symptoms of depression and anxiety were randomly allocated to receive four to ten sessions of CBT or treatment as usual (where patients could engage in any treatment they desired). Their findings indicated that patients who received early intervention had significantly improved mental health at 12 months post injury than those who were allocated to usual care. Furthermore, they found that screening procedures were able to correctly identify a significant majority (89%) of those who later developed an anxiety or mood disorder (O’Donnell et al., 2012).

Zatzick et al. (2004) also conducted a randomized effectiveness trial of stepped care for trauma exposed adults compared to usual care. The stepped care intervention involved a series of steps whereby participants were first provided with case management for six months by a trauma support specialist who monitored and coordinated interventions depending on the needs of the individual. Three months post-trauma, participants were assessed for PTSD using a structured clinical interview and those who met criteria for the disorder were offered CBT, pharmacotherapy or combined treatment. From 6 to 12 months post-trauma, participants who remained symptomatic were received ongoing trauma support and evidence-based treatment, all the while continuing contact with their case manager who monitored treatment compliance, symptoms and function. At the end of the 12 month period, those who received stepped care were found to be significantly less symptomatic with regard to PTSD
and alcohol dependence than controls. Similar results were also obtained in a more recent study by Zatzick et al. (2013), further reinforcing the benefits of a stepped care model for victims of trauma.

While these findings provide preliminary support that a stepped care approach to early intervention may be effective in improving mental health outcomes, further research is needed to examine whether this approach may be helpful with all trauma-exposed individuals. Indeed, McFarlane & Bryant (2007) highlighted the paucity of research examining screening, monitoring and the effectiveness of evidence-based treatment across high-risk occupational groups such as the emergency services. Thus, given the Australian guideline recommendations for a stepped care approach with trauma-exposed individuals, future research should investigate whether screening, monitoring and providing increasingly intensive interventions may be an effective approach with high-risk occupational groups.

4.4 Conclusion

In summary, the current study aimed to investigate the effects of differently-focused discussions (i.e., fact-focused and emotion-focused) on post-trauma psychological adjustment and memory integrity. Further, the study aimed to examine whether effects differed depending on the mode of delivery (i.e., individual or group-based discussion). We found that the effects of emotion- and fact-focused discussions did not lead to improvements or detriments in psychological adjustment compared to an unrelated discussion, irrespective of the mode of administration. With respect to memory integrity, the current study found that fact-focused, group-based discussions had the most detrimental effect on memory accuracy, with more misinformation reported by individuals in this condition. Nevertheless, these individuals were the most confident that their memories were accurate, irrespective of whether or not they actually were. These findings suggest that two procedures which are
commonly featured in many post-incident debriefing procedures are ineffective in preventing the development of PTSD compared to no intervention, and that one procedure, when delivered in a group setting, is detrimental to eyewitness memory. This adds further weight to existing research suggesting that psychological debriefing is problematic overall and should not be utilized with individuals following exposure to a potentially traumatic event.

Nevertheless, effective early intervention procedures are urgently required for high risk organisations and should be a high priority for future research. Based on preliminary evidence supporting its efficacy, organisations should work towards a stepped care model in which high risk individuals are identified, monitored and, should they develop clinically significant levels of symptoms, be treated with interventions known to be effective for PTSD in order to prevent secondary complications associated with this disorder.
References


The University of Sydney


APPENDICES
APPENDIX A

SYSTEMATIC REVIEW
The Efficacy of Group-Based Psychological Debriefing in Preventing Posttraumatic Stress Disorder: A Systematic Review
Abstract

Previous systematic reviews have concluded that individual applications of psychological debriefing (PD) should no longer be used with victims of trauma. However, due to a paucity of research trials, the efficacy of group-based PD has been largely unknown. While researchers have been attempting to fill this gap in the literature, no recent systematic review has been conducted to assess the available evidence regarding the efficacy of this debriefing format in preventing post-traumatic stress disorder and general psychopathology within one month of exposure to trauma. The current review aims to address this issue. A systematic review using the Preferred Reporting Items for Systematic reviews and Meta-Analyses (PRISMA) statement methodology was conducted. A literature search of PsycINFO, Medline, CINAHL and EMBASE databases and a manual search of the Journal of Traumatic Stress was performed to identify eligible studies. Nine studies were identified. The majority of studies found no difference between psychological debriefing and non-debriefing groups in alleviating post-traumatic stress symptoms, with only one study indicating a positive effect. There was also relatively equivocal evidence for benefits on depression and anxiety. Only one study showed a negative impact of group debriefing which was related to increased alcohol use. Two studies suggested that group PD may be more beneficial for those exposed to higher levels of trauma.

Methodological quality was assessed using three separate measures and was found to be intermediate overall across studies. While no adverse effects of were found for PTSD symptomatology, the results of the current review suggest that group debriefing interventions based on Mitchell’s (1983) CISD model do not reduce PTSD symptoms over and above the effect of time. Thus, these procedures cannot be recommended for use with victims of trauma.
1. Introduction

Research has shown that trauma exposure is highly prevalent in the general population. Indeed, a recent survey of 2,953 U.S. adults conducted by Kilpatrick et al. (2013) found that up to 89.7% of participants had been exposed to at least one traumatic event in their lifetime. While most individuals report psychological distress in response to these events (for review, see Litz, Gray, Bryant & Adler, 2002), the majority are able to successfully return to their pre-trauma level of functioning within one year (Breslau, 2009; Chapman et al., 2012; Kessler et al., 1995). Nevertheless, a minority of trauma survivors fail to recover and may go on to develop diagnosable psychiatric disorders. Specifically, posttraumatic stress disorder (PTSD) has been well-established as a predominant disorder following exposure to trauma (for a review, see Neria, Nandi, & Galea, 2008).

While PTSD prevalence rates have been found to vary across populations, research suggests that it is not a rare disorder. Indeed, Neria, Nandi and Galea (2008) identified rates of 30% to 40% among direct victims of disasters, 10% to 20% among rescue workers and 5% to 10% among the general population. These high rates are concerning, particularly given the significant impairment associated with this disorder. Specifically, PTSD is associated with increased risk for anxiety, depression, substance abuse, suicidal ideation and suicide attempts (Kessler, 2000). Further, PTSD is associated with occupational impairment (missing days and decreased efficiency) and marital stability (Kessler, 2000). Finally, more than one third of those diagnosed with PTSD will continue to experience symptoms decades after the traumatic event (Chapman et al., 2012). As a result, researchers have focused their efforts on developing effective post-trauma early interventions aimed at preventing the development of PTSD.
The development of such interventions is not only important for trauma-affected individuals, but also for high-risk organisations such as the emergency services. Indeed, organisations have duty of care requirements that they must uphold under workplace health and safety laws. Furthermore, given the occupational impairment associated with PTSD, it is important to attend to the needs of employees (and other individuals) exposed to traumatic events to facilitate their return to pre-trauma levels of functioning.

### 1.1 Early interventions following trauma exposure

The fifth edition of the Diagnostic and Statistical Manual of psychiatric disorders (DSM-5; APA, 2013) requires that PTSD symptoms are present for at least four weeks following trauma to meet diagnostic criteria. Thus, interventions provided within the first four weeks following exposure to trauma are considered ‘preventative’ (van Emmerik, Kamphuis, Hulsbosch & Emmelkamp, 2002). For many decades, psychological debriefing (PD) was the most thoroughly investigated and employed early intervention method following trauma exposure. Of the many variants of PD, Critical Incident Stress Debriefing (CISD; Mitchell, 1983) has been the most widely used and researched. This single-session intervention is administered shortly following trauma exposure (i.e., usually between 24 and 72 hours; Devilly, Gist & Cotton, 2006). The procedure consists of seven stages outlined in Table 1.1.
Table 1.1. *CISD stages as outlined by Everly & Mitchell (1995)*

<table>
<thead>
<tr>
<th>Phase</th>
<th>Objective</th>
</tr>
</thead>
<tbody>
<tr>
<td>Introduction</td>
<td>To explain the debriefing process to participants, establish ground rules for the debriefing session, and address confidentiality</td>
</tr>
<tr>
<td>Fact</td>
<td>Participants recount the factual details of the traumatic event</td>
</tr>
<tr>
<td>Thought</td>
<td>To discuss participants’ thoughts and attributions related to the event</td>
</tr>
<tr>
<td>Reaction</td>
<td>To discuss the emotional reactions participants experienced during the height of the event</td>
</tr>
<tr>
<td>Symptom</td>
<td>To identify and discuss the symptoms of stress that participants are currently experiencing</td>
</tr>
<tr>
<td>Teaching</td>
<td>Facilitators provide participants with information about normal stress reactions and strategies to effectively manage stress</td>
</tr>
<tr>
<td>Re-entry</td>
<td>Any remaining issues or questions are addressed with participants</td>
</tr>
</tbody>
</table>

*CISD was traditionally recommended to be delivered to groups of individuals (Everly & Mitchell, 1995). Indeed, the rationale for employing a group-based intervention is that this format is particularly useful in promoting catharsis. That is, emotional ventilation is encouraged because individuals feel safe and understood when in the company of others who have experienced similar trauma (Kenardy & Carr, 1996). Furthermore, a group format provides individuals with the opportunity to discuss effective coping strategies which can then be employed by others. While a number of advantages have been proposed for group-based CISD, the intervention has also been administered individually (e.g., Lee, Slade & Lygo, 1996; Marchand et al., 2006)*

*Indeed, CISD was routinely offered in a number of settings internationally from its conception in the 1980s. However, due to its widespread application, the intervention has*
been subject to many empirical evaluations of its effectiveness. This has resulted in an abundance of research indicating that while the majority of recipients describe the intervention as helpful (Devilly et al., 2006), CISD provides no real benefit for psychological health. Indeed, a number of systematic reviews have investigated the effectiveness of PD (e.g., Australian Centre for Posttraumatic Mental Health, 2013; National Institute for Clinical Excellence (NICE), 2005; Rose & Bisson, 1998; Rose, Bisson, Churchill & Wessely, 2002; Rose, Bisson & Wessely, 2003; van Emmerik et al., 2002). These have unanimously concluded that the effect of PD is equivocal to no intervention in reducing the risk of developing PTSD, and may even be detrimental to long-term adjustment.

The discovery of negative outcomes in particular has led to recommendations that the routine use of PD should be ceased with victims of trauma (e.g., NICE, 2005; Rose et al., 2002). Two studies have become influential in PD research due to such findings; that is, those of Bisson et al. (1997) and Mayou et al. (2000). Nevertheless, closer examination of these studies highlights significant issues. Firstly, debriefed participants in these studies were more severely injured than non-debriefed participants, and when injury severity and initial trauma symptoms were controlled for, the negative effect of debriefing on later trauma symptomatology was reduced to marginal significance ($p < 0.07$, Mayou et al., 2000) or disappeared (Bisson et al., 1997). Secondly, both studies reported that the ‘debriefing’ offered was markedly different from that described by Mitchell (1983) and were not standardised. Finally, and importantly, both studies only provided psychological debriefing on an individual basis.

Thus, while reviewers such as Rose et al. (2002) have concluded that “compulsory debriefing of victims of trauma should cease” (p. 2), they have recognised that their results may not apply to group debriefing. Indeed, the NICE Guidelines (2005) reported that no
group-based debriefing interventions following Mitchell’s (1983) model met their inclusion criteria, precluding generalisation of their results to this debriefing format. This prompted calls for more randomised trials of group debriefing, leading to a surge in studies utilising this debriefing format (e.g., Devilly & Annab, 2008). As a result, a review of the efficacy of group debriefing is warranted. The current paper will review the evidence for the efficacy of this intervention in preventing the development of psychological sequelae (i.e., posttraumatic stress and associated symptomatology).

2. Method

2.1 Criteria for considering studies for this review

Studies investigating the effectiveness of single-session group debriefing in preventing or ameliorating post-traumatic stress disorder and/or associated psychopathology were assessed. For the purpose of this review, posttraumatic or associated psychopathology referred to anxiety, mood, negative emotion, and trauma-like symptomatology. Due to the ethical and practical issues associated with conducting research following trauma exposure, and the resultant paucity of studies, both randomised controlled trials (RCTs) and studies involving non-randomised allocation of participants to groups were included in the current review. Furthermore, studies involving real or analogue to trauma were considered. Only peer-reviewed studies published in the English language were included. No publication date restrictions were imposed.

Inclusion criteria were that a group-based debriefing intervention had been provided within four weeks of exposure to a traumatic event (real or analogue) to participants aged 16 years or older. The rationale for this timeframe was based on the DSM-5 (APA, 2013) diagnostic criteria for PTSD which states that symptoms must persist for a minimum of four
weeks to meet caseness. Thus, interventions provided more than one month after trauma would classify as curative rather than preventive (van Emmerik et al., 2002), and were not included in the current review.

Debriefing procedures were required to contain key components of psychological debriefing as described by Mitchell (1983), although not necessarily adhering to this method. That is, interventions involving any combination of the following stages were included: discussion of the event, associated cognitions and/or emotional reactions, normalising stress reactions and/or discussion of future coping strategies. Studies were also required to have included a comparison/control group consisting of individuals who were exposed to the same trauma. These individuals could be allocated to a no debriefing condition or active control (e.g. usual care or any debriefing intervention which did not meet PD criteria in this study).

Studies were required to have assessed outcomes both before and after the intervention using valid and reliable measures to ensure baseline comparability between groups. As we were interested in the effects on group debriefing on post-traumatic stress symptomatology and associated psychopathology, studies that included valid and reliable measures assessing symptoms of PTSD and/or other psychopathology were included.

Eligibility assessment was completed in a standardized manner according to the preset criteria. Studies to be included in the review were assessed for eligibility by two authors at two stages (i.e., at title and abstract stage and full-text stage). Where disagreements occurred between reviewers, this was resolved by discussion and mutual agreement as to whether eligibility criteria were met. Inter-rater reliability was 99% at stage one and 96% at stage two.
2.2 Summary of search strategy

Studies were identified by searching the following electronic databases: PsycINFO (1806-February, 2014), Medline (1946- February, 2014), CINAHL (Cumulative Nursing and Allied Health Literature; 1982- February, 2014) and EMBASE (1947- February, 2014). Keywords were: “posttraumatic stress disorder” (exploded to include “combat experience”, “debriefing (psychological)”, “emotional trauma”, “stress reactions”, “trauma”, and “traumatic neurosis”) AND “debriefing OR critical incident stress debriefing OR crisis intervention OR intervention”.

Furthermore, English language, human and peer reviewed limits were applied. In addition, a manual search of articles published in the Journal of Traumatic Stress since 2010 was conducted. Reference lists of included articles were also scanned for additional relevant studies. Two authors were contacted for missing information regarding the size of debriefed groups. The last search was performed on 26 February, 2014.

2.3 Data collection and quality rating of studies

Papers were reviewed independently by the author to extract relevant points of data. Studies were evaluated based on sample size, target population, timing of the intervention since trauma, size of debriefing groups, duration of debriefing, experimental conditions, outcome measures used, study outcomes and length of follow-up (see Table 3.1).

Methodological quality of included studies was evaluated by two raters using three separate rating scales which have been used in previous reviews of psychological debriefing (e.g., Rose et al., 2002): the Cochrane Collaboration Quality Assessment Tool (as outlined in the Cochrane Collaboration Handbook; Higgins & Green, 2011); the CCDAN quality rating scale (Moncrieff, Churchill, Drummond & McGuire, 2001), and; a quality assessment
measure specifically developed for evaluating trials of psychological debriefing (Kenardy & Carr, 1996). The Cochrane Collaboration Quality Assessment Tool assesses risk of bias based on method of randomisation, allocation concealment, blinding of subjects and assessors, incomplete outcome data, and selective outcome reporting (Higgins & Green, 2011). CCDAN (Moncrieff et al., 2001) ratings are made on objectives of the trial, sample size, length of follow-up, power, randomisation, standardisation of treatment, blinding, source of participants, recruitment procedures, exclusion criteria, demographic descriptions, blinded assessments, reasons for withdrawal, outcome measures, intention-to-treat, presentation of results, statistical analysis and control for baseline differences. The scale has a maximum score of 46, with each criterion scored from 0-2. Finally, the Kenardy and Carr (1996) rating scale assesses studies based on delineation of goals of debriefing, randomisation, use of self-report and objective measures, debriefing procedures, the use of trained personnel conducting the debriefing, quality control measures, amount of exposure to PD, and use of blind raters. One score is allocated for each criterion satisfied, with a maximum possible score of 26. There were no discrepancies in quality ratings between raters, thus, these ratings informed the subsequent quality scores.

2.4 Reporting

The Preferred Reporting Items for Systematic reviews and Meta-Analyses (PRISMA) statement and guidelines were used to guide the summarising of evidence and ensure appropriate and transparent reporting of results (Liberati et al., 2009).

3. Results

3.1 Selection and exclusion
A flow diagram of the study selection process is provided in Figure 3.1. Nine studies were identified that met the eligibility criteria for the current review. Six of these were RCTs (i.e., Adler et al., 2008; Adler, Bliese, McGurk, Hoge, & Castro, 2009; Devilly & Annab, 2008; Devilly & Varker, 2008; Tuckey & Scott, 2014; Wu et al., 2011), two employed a self-selecting approach during group allocation (i.e., De Gaglia, 2006; Ruck, Bowes & Tehrani, 2013), and the remaining study assigned participants to groups based on individual availability and other commitments (Deahl et al., 2000). The earliest study was published in 2000 and the most recent study was published in 2014. Details of included studies are given in Table 3.1.

Studies were excluded if: interventions did not satisfy criteria for debriefing (e.g., expressive writing intervention); debriefing was given more than one month after exposure; the sample included individuals younger than 16 years; debriefing was not conducted in groups; articles were dissertation abstracts, reviews, or non-empirical studies; adequate control/comparison groups were not included; baseline data was absent or not obtained; or the paper was not available in the English language. Details of studies examined and excluded after full text review are briefly listed in Table 3.2.

3.2 Intervention and control condition characteristics

All studies involved a single, group-debriefing session. These ranged from 40 to 180 minutes each, with an average length of 83.1 minutes overall. Five studies specifically used CISD as their debriefing intervention (Adler et al., 2008; Devilly & Annab, 2008; Devilly & Varker, 2008; Tuckey & Scott, 2014; Wu et al., 2011). The remaining studies used interventions that were based on the principles of PD as outlined by Mitchell (1983), but slightly adapted for use in particular contexts.
**Literature search**

Databases: PsycINFO, Medline, CINAHL, EMBASE
Journals: Journal of Traumatic Stress

Search results combined \((n = 3314)\)

Duplicates removed \((n = 568)\)

Articles screened on basis of title and abstract \((n = 2746)\)

Included for full text review \((n = 25)\)

Excluded \((n = 2721)\)

- Irrelevant area (e.g., medical, not PTSD; \(n = 1065\))
- Child/adolescent focus \((n = 670)\)
- Review/commentary/editorial \((n = 487)\)
- Not PD \((n = 249)\)
- Examined prevalence of PTSD \((n = 190)\)
- Examined predictors of PTSD \((n = 36)\)
- Qualitative study \((n = 17)\)
- Individual debriefing \((n = 5)\)
- Debriefing > 1 month post trauma \((n = 1)\)
- Not English \((n = 1)\)

Included for full text review \((n = 25)\)

Studies retained \((n = 8)\)

Studies reviewed following manual search \((n = 1)\)

Excluded \((n = 17)\)

- No baseline data obtained \((n = 6)\)
- Individual debriefing \((n = 4)\)
- No control/comparison group \((n = 4)\)
- Not empirical \((n = 2)\)
- Control group from different population than PD group \((n = 1)\)
Studies retained ($n = 9$)

*Figure 3.1. Study selection process*
<table>
<thead>
<tr>
<th>Authors (year)</th>
<th>n</th>
<th>Target population</th>
<th>Time following trauma</th>
<th>Conditions</th>
<th>Group size</th>
<th>Duration of PD (mins)</th>
<th>Measures</th>
<th>Outcome</th>
<th>Follow up period</th>
</tr>
</thead>
<tbody>
<tr>
<td>Adler et al. (2008)</td>
<td>1,004</td>
<td>U.S. soldiers</td>
<td>Within 4 weeks</td>
<td>1. CISD 2. Stress management class (SMC) 3. Survey only</td>
<td>1. M=14.9 2. M=14.9 3. M=14.9</td>
<td>47.5-148</td>
<td>PCL, CES-D, AUDIT, CTS</td>
<td>CISD group with greater trauma exposure reported lower PTSD symptoms and aggression than SMC. CISD group reported increased alcohol use compared to SMC.</td>
<td>3-4 mths 8-9 mths</td>
</tr>
<tr>
<td>Adler et al. (2009)</td>
<td>2,297</td>
<td>U.S. soldiers</td>
<td>Within 14 days</td>
<td>1. Battlemind debriefing§ 2. Small Battlemind training 3. Large Battlemind training 4. Stress education (SE)</td>
<td>1. 20-32 2. 18-45 3. 126-225 4. 51-257</td>
<td>50</td>
<td>PCL, PHQ-D</td>
<td>PD group with high levels of combat exposure had fewer PTSD and depression symptoms than SE. No difference between PD and training.</td>
<td>4 mths</td>
</tr>
<tr>
<td>Deahl et al. (2000)</td>
<td>106</td>
<td>British soldiers</td>
<td>Immediate (upon return from duty)</td>
<td>1. Debriefed§ 2. Non-debriefed</td>
<td>1. 8-10 2. NA</td>
<td>120</td>
<td>HADS, IES, PTSS-10, SCL-90, CAGE</td>
<td>Non-debriefed group reported significantly higher CAGE scores compared to debriefed group at 12 month follow-up.</td>
<td>3 mths 6 mths 12 mths</td>
</tr>
<tr>
<td>De Gaglia (2006)</td>
<td>436</td>
<td>Fire/rescue workers</td>
<td>Within 24 hrs</td>
<td>1. Defusing§ 2. No debriefing</td>
<td>1. M=8.47 2. NA</td>
<td>60</td>
<td>MAACL-R</td>
<td>PD led to decreased negative affect compared to no debriefing</td>
<td>Nil</td>
</tr>
<tr>
<td>Devilly &amp; Annab (2008)</td>
<td>64</td>
<td>University students</td>
<td>Immediate</td>
<td>1. CISD 2. No debriefing</td>
<td>1. 6-8 2. 6-8</td>
<td>40-50</td>
<td>PDS, DASS-21</td>
<td>No significant difference between groups.</td>
<td>4 wks</td>
</tr>
</tbody>
</table>

1-13 Posttraumatic Stress Disorder Checklist; Center for Epidemiological Studies – Depression Scale; Alcohol Users Disorder Identification Test; Conflict Tactics Scale; Patient Health Questionnaire for Depression; Hospital Anxiety and Depression Scale; Impact of Event Scale; Posttraumatic Symptom Scale; Symptom Checklist 90 Revised; CAGE Questionnaire; Multiple Affect Adjective Checklist – Revised; Posttraumatic Stress Diagnostic Scale; Depression, Anxiety, Stress Scale-21; PTSD Symptom Scale-Self-Report; Impact of Event Scale-Extended; Generalised Anxiety and Depression Scale; Impact of Event Scale – Revised; Kessler Psychological Distress Scale; Structured Interview for PTSD.

§ = based on Mitchell’s (1989) CISD model †= Critical Incident Stress Management NR = Not Reported NA = Not Applicable
Table 3.1
*Characteristics of retained studies (cont.)*

<table>
<thead>
<tr>
<th>Authors (year)</th>
<th>n</th>
<th>Target population</th>
<th>Time following trauma</th>
<th>Conditions</th>
<th>Group size</th>
<th>Duration of PD (mins)</th>
<th>Measures</th>
<th>Outcome</th>
<th>Follow up period</th>
</tr>
</thead>
</table>
| Devilly & Varker (2008) | 119 | University students   | Immediate             | 1. CISD  
2. No debriefing                   | 1. 5-11     | 40-50                  | PSS-SR<sup>14</sup>  
DASS-21 | Debriefing + more distressing video led to greater distress immediately afterwards than debriefing + less distressing video. No significant difference at follow-up. | 4 weeks     |
| Ruck, Bowes & Tehrani (2013) | 91  | Prison staff          | Within 4 weeks        | 1. Debriefed§  
2. Not debriefed                   | 1. 4-10     | 180                   | ISE<sup>15</sup> GAD<sup>16</sup> | Debriefed participants reported a significant reduction in PTSD symptomatology over time, however, no significant change for controls. | 1 month       |
| Tuckey & Scott (2014) | 67  | Volunteer firefighters | Within 3 days         | 1. CISD  
2. Stress Management Education (SME)  
3. Screening | 1. 6-20     | 90                   | IES-R<sup>17</sup> K10<sup>18</sup> | No significant difference between groups on IES-R or K10. | Nil.         |
| Wu et al. (2011) | 1,267 | Chinese soldiers      | 20-33 days            | 1. CISD  
2. 512 PIM debriefing  
3. No debriefing                   | 1. NR       | 60                   | SI-PTSD<sup>19</sup> HADS | No significant difference between CISD and control. 512 PIM fewer PTSD, anxiety and depression symptoms than CISD and no debriefing groups (2-, 4-months) | 1 month 2 months 4 months |

<sup>1-19</sup> Posttraumatic Stress Disorder Checklist; Center for Epidemiological Studies – Depression Scale; Alcohol Users Disorder Identification Test; Conflict Tactics Scale; Patient Health Questionnaire for Depression; Hospital Anxiety and Depression Scale; Impact of Event Scale; Posttraumatic Symptom Scale; Symptom Checklist 90 Revised; CAGE Questionnaire; Multiple Affect Adjective Checklist – Revised; Posttraumatic Stress Diagnostic Scale; Depression, Anxiety, Stress Scale-21; PTSD Symptom Scale-Self-Report; Impact of Event Scale-Extended; Generalised Anxiety and Depression Scale; Impact of Event Scale – Revised; Kessler Psychological Distress Scale; Structured Interview for PTSD.

§ = based on Mitchell’s (1989) CISD model  
†= Critical Incident Stress Management  
NR = Not Reported  
NA = Not Applicable
Table 3.2  
Studies not eligible for inclusion

<table>
<thead>
<tr>
<th>Study</th>
<th>Year</th>
<th>Reason for exclusion</th>
</tr>
</thead>
<tbody>
<tr>
<td>Armstrong et al.</td>
<td>1998</td>
<td>Non-empirical study</td>
</tr>
<tr>
<td>Campfield &amp; Hills</td>
<td>2001</td>
<td>No comparison/control group</td>
</tr>
<tr>
<td>Conlon, Fahy &amp; Conroy</td>
<td>1999</td>
<td>Individual debriefing</td>
</tr>
<tr>
<td>Eid, Johnsen &amp; Weisaeth</td>
<td>2001</td>
<td>No baseline data obtained</td>
</tr>
<tr>
<td>Hobbs, Mayou &amp; Harrison &amp; Worlock</td>
<td>1996</td>
<td>Individual debriefing</td>
</tr>
<tr>
<td>Humphries &amp; Carr</td>
<td>2001</td>
<td>Control group from different population/trauma</td>
</tr>
<tr>
<td>Kenardy, Webster, Lewin, Carr, Hazell &amp; Carter</td>
<td>1996</td>
<td>No baseline data obtained</td>
</tr>
<tr>
<td>Lee, Slade &amp; Lygo</td>
<td>1996</td>
<td>Individual debriefing</td>
</tr>
<tr>
<td>Leonard &amp; Alison</td>
<td>1999</td>
<td>No baseline data obtained</td>
</tr>
<tr>
<td>Matthews</td>
<td>1998</td>
<td>No baseline data obtained</td>
</tr>
<tr>
<td>Mayou, Ehlers &amp; Hobbs</td>
<td>2000</td>
<td>Individual debriefing</td>
</tr>
<tr>
<td>Nurmi</td>
<td>1999</td>
<td>No baseline data obtained</td>
</tr>
<tr>
<td>Regehr &amp; Hill</td>
<td>2000</td>
<td>Non-empirical study</td>
</tr>
<tr>
<td>Richards</td>
<td>2001</td>
<td>No adequate comparison/control group</td>
</tr>
<tr>
<td>Shalev, Peri, Rogel-Fuchs, Ursano &amp; Marlowe</td>
<td>1998</td>
<td>No comparison/control group</td>
</tr>
<tr>
<td>True</td>
<td>2000</td>
<td>No comparison/control group</td>
</tr>
<tr>
<td>Wee, Mills &amp; Koehler</td>
<td>1999</td>
<td>No baseline data obtained</td>
</tr>
</tbody>
</table>

Six studies included a no debriefing control condition (Deahl et al., 2000; De Gaglia, 2006; Devilly & Annab, 2008; Devilly & Varker, 2008; Ruck et al., 2013; Wu et al., 2011). Two studies included a no intervention comparison group who completed screening surveys (Adler et al., 2008; Tuckey & Scott, 2014). Four studies included an active intervention condition. Of these, three included stress management/education classes (Adler et al., 2008; Adler et al., 2009; Tuckey & Scott, 2014) in which information was provided about how to identify and manage stress, general self-care and coping strategies and treatment options if
needed. One study included a small (18-45 participants per group) and large (126-225 participants per group) Battlemind Training intervention which involved reviewing and adapting combat skills to civilian life to facilitate transition to home (i.e., Adler et al., 2009). Finally, one study included an alternative PD model, called 512PIM debriefing which consisted of CISD plus a key cohesion training stage in which participants played games to develop team cooperation (i.e., Wu et al., 2011).

### 3.3 Sample characteristics

Sample sizes for the included studies ranged from 64 to 2,297. Studies recruiting from military populations had greater sample sizes (range 106-2,297; Adler et al., 2008; Adler et al., 2009; Deahl et al., 2000; Wu et al., 2011) than non-military samples (range = 64-436; De Gaglia, 2006; Devilly & Annab, 2008; Devilly & Varker, 2008; Ruck et al., 2013; Tuckey & Scott, 2014). The number of participants per debriefing group ranged from 4 to 32. One study did not report the size of debriefing groups (Wu et al., 2011).

Six studies involved an excess of males, reflecting the gender distribution in military and fire/rescue populations (Adler et al., 2008; Adler et al., 2009; Deahl et al., 2000; De Gaglia, 2006; Tuckey & Scott, 2014; Wu et al., 2011). One study involved a predominance of females, reflecting the gender distribution of first year psychology students (Devilly & Annab, 2008). One study had a comparable male/female distribution (Devilly & Varker, 2008). The remaining study did not provide information regarding gender split of the sample (Ruck et al., 2013). Three studies were based in Australia (Devilly & Annab, 2008; Devilly & Varker, 2008; Tuckey & Scott, 2014), two in the US (Adler et al., 2009; De Gaglia, 2006), two in the UK (Deahl et al., 2000; Ruck et al., 2013), one in Kosovo with follow-up in Germany (Adler et al., 2008), and one in China (Wu et al., 2011).
3.4 Nature of the trauma

The type of traumatic events that participants were exposed to across the studies was diverse. Two studies used analogue trauma stimuli (i.e., a distressing video) to evaluate the effects of debriefing (Devilly & Annab, 2008; Devilly & Varker, 2008). Three studies involved military personnel returning from active duty (Adler et al., 2008) or peacekeeping missions (Adler et al., 2009; Deahl et al., 2000). Two studies recruited fire-fighters/rescue personnel exposed to work-related traumatic experiences (De Gaglia, 2006; Tuckey & Scott, 2014). One study involved prison service workers exposed to work-related trauma (Ruck et al., 2013). The remaining study recruited soldiers involved in a rescue effort following natural disaster (Wu et al., 2011).

3.5 Timing of intervention

Timing of the debriefing intervention varied across studies. Two studies provided debriefing immediately following exposure to a distressing video and completion of outcome measures (Devilly & Annab, 2008; Devilly & Varker, 2008). In studies within organisations, debriefing was given within 24 hours (De Gaglia, 2006), three days of exposure (Tuckey & Scott, 2014), and four weeks (Ruck et al., 2013). Generally, longer time intervals were involved in military-based studies, reflecting the logistical difficulty associated with these populations. Nevertheless, Deahl et al., (2000) provided debriefing immediately upon return from deployment. One study (Wu et al., 2011) provided debriefing 20-33 days following trauma exposure, with a median of 25 days. The exact time period for Adler et al. (2008) was unclear, but it is known to be up to one month following trauma. The final study (Adler et al., 2009) provided the intervention within 14 days of soldiers returning from duty.
3.6 Outcome measures

A variety of symptom outcome measures were used across studies. Nevertheless, all were standardised and had good psychometric properties. Seven studies used self-report measures assessing post-traumatic stress symptomatology (Adler et al., 2008; Adler et al., 2009; Deahl et al., 2000; Devilly & Annab, 2008; Devilly & Varker, 2008; Ruck et al., 2013; Tuckey & Scott, 2014; Wu et al., 2011). These included the Posttraumatic Stress Disorder Checklist (PCL; Weathers, Litz, Herman, Huska, & Keane, 1993), Impact of Events Scale (IES; Horowitz et al., 1979); Posttraumatic Symptom Scale (PTSS-10; Holen, Sund, & Weisaeth, 1989), Post-traumatic Stress Diagnostic Scale (PDS; Foa, 1995); the PTSD Symptom Scale-Self-Report (PSS-SR; Foa, Riggs, Dancu, & Rothbaum, 1993), the Impact of Event Scale-Extended (IES-E; Tehrani, Cox, & Cox, 2002), and the Impact of Event Scale-Revised (IES-R; Weiss, 2004). Wu et al. (2011) measured PTSD symptomatology using the Structured Interview for PTSD (SI-PTSD; Davidson, Smith & Kudler, 1989). One study (De Gaglia, 2006) did not examine post-traumatic symptomatology, but used a state version of the Multiple Affect Adjective Checklist – Revised (MAACL-R; Zuckerman, Lubin & Rinck, 1983) as the main outcome measure to investigate negative affect pre- and post-debriefing.

In addition to PTSD symptoms, five studies (i.e., Deahl et al., 2000; Devilly & Annab, 2008; Devilly & Varker, 2008; Ruck et al., 2013; Wu et al., 2011) assessed symptoms of anxiety and depression using a variety of self-report outcome measures including the Depression, Anxiety, Stress Scales-21 (DASS-21; Lovibond & Lovibond, 1995), Generalised Anxiety and Depression Scale (GAD; Goldberg, 1971), and the Hospital Anxiety and Depression Scale (HADS; Zigmond & Snaith, 1983). Two studies measured depression symptoms only (Adler et al., 2008; Adler et al., 2009) using the Center for Epidemiological Studies – Depression Scale (Radloff, 1977) and Patient Health Questionnaire for Depression
(Spitzer, Kroenke, Williams, & Patient Health Questionnaire Primary Case Study Group, 1999). Two studies (Adler et al., 2008; Deahl et al., 2000) measured alcohol use using the Alcohol Users Disorder Identification Test (Babor, Higgins-Biddle, Saunders, & Montiero, 2001) and CAGE questionnaire (Ewing, 1984). One study (Tuckey & Scott, 2014) measured global psychosocial distress using the Kessler-10 (K10; Andrews & Slade, 2001). Finally, Adler et al. (2008) used the Conflict Tactics Scale (Straus, 1979) to measure aggressive behaviour.

3.7 Follow-up and attrition rates

Follow-up periods were variable across studies, ranging from four weeks to 12 months. Two studies only assessed outcomes post-intervention did not include a follow-up assessment (i.e., De Gaglia, 2006; Tuckey & Scott, 2014). Attrition rates varied, ranging from complete retention (due to no follow-up; De Gaglia, 2006) to substantial attrition. Tuckey and Scott (2014) only measured the effects of debriefing one month post-intervention and found that 30% of participants did not return completed post-intervention measures.

Three studies reported relatively low attrition rates. Of these, two revealed that only one participant did not complete follow-up measures (Devilly & Annab, 2008; Devilly & Varker, 2008). Wu et al. (2011) reported that 1.6% of participants did not receive the intervention, 3.2% of participants were lost at one month follow-up, 6.6% at two months and 10.8% at four months. These were equally distributed across groups.

Four studies experienced significant attrition rates. Adler et al. (2009) reported that 53.9% of participants did not complete follow-up measures in their study. Further, Adler et al. (2008) experienced an attrition rate of 36% at the first follow-up (3-4 months following re-deployment) and 71% at the second follow-up (8-9 months post re-deployment). Deahl et al.
(2000) reported that 66% of the original sample of 106 soldiers completed follow-up at 6 months and 52% completed follow-up at 12 months. These were equally distributed across groups. Further, Ruck et al. (2013) reported that only 41% of the original sample completed one month follow-up measures.

3.8 Criteria for inclusion/exclusion

Only two studies outlined specific inclusion/exclusion criteria. Wu et al. (2011) required that participants met criterion A1 of the DSM-IV criteria for PTSD, were proficient in Chinese and aged 18 years or older. Participants were excluded if they had suicidal ideation, were receiving psychological intervention since the trauma, or if they met DSM-IV criteria for a psychotic disorder, organic disorder, substance abuse, or chronic PTSD. Adler et al. (2008) included soldiers returning from deployment who were at least 18 years old. No exclusion criteria were applied.

3.9 Main findings of studies

Outcome results of studies are presented in Table 3.1.

3.9.1 Debriefing versus no intervention

Seven studies investigated the impact of group debriefing on PTSD symptoms compared to a control condition. Six of these found no impact of group debriefing (Adler et al., 2008; Deahl et al., 2000; Devilly & Annab, 2008; Devilly & Varker, 2008; Tuckey & Scott, 2014; Wu et al., 2011). One study found that debriefing led to a significant reduction in PTSD symptomatology, whereas no significant change was observed for the control group over time (Ruck et al., 2013).
Seven studies investigated the impact of group debriefing on symptoms of anxiety and depression. Five of these found no significant difference between debriefed and non-debriefed participants (Adler et al., 2008; Devilly & Annab, 2008; Devilly & Varker, 2008; Ruck et al., 2013; Wu et al., 2011), while two studies found a positive effect of group debriefing. Specifically, De Gaglia (2006) found that debriefed individuals reported significantly lower levels of negative affect than non-debriefed individuals. Second, Deahl et al. (2000) found that debriefed participants reported fewer symptoms of anxiety and depression at six-month follow-up compared to non-debriefed participants.

Two studies which compared group debriefing to a no intervention control condition examined alcohol misuse. Of these, one obtained favourable outcomes for PD, in that debriefed participants reported significantly lower rates of alcohol consumption than controls (Deahl et al., 2000), while the other found that alcohol use increased significantly over time for participants who received CISD compared to no intervention (Adler et al., 2008).

3.9.2 Debriefing versus alternative intervention

Four studies investigated the impact of group debriefing on PTSD symptomatology compared to an alternative intervention. Three studies compared group debriefing to a stress management class. Of those, two found that group debriefing was more effective in reducing symptoms for those with higher degrees of trauma exposure (Adler et al., 2008; Adler et al., 2009) and the final study found no difference (Tuckey & Scott, 2014). One study compared group-based CISD to 512 PIM (i.e., a group-based intervention comprised of CISD plus a cohesion training stage) and found that at two and four month follow up, 512 PIM was more effective in reducing PTSD symptomatology than CISD alone (Wu et al., 2011).
Four studies investigated the impact of group debriefing on associated psychopathology compared to an alternative intervention. One of these investigated alcohol use and compared group debriefing to a stress management class, finding that group debriefing led to increased alcohol use over time (Adler et al., 2008). Two studies investigated the impact of group PD on symptoms of depression compared to a stress management class. One study found that PD was more effective for those with high levels of combat exposure in particular (Adler et al., 2009), while the second study found no difference (Adler et al., 2008). Finally, one study compared group-based CISD to 512 PIM when investigating the effect of PD on symptoms of anxiety and depression, with 512 PIM found to be more effective in reducing symptoms than CISD (Wu et al., 2011).

3.10 Methodological Quality of the Studies

Table 3.3 provides methodological quality ratings for each study using the scales developed by Moncrieff et al. (2001; i.e., CCDAN) and Kenardy and Carr (1996). Scores ranged from 14 to 29 out of 46 on the CCDAN scale. Ratings on the Kenardy and Carr (1996) scale ranged from 9 to 17 out of a maximum of 26.

The differences between the CCDAN (Moncrieff et al., 2001) and Kenardy and Carr (1996) scale ratings are due to a number of factors. First, the CCDAN emphasizes general methodological issues relevant to clinical trials, particularly pharmacological trials (e.g., compliance with treatment, reporting of side effects). In contrast, the Kenardy and Carr (1996) scale gives more weight to debriefing-related methodological issues (e.g., defining the nature of trauma, outlining goals of debriefing). Since it was specifically designed to evaluate trials of debriefing, it was decided that the Kenardy and Carr (1996) ratings would be used for the final analysis.
Table 3.3  
*Summary of methodological quality ratings*

<table>
<thead>
<tr>
<th>Authors (year)</th>
<th>CCDAN Scale (max score 46)</th>
<th>Kenardy &amp; Carr (1996) Scale (max score 26)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Adler et al., (2008)</td>
<td>28</td>
<td>15</td>
</tr>
<tr>
<td>Adler et al., (2009)</td>
<td>29</td>
<td>13</td>
</tr>
<tr>
<td>Deahl et al., (2000)</td>
<td>22</td>
<td>15</td>
</tr>
<tr>
<td>De Gaglia (2006)</td>
<td>17</td>
<td>9</td>
</tr>
<tr>
<td>Devilly &amp; Annab (2008)</td>
<td>17</td>
<td>13</td>
</tr>
<tr>
<td>Devilly &amp; Varker (2008)</td>
<td>20</td>
<td>12</td>
</tr>
<tr>
<td>Ruck, Bowes &amp; Tehrani (2013)</td>
<td>14</td>
<td>10</td>
</tr>
<tr>
<td>Tuckey &amp; Scott (2014)</td>
<td>20</td>
<td>10</td>
</tr>
<tr>
<td>Wu et al., (2011)</td>
<td>28</td>
<td>17</td>
</tr>
</tbody>
</table>

Overall, methodological quality of the included studies was relatively uniform, at an intermediate level. Trials involving military samples performed best, with Wu et al.’s (2011) RCT demonstrating the highest methodological quality of all the included studies. Most studies gave adequate information regarding the source of the sample, thorough descriptions of debriefing procedures, and used reliable and valid self-report outcome measures. Common methodological shortcomings related to a lack of objective measures being used across studies, inadequate randomisation, lack of allocation concealment, a lack of information obtained regarding pre-morbid vulnerability factors, and not reporting specific trauma information.

Using the Cochrane Collaboration Quality Assessment Tool, results indicated a high risk of bias for studies across many domains (see Table 3.4). Randomisation and allocation concealment was a key issue for all studies. Indeed, despite reporting that they had randomised participants, many studies did not specify how this was achieved (e.g., Devilly &
Annab, 2008), or used a cluster randomisation approach (e.g., Adler et al., 2009). Thus, high or unclear risk for selection bias was observed in the included studies. Indeed, one study found that intervention groups were not entirely equivalent after cluster randomisation (Adler et al., 2009). These randomisation issues reflect the ethical and logistical difficulties associated with conducting research with victims of trauma.

Blinding of participants and external raters was another consistent shortfall across studies, indicating a high risk for performance and detection bias in all studies. Again, this is a common issue associated with conducting research in this area. For example, Wu et al. (2011) were able to blind assessors, but not participants. As a result, the potential for assessors to become aware of group allocation was possible. Other sources of bias related to attrition rates and integrity of interventions. Indeed, four studies (Adler et al., 2008; Adler et al., 2009; Deahl et al., 2000; Ruck et al., 2013) demonstrated high risk for attrition bias due to a significant proportion of participants not completing follow-up measures. Furthermore, four studies did not attempt to assess intervention integrity.
# Table 3.4

**Risk of bias**

<table>
<thead>
<tr>
<th>Type of bias</th>
<th>Selection</th>
<th>Performance</th>
<th>Detection</th>
<th>Attrition</th>
<th>Reporting</th>
<th>Other</th>
</tr>
</thead>
<tbody>
<tr>
<td>Random sequence generation</td>
<td>?</td>
<td>-</td>
<td>-</td>
<td>NA</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Allocation concealment</td>
<td>?</td>
<td>-</td>
<td>-</td>
<td>NA</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Blinding of participants/personnel</td>
<td>-</td>
<td>?</td>
<td>-</td>
<td>?</td>
<td>+</td>
<td>+</td>
</tr>
<tr>
<td>Blinding of outcome Ax (patient report)</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>?</td>
<td>+</td>
<td>+</td>
</tr>
<tr>
<td>Blinding of outcome Ax (externally rate-t)</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>?</td>
<td>+</td>
<td>+</td>
</tr>
<tr>
<td>Incomplete outcome data (post)</td>
<td>-</td>
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<tr>
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<tr>
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<tr>
<td>Management of confounders</td>
<td>-</td>
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<td>+</td>
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<td>-</td>
<td>?</td>
<td>+</td>
<td>+</td>
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<td>-</td>
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<tr>
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<td>-</td>
<td>-</td>
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<tr>
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<td>-</td>
<td>-</td>
<td>-</td>
<td>?</td>
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</table>

*+ = low risk of bias; - = high risk of bias; ? = unclear risk of bias; NA = not applicable*
### Table 3.4

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#### Risk of bias (cont.)

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<td>?</td>
<td>+</td>
<td>-</td>
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<td>+</td>
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<tr>
<td>Ruck et al. (2013)</td>
<td>-</td>
<td>-</td>
<td>-</td>
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<td>Wu et al. (2011)</td>
<td>?</td>
<td>-</td>
<td>-</td>
<td>-</td>
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<td>+</td>
</tr>
</tbody>
</table>

+= low risk of bias; - = high risk of bias; ? = unclear risk of bias; NA = not applicable
4. Discussion

While previous reviews have concluded that single-session individual debriefing is ineffective and may even be harmful (e.g., Rose et al., 2002); the efficacy of group debriefing has been largely unknown. This review of group-based PD largely replicates the individual PD literature. That is, there is little evidence of benefit for group debriefing on PTSD symptoms. Indeed, only one out of nine studies found a benefit. Interestingly, the results of two studies suggest that group PD may be more beneficial for individuals exposed to higher levels of trauma; however, given that both studies experienced significant attrition rates, further studies are required to replicate these results before confident inferences can be made. With respect to associated psychopathology, there was also relatively equivocal evidence for benefits on depression and anxiety. Finally, while no study showed a negative impact of group debriefing on symptoms of PTSD, anxiety and depression, one out of two studies did find increased alcohol use.

When compared to alternative interventions, the current review indicates that stress education classes, in which symptoms of stress and adaptive self-care/coping strategies are reviewed, appear to no more efficacious than group debriefing. However, given that only three studies included such interventions, future systematic reviews should investigate the efficacy of this intervention more specifically. Interestingly, one study found that an alternative group intervention (i.e., 512 PIM), which contained all of the CISD stages as well as a cohesion training stage, led to a greater reduction in PTSD, anxiety and depression symptoms in a sample of Chinese soldiers compared to CISD alone (Wu et al., 2011). The researchers attributed the effectiveness of the alternative inter
vention to the additional cohesion training element which was proposed to have enhanced participants’ sense of belonging and perceived social support. While this finding is promising, further studies are required to replicate these results before confident inferences can be made about the effectiveness of this alternative intervention.

A limitation of the current review is that our exclusion criteria resulted in only a small number of studies being included. Nevertheless, this was necessary to prevent the inclusion of studies with a higher risk of bias than that observed in the included studies. That is, while it would have been possible to widen the inclusion criteria to allow studies without baseline data, for example, this would have raised concerns regarding comparability between groups. Thus, this exclusion criterion was considered to be a strength of the current review. Another important limitation relates to the generalisability of the current findings. That is, given that we only included studies with participants over the age of 16, the current findings do not extend to the use of group PD with children and adolescents under the age of 16. Therefore, this is an area which requires further exploration. Furthermore, whilst the current review did not include a meta-analysis of the data of included trials, future reviews of the area would benefit from such an undertaking. Finally, our review only included published articles, which may have biased the results obtained.

The scarcity of high quality randomised controlled trials is an issue which is difficult to overcome when investigating sensitive issues such as trauma. Indeed, the identified studies had a number of methodological limitations including blinding of participants and personnel, randomisation, allocation concealment, and substantial attrition rates in some studies. The significant attrition rates observed in three studies in particular raises concerns about the
reliability and validity of the findings. While these issues largely reflect the logistical and ethical challenges involved in research with trauma-exposed individuals, future studies should attempt to reduce these potential biases in the future. In summary, the majority of studies in the current review indicate that when compared to natural recovery over time, group debriefing results in equivocal outcomes (with respect to PTSD symptomatology and associated psychopathology). As a result, group debriefing interventions based on Mitchell’s (1983) CISD model cannot be recommended for use with victims of trauma on the basis of the available evidence. While no adverse effects of group debriefing on PTSD and trauma-related symptomatology were identified in the included studies (with the exception of increased alcohol in one study), the reason for the absence of a positive effect is worthy of future examination. Indeed, to inform the development of more efficacious and evidence-based early interventions for PTSD in future, it is important that both helpful and potentially unhelpful features of currently-used procedures are identified.
References


debriefing for victims of an armed robbery. Brief treatment and crisis intervention, 6(2), 122.


**Search strategy**

PsycINFO (1806-May 2012)

1. Posttraumatic stress disorder/ or combat experience/ or "debriefing (psychological)"/ or emotional trauma/ or stress reactions/ or trauma/ or traumatic neurosis/
2. (Debriefing or critical incident stress debriefing or intervention).mp. [mp=title, abstract, heading word, table of contents, key concepts, original title, tests & measures]
3. 1 and 2

The following databases were searched using a modified version of the above strategy:

- PubMed (1950-May 2012)
- CINAHL (Cumulative Nursing and Allied Health Literature; 1982-May 2012)
- EMBASE (1947- February, 2014)
APPENDIX B

ETHICS DOCUMENTATION
Ref: MFHW
6 December 2011

Professor Louise Sharpe
Clinical Research Director
School of Psychology
University of Sydney
Louise.sharpe@sydney.edu.au

Dear Professor Sharpe

Thank you for your correspondence dated 1 December 2011 addressing comments made to you by the Human Research Ethics Committee (HREC).

I am pleased to inform you that with the matters now addressed your protocol entitled “The effect of post-incident discussion on psychological well being and memory integrity” has been approved.

Details of the approval are as follows:

<table>
<thead>
<tr>
<th>Protocol No.</th>
<th>Approval Date</th>
<th>First Annual Report Due</th>
</tr>
</thead>
<tbody>
<tr>
<td>14355</td>
<td>6 December 2011</td>
<td>31 December 2012</td>
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Authorised Personnel:
Professor Louise Sharpe
Dr Helen Paterson
Leila Bukalo

Documents Approved:

<table>
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<tr>
<th>Document</th>
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<tbody>
<tr>
<td>Sona Advertisement</td>
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<td>Debriefing Form</td>
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</table>

HREC approval is valid for four (4) years from the approval date stated in this letter and is granted pending the following conditions being met:

Condition/s of Approval

Manager Human Ethics
Dr Margaret Faedo
T: +61 2 8627 8170
E: margaret.faedo@sydney.edu.au

Human Ethics Secretariat:
Ms Karen Greer
T: +61 2 8627 8171
E: karen.greer@sydney.edu.au
Ms Patricia Engelmann
T: +61 2 8627 8172
E: patricia.engelmann@sydney.edu.au
Ms Kala Rethnam
T: +61 2 8627 8173
E: kala.rethnam@sydney.edu.au
• Continuing compliance with the National Statement on Ethical Conduct in Research Involving Humans.

• Provision of an annual report on this research to the Human Research Ethics Committee from the approval date and at the completion of the study. Failure to submit reports will result in withdrawal of ethics approval for the project.

• All serious and unexpected adverse events should be reported to the HREC within 72 hours.

• All unforeseen events that might affect continued ethical acceptability of the project should be reported to the HREC as soon as possible.

• Any changes to the protocol including changes to research personnel must be approved by the HREC by submitting a Modification Form before the research project can proceed.

Chief Investigator / Supervisor’s responsibilities:
1. You must retain copies of all signed Consent Forms and provide these to the HREC on request.

2. It is your responsibility to provide a copy of this letter to any internal/external granting agencies if requested.

Please do not hesitate to contact Research Integrity (Human Ethics) should you require further information or clarification.

Yours sincerely

[Signature]

Dr Margaret Faedo
Manager, Human Ethics
On behalf of the HREC

Cc Leila Bukalo lbuk6751@uni.sydney.edu.au

This HREC is constituted and operates in accordance with the National Health and Medical Research Council’s (NHMRC) National Statement on Ethical Conduct in Human Research (2007), NHMRC and Universities Australia Australian Code for the Responsible Conduct of Research (2007) and the CPMP/ICH Note for Guidance on Good Clinical Practice.
APPENDIX C

QUESTIONNAIRE AND EXPERIMENT PROTOCOLS
C.1 Video Commentary

Version A

You are about to view part of an autopsy of an unidentified deceased male. The body was found in a car park on the 25th January, 2005. Trauma to the head and neck was apparent, and two gunshot wounds were found: one in the chest, and one in the right thigh. The body is believed to be linked to a serial killer that has presumably been at large for the last three years. All supposed victims, thirteen in total, have been found within a one kilometre radius, with two found at this location.

We have now removed the scalp, and to orientate you, here is the face, some trauma on the right hand side above the ear, and moving down to the hairline of, as we can see, the elderly victim. We will now remove the skull cap, and we can see that a saw cut has been made around the skull, but it may still be difficult to remove it as there may still be adhesions between the bone and the surface of the brain.

Here we can see a protective layer over the brain, which is quite thick and tough, and this front portion here has been ripped, probably due to the nature of the trauma to the head, and we can now see that there is actually some bruising in the frontal area of the brain due to what can now be assumed to be quite significant head trauma.

Here we can see the protective layer over the brain being removed. Given that this layer is quite tough, a great deal of force must have been dealt to actually tear them within the skull. The perpetrator may have been trying to knock the victim unconscious, or there may have been a struggle. As no heavy weapons were found at the scene, this head trauma may have even been sustained via forceful bodily violence on the part of the serial killer.

That layer has now been removed and we can now see the entire surface of the brain. Here we have the frontal lobe, the temporal, and occipital lobes. And here is the dividing ridge between the two hemispheres. These white clusters around the divide show evidence of some sort of disease, most probably meningitis, which is quite widespread.

We are now looking underneath the brain. The eyeballs are situated under this bone here.

If we look underneath the brain we can see the nerves connecting to various areas such as the eyes, and the nose. In order to take the brain out of the cranial cavity, all of these nerves need to be cut.

We’re intending to further inspect our victim’s brain by slicing it, and cutting it into sections. In order to histologically examine the brain in more detail it must be hardened by suspending it in fixing solution. When the brain is set we can separate its sections and more precisely identify the nature of the diseases we have discovered on the brains surface.
Version B

You are about to view part of an autopsy of an unidentified deceased male. The body was found in a park on the 25th February, 2005. Trauma to the head and legs was apparent, and two gunshot wounds were found: one of them in the chest. The body is believed to be linked to a serial killer that has presumably been at large for the last two years. All supposed victims, thirteen in total, have been found within a one kilometre radius. However, this body differs in that the victim is much older than the others.

We have now removed the scalp, and to orientate you, here is the face, some trauma on the right hand side above the ear, and moving down to the hairline of, as we can see, the elderly victim. We will now remove the skull cap, and we can see that a saw cut has been made around the skull, but it may still be difficult to remove it as there may still be adhesions between the bone and the surface of the brain.

Here we can see a protective layer over the brain, which is quite thick and tough, and this front portion here has been ripped, probably due to the nature of the trauma to the head, and we can now see that there is actually some bruising in the frontal area of the brain due to what can now be assumed to be quite significant head trauma.

Here we can see the protective layer over the brain being removed. Given that this layer is quite tough, a great deal of force must have been dealt to actually tear them within the skull. The perpetrator may have been trying to knock the victim unconscious, or there may have been a struggle. As no specific heavy weapons were found at the scene, this head trauma may have even been sustained by slamming the victim’s head into an object: Perhaps the side of a car.

That layer has now been removed and we can now see the entire surface of the brain. Here we have the frontal lobe, the temporal, and occipital lobes. And here is the dividing ridge between the two hemispheres. These white clusters around the divide show that there is evidence of some sort of disease, which is quite widespread.

We are now looking underneath the brain. The eyeballs are situated under this bone here.

If we look underneath the brain we can see the nerves connecting to various areas such as the eyes, and the nose. In order to take the brain out of the cranial cavity, all of these nerves need to be cut.

We’re intending to further inspect our victim’s brain by slicing it, and cutting it into sections. In order to histologically examine the brain in more detail it must be hardened by suspending it in fixing solution for 5 to 6 weeks. When the brain is set we can separate its sections and more precisely identify the nature of the diseases we have discovered on the brains surface.
C.2 State Trait Anxiety Inventory – State version

**STAI – State**

Directions (PLEASE READ CAREFULLY BEFORE FILLING IN):

A number of statements which people have used to describe themselves are given below. Read each statement and then circle the appropriate number to the right of the statement to *indicate how you feel RIGHT NOW, at this moment*.

There are no right or wrong answers. Do not spend too much time on any one statement but give the answer which seems to describe how you generally feel.

1 = Not At All  2 = Somewhat  3 = Moderately  4 = Very Much So

<p>| | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
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</thead>
<tbody>
<tr>
<td>1.</td>
<td>I feel calm</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>2.</td>
<td>I feel secure</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>3.</td>
<td>I am tense</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>4.</td>
<td>I am regretful</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>5.</td>
<td>I feel at ease</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>6.</td>
<td>I am worrying over possible misfortunes</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>7.</td>
<td>I feel rested.</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>8.</td>
<td>I feel anxious</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>9.</td>
<td>I feel comfortable</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>10.</td>
<td>I feel self-confident</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>11.</td>
<td>I feel nervous</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>12.</td>
<td>I feel upset</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>13.</td>
<td>I am jittery</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>14.</td>
<td>I feel high strung</td>
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<td>2</td>
</tr>
<tr>
<td>15.</td>
<td>I am relaxed</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>16.</td>
<td>I feel content</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>17.</td>
<td>I am worried</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>18.</td>
<td>I feel over-excited and rattled</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>19.</td>
<td>I feel joyful</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>20.</td>
<td>I feel pleasant.</td>
<td>1</td>
<td>2</td>
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C.3 Depression, Anxiety, Stress Scales 21 (DASS21)

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

PANAS Questionnaire

This scale consists of a number of words that describe different feelings and emotions. Read each item and then circle the number that corresponds to the responses below. **Indicate to what extent you felt this way [since watching the film] / [during the past week].**

1 = very slightly or not at all

2 = a little

3 = moderately

4 = quite a bit

5 = extremely

<table>
<thead>
<tr>
<th>Emotion</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
</tr>
</thead>
<tbody>
<tr>
<td>Interested</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distressed</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Excited</td>
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<td>2</td>
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<td>Upset</td>
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<td>3</td>
<td>4</td>
<td>5</td>
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<tr>
<td>Strong</td>
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<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Guilty</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Scared</td>
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<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Hostile</td>
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<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Enthusiastic</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Proud</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Irritable</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Alert</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Ashamed</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Inspired</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Nervous</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Determined</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Attentive</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Jittery</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Active</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
<tr>
<td>Afraid</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
</tr>
</tbody>
</table>
C.5 Impact of Event Scale - Revised

Below is a list of comments. Read each item and decide how frequently each item was true for you after viewing the film. These comments refer specifically to the content of the film. Read each statement carefully and then respond.

If the item did not occur please choose the not at all option. Using the following scale of 0 to 5, circle the number of the response which best describes that item. Please complete each item.

0 = Not at all; 1 = A little bit; 2 = Moderately; 3 = Quite a bit; 4 = Extremely.

1. Any reminder brought back feelings about it.
2. I had trouble staying asleep.
3. Other things kept making me think about it.
4. I felt irritable and angry.
5. I avoided letting myself get upset when I thought about it or was reminded of it.
6. I thought about it when I didn’t mean to.
7. I felt as if it hadn’t happened or wasn’t real.
8. I stayed away from reminders of it.
9. Pictures about it popped into my mind.
10. I was jumpy and easily startled.
11. I tried not to think about it.
12. I was aware that I still had a lot of feelings about it, but I didn’t deal with them.
13. My feelings about it were kind of numb.
14. I found myself acting or feeling like I was back at that time.
15. I had trouble falling asleep.
16. I had waves of strong feelings about it.
17. I tried to remove it from my memory.
18. I had trouble concentrating.
19. Reminders of it caused me to have physical reactions, such as sweating, trouble breathing, nausea, or a pounding heart.
20. I had dreams about it.
21. I felt watchful and on-guard.
22. I tried not to talk about it.
C.6 Free Recall Task

Write down what you remember of the video you just watched. Try and recount the entire progression of the video, including specific factual information and information regarding procedures used in the video. To guide you, here are two questions.

1. At the beginning of the video, a woman in a white lab coat spoke about some of the details of the crime. Recall as much of this information as you can.

2. Describe the sequence of events from the point where the autopsy began. Be as specific as you possibly can. Also, discuss some of the details mentioned about the crime in this section.
C.7 Free Recall Task Scoring Guide

Below is a list of information items contained in both versions of the video. It is possible that participants will have mentioned items that were included in the video that were not included in this list. Items bolded and in italics below are categorical labels, and are not to be considered as information present in the videos. Items in italics refer to information that differed between the videos. You are to score the free recall responses for presence of three types of items. Firstly, participants should be allocated 1 Correct item point for items that they correctly recall as being in the video. If participants mention an item that was included in the video, but not included on this list, allocate 1 Correct item point. Also, allocate 1 Correct item point for every piece of correctly recalled information where information differed between videos. Secondly, allocate 1 Misinformation item point if participants report items contained in the alternate version of the video to the one they viewed. Third, allocate 1 Confabulation item point for items reported that were not included on this list, and not included in the video.

- Woman speaking
  - Lab coat
    - white
- Deceased Male
  - Unidentified
- Location of body
  - A: Car park vs. B: Park
- Date Body Discovered
  - 25th
    - A: January vs. B: February
  - 2005
- Trauma
  - Head
    - A: neck vs. B: legs
- Two gunshot wounds
  - A: both locations revealed
    - Chest
    - Thigh
      - right
  - B: one location revealed
    - Chest
- Serial Killer
  - At large
    - A: Three Years vs. B: Two years
  - Thirteen supposed victims
    - Found within a one kilometre radius
    - A: Two victims found at this location vs. B: victim much older than the others
- Two physicians performing the autopsy
- Scalp has already been removed, showing the skull cap
- Identifies key areas
- Face
- Trauma
  - A: Ear
    - Right
- Hairline
- Elderly victim
- Saw cut has been made around the skull cap
- May be difficult to remove
  - Adhesions
    - Bone
    - Surface of the brain
- Removes skull cap
  - Quite difficult to remove
- Protective layer over the brain
  - Thick and tough
    - Front portion ripped
    - Due to head trauma
  - Bruising
    - Frontal area
    - Due to head trauma
- Protective layer removed
  - Scalpel
  - Layer is tough
    - Great force to tear them
    - Significant trauma
    - Perpetrator may have been trying to knock the victim unconscious
      - May have been a struggle
    - No weapons found
      - A: Forceful bodily violence vs. B: impact with an object
        - B: Object: side of a car
- Looking at entire surface of the brain
  - Frontal lobe
  - Temporal lobes
  - Occipital Lobe
  - Dividing ridge
    - Divides the hemispheres
- White clusters
  - Around dividing ridge
  - Quite widespread
  - Disease
    - A: Identified as meningitis
- Looking underneath the brain
  - Eyeballs
    - situated underneath bone
  - Nerves connecting to various parts of the brain
    - Eyes
- Nose
- Cuts nerves
  - scalpel
  - To remove brain from cranial cavity
- Intending to further inspect brain
  - Slicing it into sections
  - Histological examination
    - Hardened
      - Suspend in fixing solution
        - String
        - \textit{B: 5 to 6 weeks}
    - Cut into sections to identify natures of the diseases
C.8 Recognition Task

With reference to the video you just viewed, answer the following questions as true or false.

When answering, rate the confidence with which you believe your answer to be correct on a scale of 1 – 5.

Scale: 1 = Extremely unsure  2 = Somewhat unsure  3 = Neither confident or unsure  4 = Somewhat confident  5 = Extremely confident

<table>
<thead>
<tr>
<th>Question</th>
<th>True</th>
<th>False</th>
<th>Confidence</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. The victim’s name was John.</td>
<td></td>
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</tr>
<tr>
<td>2. The victim was female.</td>
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<tr>
<td>3. To examine the brain further it will be cut in its current state.</td>
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<tr>
<td>4. Between the two hemispheres of the brain is the dividing ridge.</td>
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<tr>
<td>5. Bruising to the frontal areas of the brain was apparent due to the trauma.</td>
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<tr>
<td>6. In total, the serial killer is suspected to have killed fourteen people.</td>
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<tr>
<td>7. The body was discovered on the 25th January, 2005.</td>
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<tr>
<td>8. A saw was used to cut the skull cap.</td>
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<tr>
<td>9. In the video there were two physicians performing the autopsy.</td>
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</tbody>
</table>
10. Cutting the brain helps to better investigate the disease within it.

11. The head trauma may have been sustained by impact with a car.

12. It takes five to six weeks for the brain to set hard in fixing solution.

13. The skull cap was easy to remove as there were no adhesions between the bone and the brain.

14. The victim was unidentified.

15. The victim sustained trauma to the head. He also sustained trauma to the legs.

16. To remove the brain, all the cranial nerves needed to be cut.

17. One bullet wound was found in the man’s chest, the other was found in the thigh.

18. The thick protective layer covering the brain was torn due to severe head trauma.

19. The brain does not need to be suspended when in the fixing solution.

20. This victim differs from the serial killer’s other victims as he is much older.

21. The victim was found in a car park.

22. Removing the brain is not standard procedure in an autopsy.
23. When the physician examined the victim’s brain, evidence of meningitis was found.

24. All suspected victims of the serial killer were found within a one kilometre radius.

25. The victim’s hair was brown.

26. This was the first of the serial killer’s victims.

27. Of the serial killer’s victims, two were found in the same location.

28. There are no protective layers between the brain and the skull.
C.9 Filler Task

**Instructions:** On the next page is a paragraph for you to read. Whilst you read the paragraph, you will complete a letter circling task. Every time you read the target letter in a word, you need to circle it.

**Target letter:**  N / n

The intensity of the car accident had caused a large crowd to gather rapidly, whilst intrigued drivers of passing cars slowed down to stare. Within minutes of the crash an ambulance had arrived at the scene of the accident to take charge, informally questioning the drivers as they waited for police. The police were only minutes later, the officers alighting from their vehicle to find a scene of conflict between the two drivers. The first driver, a woman, kept screaming: “Are you completely incompetent?! Everyone knows you can’t turn right on a main street without stopping first!” Whilst both drivers were merely shaken, Sam, the three year old passenger of the second vehicle, was hurt and scared, the impact having been to the back left of the vehicle where he had been sitting. The ambulance officers contacted the hospital to alert them of the impending arrival of the infant. It seemed an infinite wait as they sorted the accident scene out but finally they were ready to transfer Sam’s stretcher into the ambulance, leaving the police to deal with the insolent driver of the at-fault vehicle. Once at the emergency department of the hospital, the nurses transferred Sam to a white cubicle gleaming with odd looking instruments so that a full examination of his condition could be made. Due to Sam’s distressed state, doctors started giving him an injection of a mild sedative. It appeared that the most serious damage was to his arm, making it necessary to cut away the bright indigo jumper Sam had been wearing to reveal his wounds. There was a gash in Sam’s arm that would require seven stitches, but which needed first to be sterilised to minimise the risk of infection. Sam’s mother was relieved when the doctors informed her that Sam would be fine and that once the gash was stitched they would only need to keep him in for a few hours for observation. Sam’s mother was quick to thank the hospital staff for their invaluable assistance with her son, and after a few checkups by nurses he was able to be released from hospital that afternoon.
C.10 Experimental Script

**Individual discussions**

**Emotion-focused condition:**

In a moment I will test you with regard to the content of the video you just watched, but before you are tested I would like to have a discussion with you about the video. Specifically, during this discussion, you are only allowed to discuss the types of emotional reactions that you experienced while viewing the video. Discuss the types of emotions that you experienced and the intensity of these emotions. Think about why you felt the way you did. Although you will be picturing the video in your mind, you must avoid directly speaking about any factual information contained within the video. I understand that this task may be difficult, but at this stage I would only like you to discuss your emotional reactions. Try and fully explain the emotion as clearly as possible. If I require any clarification, I may stop to ask you to elaborate or explain. I’d like you to start with how you felt at the beginning of the video when the person in the white lab coat described the details of the crime to you, and continue to discuss how you felt as the video played and the autopsy was conducted.

**Fact-focused condition:**

In a moment I will test you with regard to the content of the video you just watched, but before you are tested I would like to have a discussion with you about the video. Specifically, during this discussion, you are only allowed to discuss the specific content that was contained within the video as you remember it so that you are able to inform me of the entire progression of the video. Make sure that you remember as much detail as possible. While performing this task, I ask you to avoid discussing your emotional reactions to the video. The aim of this discussion is to review factual details contained within the video. To guide your discussion, try and recall the details about the crime that the person in the white lab coat conveyed to you at the beginning of the video and then discuss the information you can remember that was conveyed to you as the video progressed and the autopsy was carried out. If I require any clarification, I may stop to ask you to elaborate or explain.

**Control condition:**

In a moment I will test you with regard to the content of the video you just watched, but before you are tested I would like to have a discussion with you. For this discussion, I will be asking you questions about a variety of things. To start with, can you describe the shoes you are wearing to me (allow description of shoes). Now can you talk about what you had for breakfast this morning (allow discussion). Can you tell me now about your university studies, for example, what degree you are doing, what subjects you are taking (allow time for discussion). Finally, can you tell me about what you did over the past weekend.

**Prompts used by the experimenter during discussions:**

“What do you mean?”, “Tell me more about that.”, or “Is there anything else you would like to add?”
**Group-based discussions**

**Emotion-focused condition:**

In a moment I will test you with regard to the content of the video you just watched, but before you are tested I would like to have a discussion with you all about the video. Specifically, during this discussion, you are only allowed to discuss the types of emotional reactions that you experienced while viewing the video. I would like you all to discuss the types of emotions that you experienced and the intensity of these emotions. Make sure you are aware of how the other person responded to the video and the types of emotional reactions that they experienced while watching the video. Think about why they may have felt the way they did and allow your imaginations to inform the discussion. Although you will be picturing the video in your mind, you must avoid directly speaking about any factual information contained within the video. I understand that this task may be difficult, but at this stage I would only like you to discuss your emotional reactions with each other. During your discussion, try and fully explain the emotion as clearly as possible. If I require any clarification, I may stop to ask you to elaborate or explain. I’d like you to now start with how you felt at the beginning of the video when the person in the white lab coat described the details of the crime to you, and continue to discuss how you felt as the video played and the autopsy was conducted.

**Fact-focused condition:**

In a moment I will test you with regard to the content of the video you just watched, but before you are tested I would like to have a discussion with you all about the video. Specifically, during this discussion, you are only allowed to discuss the specific content contained within the video as you remember it so that you are able to inform me of the entire progression of the video. Together, make sure that you remember as much detail as possible and you should reach an agreement on all details of the video. While performing this task, I ask you to avoid discussing your emotional reactions to the video with each other. The aim of this discussion is to review factual details contained within the video. To guide your discussion, try and recall the details about the crime that the person in the white lab coat conveyed to you at the beginning of the video and then discuss the information you can remember that was conveyed to you as the video progressed and the autopsy was carried out. If I require any clarification, I may stop to ask you to elaborate or explain. You may begin.

**Control condition:**

In a moment I will test you with regard to the content of the video you just watched, but before you are tested I would like to have a discussion with you all. For this discussion, I will be asking you questions about a variety of things. To start with, can each of you describe the shoes you are wearing (allow description of shoes). Now can each of you talk about what you had for breakfast this morning (allow time for discussion). Now can you talk about your university studies, for example, what degrees you are doing, what subjects you are taking and so on (allow time for discussion). Finally, can you talk about what you did over the past weekend.

**Prompts used by the experimenter during discussions:**

“What do you mean?”, “Tell me more about that.”, or “Is there anything else you would like to add?”
# APPENDIX D

## SONA ADVERTISEMENT

<table>
<thead>
<tr>
<th>Study Name</th>
<th>A Post-Incident Debriefing Study</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Abstract</strong></td>
<td>Participants in this study will watch a forensic video, complete some questionnaires and may discuss the video afterward. You will complete a follow up questionnaire online 1 week later.</td>
</tr>
<tr>
<td><strong>Description</strong></td>
<td>In this study you will be required to watch a video that some people may find graphic and stressful. The study will investigate reactions to forensic procedures.</td>
</tr>
<tr>
<td></td>
<td>In the first session (1 hour) you will view the video, complete some questionnaires and be involved in a discussion.</td>
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<tr>
<td></td>
<td>In the second session (1/2 hour) exactly one week later, you will complete some follow-up questionnaires online (the link will be emailed to you).</td>
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<tr>
<td></td>
<td>You must complete BOTH sessions to receive credit. Please do not sign up if you cannot complete the requirements of the second session. Participation is anonymous and voluntary.</td>
</tr>
<tr>
<td><strong>Eligibility</strong></td>
<td>Do not have a history, or current diagnosis, of Post Traumatic Stress Disorder.</td>
</tr>
<tr>
<td><strong>Requirements</strong></td>
<td>Fluent in written and spoken English.</td>
</tr>
<tr>
<td><strong>Duration</strong></td>
<td>90 minutes</td>
</tr>
<tr>
<td><strong>Credits</strong></td>
<td>1.5 Credits</td>
</tr>
</tbody>
</table>
APPENDIX E

PARTICIPANT FORMS
PARTICIPANT INFORMATION STATEMENT

Title: Post-Incident Debriefing Study

(1) **What is the study about?**
You are being asked to participate in a research study about psychological and cognitive reactions to forensic procedures.

(2) **Who is carrying out the study?**
The study is being conducted by Leila Bukalo and will form the basis for the degree of Doctor of Clinical Psychology/Master of Science (The University of Sydney) under the supervision of Professor Louise Sharpe (The University of Sydney), and Dr. Helen Paterson (Lecturer, The University of Sydney).

(3) **What will the study involve?**
The study involves **TWO sessions**.
In the first session you will be asked to complete a task where you will view a short, but **graphic video** of a forensic autopsy and answer some questions. The video is very graphic and depicts a brain being removed from a cadaver. You may then be asked to take part in a discussion about the video which will be videotaped. Finally, you will be asked to answer some brief questions about the forensic procedure and your reactions to the video. You will be asked to complete some additional questions online one week later. The link will be emailed to you.

(4) **How much time will the study take?**
The first session of the study will take approximately 1 hour to complete. The follow-up session one week later will take approximately half an hour.

(5) **Can I withdraw from the study?**
Being in this study is completely voluntary - you are not under any obligation to consent and - if you do consent - you can withdraw at any time without affecting your relationship with the University of Sydney. Although the potential risks encountered in this study are minimal, it is possible that the graphic nature of the video, or some of the questions, may make you feel uncomfortable or upset. If the video or any of the questions prove to be bothersome, we encourage you to either discontinue participation in the experiment, or skip the particular task or questions you do not like. If you decide to withdraw from the study you will receive credit for the hours for which you have participated.
(6) **Will anyone else know the results?**
All aspects of the study, including results, will be strictly confidential and only the researchers will have access to information on participants. A report of the study may be submitted for publication, but individual participants will not be identifiable in such a report.

(7) **Will the study benefit me?**
You will be given course credit for your participation. You may also gain some insight into the research process.

(8) **Can I tell other people about the study?**
You are free to tell other people about the study. However, we ask you not to discuss the details of this study with any of your peers who may become participants in the study, in order to prevent any contamination of future results.

(9) **What if I require further information?**
When you have read this information, Leila Bukalo will discuss it with you further and answer any questions you may have. If you would like to know more at any stage, please feel free to contact Professor Louise Sharpe (louise.sharpe@sydney.edu.au, 9351 4558) or Dr Helen Paterson, Lecturer (helen.paterson@sydney.edu.au, 9036 9403).

(10) **What if I have a complaint or concerns?**
Any person with concerns or complaints about the conduct of a research study can contact the Manager, Ethics Administration, University of Sydney on (02) 8627 8175 (Telephone); (02) 8627 8180 (Facsimile) or ro.humanethics@sydney.edu.au (Email).

*This information sheet is for you to keep*
E.2 Participant Consent Form

School of Psychology

ABN 15 211 513 464

Louise Sharpe M. Clin. Psych. PhD
Professor

Helen Paterson PhD
Lecturer

Leila Bukalo
Doctor of Clinical Psychology/Master of Science Candidate

PARTICIPANT CONSENT FORM

I, ................................................................. give consent to my participation in the research project.

Name (please print)

TITLE: Post-Incident Debriefing Study

In giving my consent I acknowledge that:

1. The procedures required for the project and the time involved have been explained to me, including any inconvenience, risk, discomfort, or side effect and their implications, and any questions I have about the project have been answered to my satisfaction.

2. I have read the Participant Information Statement and have been given the opportunity to discuss the information and my involvement in the project with the researcher/s.

3. I understand that I can withdraw from the study at any time, without affecting my relationship with the researcher(s) now or in the future.

4. I understand that my involvement is strictly confidential and no information about me will be used in any way that reveals my identity.

5. I understand that being in this study is completely voluntary – I am not under any obligation to consent.

6. I consent to video recording.

Signed: ...........................................................................................................................................

Name: ...........................................................................................................................................

Date: ..............................................................................................................................................

Any person with concerns or complaints about the conduct of a research study can contact The Manager, Human Ethics Administration, University of Sydney, on +61 2 8627 8176 (Telephone); +61 2 8627 8177 (Facsimile); or ro.humanethics@sydney.edu.au (Email)
E.3 Participant Debrief

Thank you for choosing to participate in our experiment. We appreciate the time and effort you have put into completing this study. Your responses will provide us with useful data on the stress levels experienced and memory accuracy observed after discussing information related to a distressing event.

Psychological debriefing procedures used in organisations such as the emergency services (e.g. paramedics, police officers and fire-fighters) involve discussions about the event to help individuals recover from trauma and reduce their chances of developing post-traumatic stress disorder (PTSD). Although commonly endorsed, recent research has identified potential adverse effects associated with individual administration of these procedures for post-trauma psychological recovery (Rose, Bisson, Churchill & Wessely, 2002). While debriefing procedures have been found to be ineffective or unhelpful overall, the specific aspects of these procedures which may be contributing to such outcomes is largely unknown.

In addition, debriefing has also been found to negatively impact on memory integrity when administered in a group format (Devilly & Annab, 2008). Specifically, these discussions can increase susceptibility to the ‘misinformation effect’ (Loftus, 1992, 2005). This term refers to the phenomenon observed when individuals incorporate inaccurate information delivered after the event into their memory for events. Misinformation can be incorporated via a variety of avenues, but research has shown that discussions amongst individuals is the strongest way that misinformation can be transferred (Paterson & Kemp, 2006). As emergency services personnel are frequently required to act as expert witnesses in legal proceedings, if debriefing procedures are contaminating personnel’s memories with inaccurate information as a result of discussion, then this could compromise the sense of justice and fairness in our legal system.

Most psychological debriefing procedures involve a discussion of factual details and emotional reactions to the event. Therefore, one of the aims of the current study was to examine whether discussing facts or emotions was more responsible for these negative outcomes (i.e., on psychological wellbeing and memory integrity). The current study was also interested in whether the effects of these discussions differ depending on whether they are conducted individually (one-on-one) versus in a group format. For this reason, participants in this study were involved in either an emotion-focused, fact-focused or unrelated discussion which was held individually (i.e., one on one) or in a group setting.

To achieve the aims of the study, it was necessary to show you a mildly stressful video to simulate the effects of real world trauma. While we took measures to ensure that the video you viewed was not more stressful than needed, if you feel particularly affected by any of the images you have seen whilst participating in this study then we invite you to contact the University of Sydney Psychology Clinic (Mackie Building K01, 2 Arundel Street, Glebe, University of Sydney, phone: 9351 2629).

For those involved in group discussions:
While you were under the impression that you were all viewing the same video as other participants in your group, we needed to show participants slightly different versions of the forensic autopsy video to allow the chance for participants to contaminate each others' memories with inaccurate information during discussion. We later tested your memory to determine whether new or different information was included in your answers which may have been provided by other group members. The misinformation effect is an extremely robust phenomenon, and it is very common for people to include misinformation in their recall for events, so you should not feel embarrassed if you did this.

If you have any questions or concerns about the study, please feel free to contact Dr Helen Paterson, Lecturer (helen.paterson@sydney.edu.au), Prof Louise Sharpe (louise.sharpe@sydney.edu.au), or Leila Bukalo (lbuk6751@uni.sydney.edu.au).

We kindly ask you to please refrain from discussing the current study with your peers, in order to prevent any contamination of future results.

Again, we thank you for your participation.

Leila Bukalo    Dr. Helen Paterson    Prof Louise Sharpe