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# A randomised controlled trial of group debriefing

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## Abstract

There has never been published a randomised controlled trial of group debriefing. In this study we employed an analogue study with students to conduct the first such trial. Sixty-four participants were shown a stressful video of paramedics attending to injured and dead victims of a road traffic accident. Half the participants were subsequently debriefed and half were provided with tea and biscuits and allowed to talk amongst themselves. A 1 month follow-up was administered. It was found that, while the video was rated as distressing, there were no significant differences between the debriefed and non-debriefed groups on measures of affective distress and trauma symptoms. Those who were debriefed later recalled having wanted to talk more to someone about the video than those who were not debriefed. It is suggested that cognitive dissonance may explain this result.

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*Keyword:* Trauma; Group debriefing; Treatment; Outcome; Analogue

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## 1. Introduction

Psychological debriefing (PD) is a generic term describing a variety of brief crisis intervention models with the major aim of mitigating trauma related psychopathologies, particularly that of post-traumatic stress disorder (PTSD; Devilly, Wright, & Gist, 2003; McNally, Bryant, & Ehlers, 2003). Within the genre of PD models, the oldest and most enduring is Mitchell's (1983) critical incident stress debriefing (CISD; Everly, Flannery, & Eyer, 2002). Promoted as a standardised group process with the primary objectives of mitigation of post-traumatic stress and to enable the screening of victims of trauma

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(Everly, 1995), Mitchell's conceptualisation has formed the blueprint for numerous variants of crisis intervention strategies (Deahl, 2000; Rose & Bisson, 1998). Indeed, the practise of CISD has emerged as the de facto response to almost any stressful life experience (Deville, Gist & Cotton, in press; Everly, 1995; Rose, Wessely, & Bisson, 2004; Wessely & Deahl, 2003).

CISD is distinguished by a guided, seven stage structured process, employing the active mechanisms of early intervention, social support, ventilation and education (Everly, Boyle, & Lating, 1999; Everly et al., 2002). The basis of the CISD model is prevention (Everly, Flannery, & Mitchell, 2000) and works on the core notion that everyone experiencing a "critical incident" is at risk for later psychopathology. Thus it assumes that everyone can benefit from the opportunity to share their experience and learn about the human stress response (Litz, Gray, Bryant, & Adler, 2002). As such, the CISD model displays the four process goals generic to most PDs. First, to promote emotional processing through sharing details of the event. Second, to instil the notion that many responses experienced by participants are normal responses. Third, to generally educate participants about stress reactions and ways of coping adaptively with them. Finally, is the provision of a screening mechanism for further intervention to those that may require it (Everly et al., 2002; Litz et al., 2002; Mitchell, 1983).

Despite its apparent popularity, researchers have recently questioned the appropriateness of using such models of interventions (Campfield & Hills, 2001; Deahl, 2000; Foy, Eriksson & Trice, 2001; Gist & Devilly, 2002). A number of recent reviews have lead many authors to suggest that, despite its intuitive appeal, not only do such intervention programs appear to display no efficacy in the reduction of trauma related symptoms, they may indeed be detrimental to longer-term recovery (e.g., Devilly & Cotton, 2003; McNally et al., 2003).

Rose et al. (2004) conducted a meta-analytic review of the PD literature under the aegis of the Cochrane Collaboration, an extensive initiative to support evidence-based practice. Their inclusion criteria limited studies to randomised controlled trials (RCTs) and operationalised debriefing as a single session intervention administered less than 1 month post-trauma which included components of "normalisation" and "ventilation". Studies included in the review predominantly found that debriefed participants were either no better off following debriefing or were deleteriously effected. Two studies (Bisson, Jenkins, Alexander, & Bannister, 1997; Hobbs, Mayou, Harrison, & Worlock, 1996) reported that those who were debriefed were more likely to develop PTSD than non-debriefed participants. Rose et al. (2004) concluded that compulsory debriefings should cease and that resources would be better utilised by focusing on those who go on to develop diagnosable psychiatric disorders. Unfortunately, the randomisation requirement, though scientifically rigorous and laudable, also meant that no group-based interventions were included in the review. This is problematic because group-based debriefing has been a principal method of delivery for this type of intervention.

Likewise, van Emmerik, Kamphuis, Hulsbosch, and Emmelkamp (2002) conducted a meta-analysis of RCTs of debriefing and analysed their results to look at CISD interventions, non-CISD interventions and no treatment controls. They concluded that "non-CISD interventions and no intervention improved symptoms of post-traumatic stress disorder, but CISD did not improve symptoms", (p. 766). They further commented that "claims that single session PD can prevent development of chronic negative

psychological sequelae are empirically unwarranted” (p. 769). However, again, there were no RCTs of *group* debriefing to enter into the analyses.

Narrative reviews have also critically assessed aspects of the available empirical literature, drawing similar conclusions. In their review of early intervention studies most commonly cited by both proponents and critics of intervention programs, McNally et al. (2003) concluded that not only is there no convincing evidence available for the effectiveness of PD, but that “for scientific and ethical reasons, professional should cease compulsory debriefing of trauma-exposed people” (p. 72). Likewise, while Litz et al. (2002) note that “it is premature to conclude unequivocally that PD hinders recovery from trauma” (p. 124), these authors do suggest that enough evidence exists to recommend that programs of debriefing following trauma are inappropriate.

A number of thematic criticisms can be found with the literature. One is the need for more robust empirical investigation. Critical analysis of most PD studies highlights the propensity for methodological inadequacies (Foy et al., 2001; McNally et al., 2003; Rose et al., 2004). Despite practical and ethical difficulties inherent within the area of trauma research (van Emmerik et al., 2002; Zeev, Iancu, & Bodner, 2001), there is a growing demand within the literature for the implementation of RCTs as a means of demonstrating the claimed efficacy of early intervention programs (e.g., Devilly & Cotton, 2003; Flannery & Everly, 2004; Litz et al., 2002; Rose et al., 2004). Of major concern, is the distinct deficiency in RCTs of *group* debriefing sessions. While the most popularly followed models of intervention are advocated as group models of PD (Devilley & Cotton, 2004; Mitchell, 2004) research into their effectiveness has lacked any level of randomised control, been based predominantly on one to one debriefing sessions or contained a mixture of both. If group processes mediate some outcomes, it seems wise to assess group debriefing trials. It has been suggested (Devilley, Gist, & Cotton, *in press*) that, among other possible reasons for iatrogenic outcomes, serial revivification of the event, where participants are exposed to distressed individuals, may lead to a raising of the estimation of threat experienced during the event through a re-evaluative process (Davey, 1993). At this point it is worth noting that increased threat appraisal has been associated as a risk factor for the development of pathological outcomes (Solomon, Mikulincer, & Benbenishty, 1989).

A second issue regards the choice of outcome measures for PD studies. Most studies have concentrated on PTSD symptomatology alone (a point raised by Deahl, 2000), without appraisal to other psychopathologic, social or economic indicators of outcome (Arendt & Elklit, 2001; Litz et al., 2002). While many victims of trauma may not have met the criteria for PTSD, this does not preclude them from experiencing extended periods of distress following sessions of PD. Thus there is a need to consider more subtle outcome effects as may be indicated by levels of depression, alcohol usage, ability to work functionally, variations in sick leave, and changes in interpersonal relations to name a few possible avenues (Deahl, 2000).

One alternative measure that is regularly cited, but highly debated, is that of client satisfaction. Proponents of PD often quote that most recipients of PD perceive the experience as helpful, and use such measures of satisfaction as a sign of efficacy. Yet while most people appear to appreciate the gesture (Devilley et al., 2003), there appears to be no link between such measures as satisfaction and more objective outcome measures (Arendt & Elklit, 2001; Litz et al., 2002; Rose, Bisson, & Wessely, 2003).

Third, while the current debates surrounding PD centre on “if” it works, objective exploration of “how” the mechanisms of PD may affect individuals following traumatic

events is lacking. While popular models of PD employ components of early intervention, ventilation, social support, normalisation and education (Everly et al., 1999, 2002), the effectiveness of such components, either individually or combined, are little understood. Nevertheless, some authors have hypothesised possible interactions between aspects of the PD process and the adaptive coping strategies of the human stress response to explain the possible negative effects of debriefing protocols on post-traumatic psychopathology.

While individual PD may be ineffective for some, for others current debriefing protocols may actually impede the natural and healthy mechanisms of coping (van Emmerik et al., 2002). For instance, the fact that elements of PD can be categorised as containing aspects of exposure therapy, it is possible that the timing and method of intervention may go some way to “increasing” fear through a negative re-appraisal of the events to which they have been subjected (Davey, 1993). While it is generally understood that revisiting and habituating to a traumatic experience is an aid to emotional and cognitive processing, people may, in fact, need a period of distancing allowing minimisation of the event to decrease the perceived danger (Charlton and Thompson, 1996; Gist & Woodall, 1999). In effect, processes of short-term denial, amnesia and numbing about the traumatic experience may be a normal adaptive response to an extreme situation that allows for a period of respite (Creamer, Burgess, & Pattison, 1992; Litz et al., 2002).

These factors may be further exacerbated by the group process (Deville, Wright, & Gist, 2003) common to PD. While the need of perceived social support in times of duress is well documented (Taylor, 1991), there may be fundamental differences in the way natural systems of social support (family, friends, community networks, etc.) and the interactions of enforced groups of colleagues or strangers function (Dyregrov, 1997; Greenberg et al., 2003). In this respect, one criticism of the current models of PD is that they may interfere with the normal responses to trauma by leading victims to bypass natural systems of social support (Gist, Lubin, & Redburn, 1999).

In summary, research into the efficacy of PD has produced no true RCTs of group debriefing and has been confined in outcome to concentrating predominantly on PTSD symptomatology and participant satisfaction. In order to meet this deficit, the current analogue study was designed to look at the effects of conducting a compulsory, structured group debriefing session on aspects of emotional and behavioural responses. Broadly, the research aimed to explore whether, following exposure to an emotive video (Holmes, Brewin, & Hennessy, 2004), groups participating in a structured session of group CISD differ in short-term and longer-term emotional and behavioural responses to the event compared to non-debriefed groups. Our goal was to follow-up students, who had viewed a stressful video of paramedics attending the scene of a fatal car crash, to assess whether (randomly assigned) debriefed groups displayed differential reactions to non-debriefed groups.

## 2. Method

### 2.1. Participant recruitment and group allocation

Sixty-four participants, 21 males ( $M_{age} = 23.0$  years,  $SD_{age} = 9.5$ ) and 43 females ( $M_{age} = 23.6$  years,  $SD_{age} = 9.3$ ), were recruited from Swinburne University. Thirty-nine were first year psychology students who received credit points for participation. The remainder responded to advertisements for volunteers within engineering and biological

science lectures. Initially, 12 trial groups were planned randomly across a 2-week period. Guided by Dyregrov's (1997) discussion on issues of group size and the consideration of time limitations within the current research context, we aimed to populate groups with 6 to 8 participants. After the first week of the recruitment phase, groups were assessed for utility. Groups listing less than 5 participants were disbanded, and new group times established. Groups with more than 12 participants were split, with a second group created at the same time in the following week. Participants in groups larger than 8, but less than 12 were contacted to see if they were available to attend underpopulated groups until a group size of 8 was obtained. This procedure continued during the second week of the recruitment phase, at the end of which, groups containing less than 5 participants were cancelled. In this way, participants were randomly allocated into 1 of the 14 final trial groups. The first was run as a pilot trial in the week proceeding the main testing phase. Based on responses and suggestions from the pilot group, a small number of changes were implemented. Of the remaining groups, 7 were randomly designated as treatment groups based on the availability of the allocated debriefer, and 6 as the non-treatment controls. Group members were reminded by e-mail 4 days, and again by phone 1 day, before their allocated testing periods with information on times and directions.

## 2.2. Measures

The following measures were combined to form two questionnaire sets. The first was a pen and paper questionnaire, with components distributed during various stages of the initial session (see below). It included demographic items, a measure of affective distress (Depression Anxiety Stress Scale; DASS 21), The Interpersonal Support Evaluation List—Short Form (ISEL 12) and measures of emotional/behavioural responses. The second was a web-based questionnaire designed for use during the 1-month follow-up phase and included measures of PTSD symptomatology (Post-traumatic Stress Diagnostic Scale; PDS), emotional/behavioural responses, satisfaction and affective distress (DASS 21).

*Demographics:* General demographic items included measures of age, sex and handedness. Items specific to the current study included measures of exposure to similar audio visual material and motor vehicle accidents, sensitivity to the sight of blood and mental health service history. Participants were also administered a short-term task where they were given a list of words and after studying these for 30 s, asked to recall as many on the list as possible.

*The Interpersonal Support Evaluation List-12 (ISEL-12):* This is the short form version of the 40 item Interpersonal Support Evaluation List (ISEL: Cohen, Mermelstein, Kamarck, & Hoberman, 1985). Initially designed to assess perceived availability of four theoretically separate functions of social support during times of stress (tangible assistance, communicative support, positive comparison and feelings of belonging), the ISEL-12 provides an overall measure of perceived social support.

*The Depression Anxiety Stress Scale-21 (DASS-21):* This is the short form version of the Depression Anxiety Stress Scale (DASS: Lovibond & Lovibond, 1995) self-report inventory measuring affective distress. Each of the three subscales assessing depression, anxiety and stress comprise of seven 4-point severity/frequency scales ranging from 0 (*did not apply to me at all*) to 3 (*applied to me very much, or most of the time*). Acceptable levels of validity and reliability for each of the subscales have been reported. Cronbach's alphas of .94, .87, .91 (Antony, Bieling, Cox, Enns, & Swinson, 1998) have been reported for the

three subscales, respectively, and test–retest reliability coefficients for the full 42 item version over a 2-week period of .71, .79, .81 have been noted (Lovibond & Lovibond, 1995).

*Post-traumatic Stress Diagnostic Scale (PDS):* PTSD Symptomatology was assessed using the symptom assessment component of the Post-traumatic Stress Diagnostic Scale (PDS; Foa, 1995). The PDS is a 49-item, 4-part self-report PTSD assessment instrument. Part 3 of the PDS requires participants to rate 17 symptoms on a 4-point scale ranging from 0 (*not experienced*) to 3 (*almost always experienced*). As the type of critical incident was already predefined (i.e. presentation of the stimulus video), some questions were modified to be incident specific. Two scores are obtained from part 3 of the PDS, the number of symptoms endorsed and a symptom severity rating score. Acceptable levels of validity and reliability have been reported in assessment of the symptom severity scale (Cronbach's alpha of .92 and test–retest reliability of .83 across 10–22 days; Foa, 1995).

*Behavioural and emotional indicators:* In total, 16 question items were developed to assess various behavioural responses (e.g., “to what extent did you physically distract yourself from the content of the video” and “how many times during the last month have you talked about the video with other people”) and emotional reactions (e.g., “how anxious are you about the video we are about to show you” and “in the video, to what degree could you empathise with (i.e. feel for) the accident victims”) to the stimulus video. During the initial session, eight questions were asked at various stages assessing participants anticipatory anxiety about the video, levels of distress at pre-, post-video and post-condition phases, extent of physical and mental distraction from the video and levels of empathy with people depicted in the video. The final item assessed content of any post-video discussion by the group, and represents the only difference in questionnaires presented between conditions. For those in treatment groups, the question simply asked what aspects of the video were discussed during the debriefing session. Those in the non-debriefing groups were asked to indicate how much the video was discussed during the break, and what aspects, if any, were discussed. During the follow-up session, participants were assessed in relation to how distressing they remembered the video to be and levels of thought and discussion about its contents, both during the initial session and the intervening 4 weeks. Items concerned with the number of times participants thought or talked about the video were scored on a 6-point scale ranging from 1 (*not at all*) to 6 (*at least once a day*). The remainder was scored on a 5-point scale ranging from 1 (*not at all*) to 5 (*extremely*). All items are then assessed as independent measures. The qualitative results were not analysed in this study, but the individual question items were analysed in both the “emotional and behavioural responses to participation” section of our results (below) and “Supplementary analyses” section.

*Satisfaction:* Participant satisfaction was assessed by a single item requiring participants to rate “*How satisfied were you with the way in which you were dealt with after watching the video?*” on a 9-point scale ranging from 1 (*not satisfied at all*) to 9 (*completely satisfied*).

### 2.3. Setting and procedure

All trials were conducted in a single meeting room containing a central conference table and non-fixed seating for 10. The audiovisual stimulus was presented via a Trinitron colour television positioned as to insure clear visibility to all participants and sound level

set to 31. The room also contained an area to facilitate provision of refreshments (urn, tea, coffee, cold drinks and biscuits). This setting was maintained for all groups.

On arrival, participants received information about the experiment and gave written informed consent of participation. Pre-video components of the questionnaires were then distributed, which included assessing demographics, affective distress, and anticipatory anxiety. After their completion, word lists were distributed face down, and participants instructed that when told, they were to turn over the lists and would have 30 s in which to memorise as many of the words as possible, and then at the end of that period they would be told to turn the page face down. On completion of this task, participants were then informed that they would now be given 45 s in which to write down as many of the words, in any order, as they could remember. On completion of the immediate recall task, all questionnaires were collected and participants informed that they would now be viewing a 10 min video of emergency workers attending the scene of a car accident. They were again reminded that some may find the video distressing and that they were free to leave with no obligation to complete the experiment. The lights were then turned off and the video started.

The video stimulus is based on 10 min of live footage following US emergency workers attending the scene of a single vehicle road accident. During the first 9 min, the video focuses on the assistance given to the four surviving victims. While obvious that three of the victims are in pain and shock, there are no obvious indicators as to the extent or type of their injuries. Hence for this portion of the video, viewers are exposed mainly to emotional and psychological implications of the accident, with no exposure to any high degree of physical trauma (such as open wounds, bleeding, etc.). The final minute of video depicts the scene after removal of the surviving victims. In this section, viewers witness the removal of the single fatality resulting from the car crash. While previously, astute viewers may have noticed indications suggesting the existence of fifth victim within the car, the final minute of video concentrates solely on the removal of the body from the car.

At completion of the video, the lights were turned back on and the post-video components of the questionnaire were distributed. These questions assessed behavioural and emotional reactions to the video (e.g., mental avoidance). Once completed, questionnaires were collected. Debriefing groups were then informed that they would soon receive a session of debriefing conducted by a trained psychologist in regards to the contents of the video. The facilitator then directed them to help themselves to refreshments while the experimenter fetched the assigned debriefer (see below). For the control groups, the facilitator informed participants that there would be a short break in proceedings as it was necessary to collect some material for the next part of the experiment and, in the meantime, were directed to the refreshments.

Fifty minutes later (after refreshments with or without debriefing) post-condition components of the questionnaires were then distributed. This assessed memory for aspects of the video (not presented here). After all participants had completed the questionnaires, they were informed that they were again to be given 45 s in which to write down as many words as they could remember from the list they had been shown near the start of the session. At completion of the delayed recall test, participants in control groups were also provided with contact details of the local psychologist and facilitator in case any further issues, concerns or general questions should arise following the session. All groups were then informed that they would be contacted in 4 weeks regarding the follow-up session and thanked for their time.



Reminder e-mails containing instructions and a link to the web-based questionnaire, were sent to all participants on the day before their 4-week follow-up session. Participants were informed that it was time for their follow-up session and that it would entail spending approximately 20 min completing a web-based questionnaire. Follow-up e-mails were sent to participants that had failed to complete the questionnaire after 2 days and also contacted by phone calls after 5 pm.

#### 2.4. Interventions

*Debriefed groups:* Treatment groups were provided with an 40–50 min session of PD based on the seven stage CISD model of group debriefing, led by a psychologist specialised in the treatment of trauma and PTSD. Following an initial introduction (Stage 1), participants were encouraged to talk about what they saw (Stage 2), thought (Stage 3) and felt (Stage 4) about the video. Participants were then queried about experiencing any stress responses to watching the video (Stage 5), after which components of the normal stress reaction were then discussed (Stage 6). Finally, participants were asked if they had any questions regarding what they had just discussed, before being provided with contact details of both the debriefer and facilitator, in case any further issues, concerns or general questions should arise following the session.

*Control (non-debriefed) groups:* The control condition was initially designed to provide a non-treatment comparison to the treatment group, but also a possible means to assess the natural interaction of group members following a stressful condition. Results of the pilot study, though, suggested that having the experimenter remain within the room following the video phase acted as an inhibitory influence, with participants looking to the experimenter for an indication of what they should do next. Hence, at the start the condition phase, the facilitator left the group on the pretext of needing to prepare material for the next stage of the experiment. After approximately 15–20 min, the facilitator then returned with the final components of the questionnaires. On returning though, the facilitator aimed to avoid overtly disrupting any ongoing discussions and instead simply observed any group interaction until it was time to start the next phase. If questioned, or drawn into conversation, the facilitator joined the discussion as a group member, but with the main aim of not influencing the naturally formed dynamics or flow of conversation.

### 3. Results

#### 3.1. Data cleaning and randomisation

Of the original 64 participants, 5 were removed as they constituted the pilot sample. A further participant (from the “debriefing” condition) failed to complete the follow-up phase. Two missing data points, one for age and the other for empathy with the victims were found for two male participants. These were replaced with the average score of all other male participants in the same condition.

To ensure that the randomisation process had not created any condition bias, groups were compared across various background, presentation and demographic variables. There were no significant differences between treatment and control groups in the distributions of gender, handedness, involvement in motor vehicle accidents at any stage before phase 1 or during the 4-week follow-up period, previous exposure to similar styles

of video as that used for the stimulus or for a history of consultations for emotional problems. Likewise, there were no significant differences in age, blood phobia, perceived social support, anticipatory anxiety regarding what they were about to be shown or memory recall ability (immediate or delayed). No significant differences were found to exist in group allocation sizes between treatment and control conditions and there were no significant differences between the debriefed and non-debriefed groups on the number of total words (correct or incorrect) from the memory task. However, more people in the non-debriefed group had “never seen anything like” the film clip that was shown ( $n = 13$ ) compared to the debriefed group ( $n = 4$ ;  $\chi^2$  (df = 1,  $n = 59$ ) = 5.48, Fischer exact correction for two tailed test  $p < .03$ ). If this were to have been a factor which influenced the results one would expect, from theory, that those who had never seen anything like the stimuli used would be more deleteriously affected by it (i.e., more people in the non-debriefed group would be “shocked”). However, there was no significant difference directly after the video between the two groups on how distressing they found it ( $t(57) = 1.53$ , ns). Therefore, this aspect will not be partialled from any analyses.

Following presentation of the video, there were also found to be no significant differences in the extent to which participants in the two conditions physically or mentally distracted themselves while viewing the video, nor in levels of participant empathy with either the victims or emergency workers depicted within the video. Overall, these results suggest that no significant differences existed within the group compositions (debriefed vs non-debriefed) before the experimental phase.

### 3.2. Emotional and behavioural responses to participation

A repeated measures (time: intake, follow-up) MANOVA for affective distress (as measured by indices of depression, anxiety and stress) found no significant effect for Condition (Wilks' Lambda (3, 54) = .97, ns), but did find a significant effect for the passage of time (Wilks' Lambda (3, 54) = .86,  $p < .04$ ). There was no significant effect for the interaction of condition by time (Wilks' Lambda (3, 54) = .96, ns). Looking at a post hoc univariate test for time (see Fig. 1), a significant main effect was found for anxiety using Tukey's HSD ( $p < .03$ ), but for neither depression nor stress. Generally, participants appeared to be experiencing less anxiety during the follow-up phase ( $M = 5.04$ ,  $SD = 8.02$ ) than when assessed at time 1 ( $M = 6.86$ ,  $SD = 7.25$ ), although this equates to only a small effect size (Hedges'  $\hat{g} = .24$ ).

Likewise, while no significant condition ( $F(1, 54) = .00$ , ns) or condition X time interaction ( $F(1, 54) = .21$ , ns) effects were found for people's initial perceptions of how anxious they were to participating in the study, there was an almost significant main effect of time ( $F(1, 55) = 3.83$ ,  $p < .06$ ). In effect, during the follow-up phase, participants in both groups tended to report being less nervous about participation ( $M = 1.61$ ,  $SD = .59$ ) than was indicated at the time ( $M = 1.82$ ,  $SD = .88$ ). This equates to a small effect (Hedges'  $\hat{g} = .28$ ).

Participants were also asked “how distressing” they found the video, on a Likert-type scale (1–5), directly after watching the video, after the group condition (debriefing or no debriefing) and again at follow-up. There were no significant differences between the conditions over time, yet both groups displayed a reduction of perceived distress caused by the video as time progressed ( $F(2, 112) = 6.83$ ,  $p < .001$ ). See Fig. 2 for a summary of this finding.

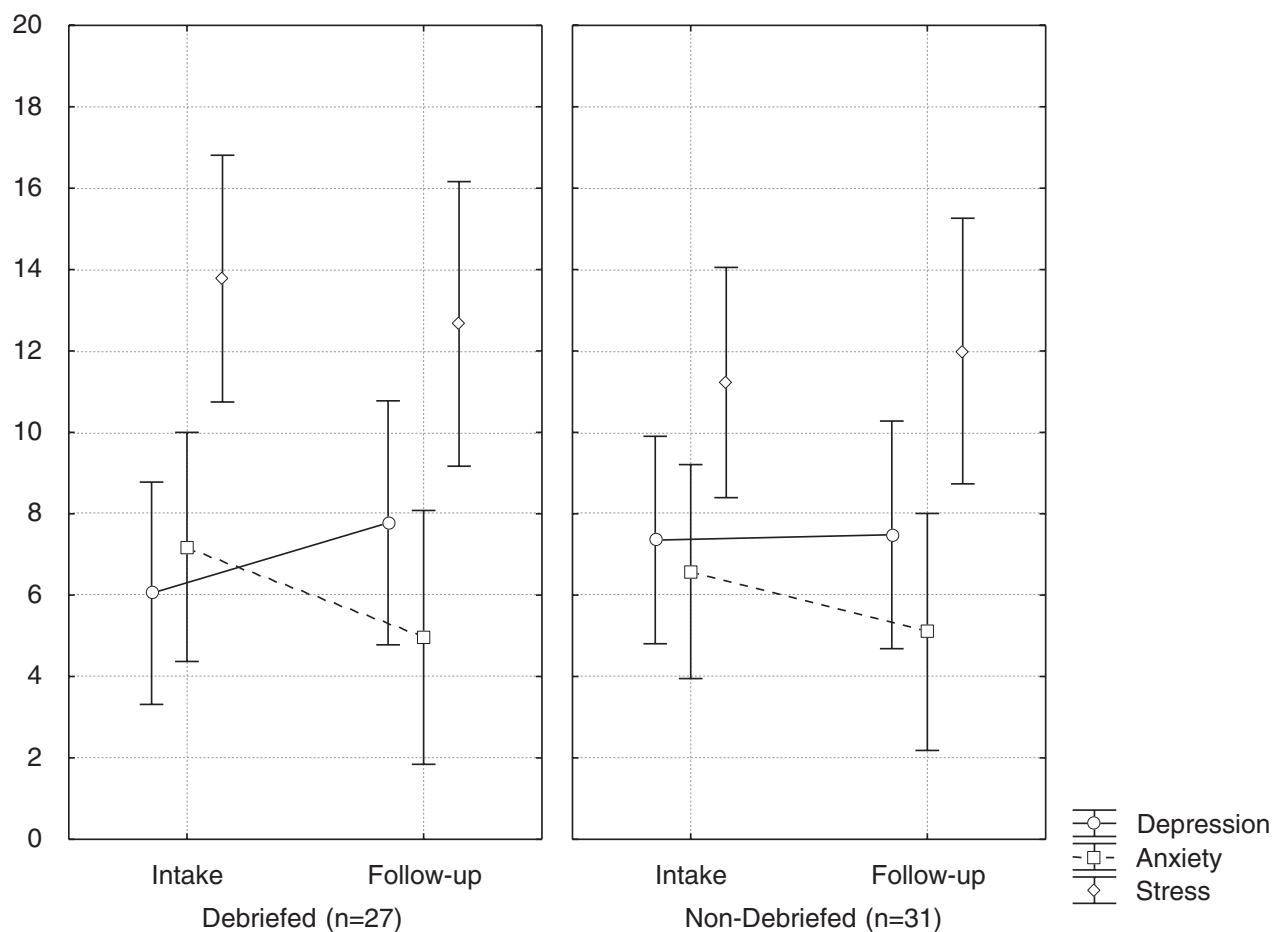


Fig. 1. Affective distress over time between debriefed and non-debriefed groups. *Note:* Vertical bars denote .95 confidence intervals.

Likewise, at follow-up, though a significant difference was found in the amount to which people recalled having talked about the video during the initial session (Mann–Whitney  $U$  ( $n = 58$ ) = 170,  $p < .001$ ; as expected the debriefed recalled having spoken more), no significant differences were found in the extent to which participants within either group had thought (Mann–Whitney  $U$  ( $n = 58$ ) = 408, ns) or talked (Mann–Whitney  $U$  ( $n = 58$ ) = 411, ns) about the video in the intervening period. Interestingly, while neither group displayed any significant difference in the desire to have discussed the video more over the previous month (Mann–Whitney  $U$  ( $n = 58$ ) = 369.5, ns), a significant difference was found in the desire to have discussed the video during the initial session (Mann–Whitney  $U$  ( $n = 58$ ) = 251,  $p < .01$ ). That is, while those in the Debriefed groups recalled having discussed the video to a greater extent during the initial session ( $M = 2.63$ ,  $SD = .93$ ) than those in non-debriefed groups ( $M = 1.58$ ,  $SD = .72$ ), they also reported to have wanted to have discussed the video more ( $M = 2.56$ ,  $SD = .93$ ) than those in the non-debriefed group ( $M = 1.90$ ,  $SD = .70$ ) when asked at follow-up. However, the degree to which people remembered as wanting to have discussed the video did not correlate with any of the outcome measures ( $p < .05$ ) nor with change scores in how distressing they found the video from after watching to follow-up ( $r = .015$ , ns).

No significant differences were found in PTSD symptomatology total scores as measured by the PDS (Mann–Whitney  $U$  ( $n = 58$ ) = 401.5, ns). Fig. 3 displays the PDS data within the three clusters for each condition.

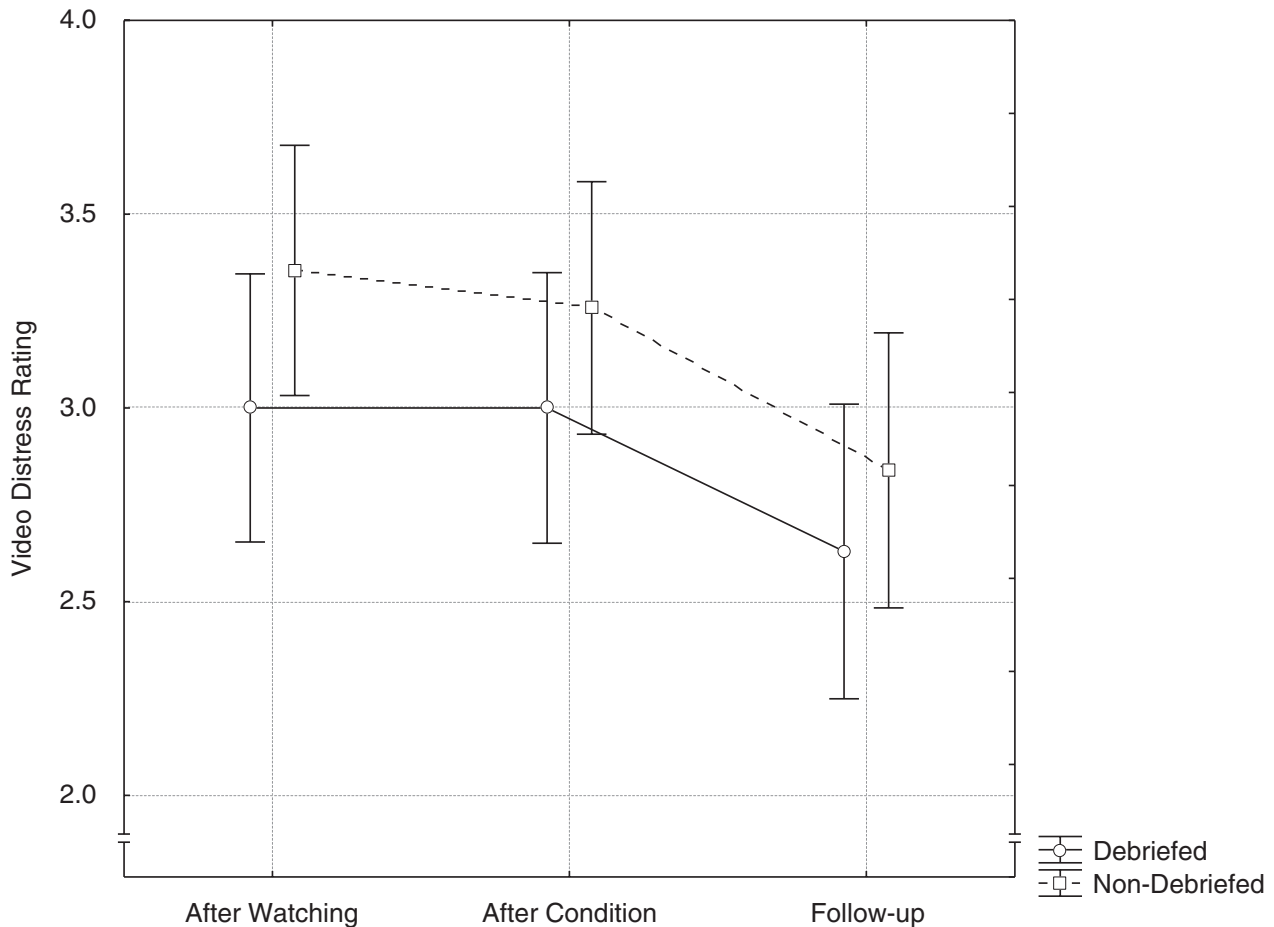


Fig. 2. Perceived distress while watching the video, as a function of time and debriefing condition. *Note:* Vertical bars denote .95 confidence intervals.

### 3.3. Supplementary analyses

Social support was assessed to see whether there were any differences between the two conditions which might, otherwise, explain the results and need to be entered as a covariate in the above calculations. There was not a significant difference between the two conditions on this measure ( $t(56) = -1.60$ , ns), and social support did not correlate with outcome for the re-experiencing ( $r = -.08$ ), the avoidance ( $r = .20$ ), or the hyper-arousal ( $r = -.20$ ) subscale of the PDS. As one would expect, perceived social support did negatively correlate with depression at both intake ( $r = -.41$ ) and follow-up ( $r = -.30$ ).

In analysing data for differences between debriefed and non-debriefed participants' levels of satisfaction, results of a Mann–Whitney  $U$ -test suggest no significant differences ( $U (n = 58) = 378$ , ns) in participant satisfaction with intervention (or lack thereof) during the condition phase. At follow-up, both the Debriefed ( $M = 7.85$ ,  $SD = 1.71$ ; mode = 9) and non-debriefed ( $M = 7.87$ ,  $SD = 1.53$ ; mode = 9) participants reported high levels of satisfaction with the way they were treated by the researchers. Finally, there were no significant differences between conditions on the degree to

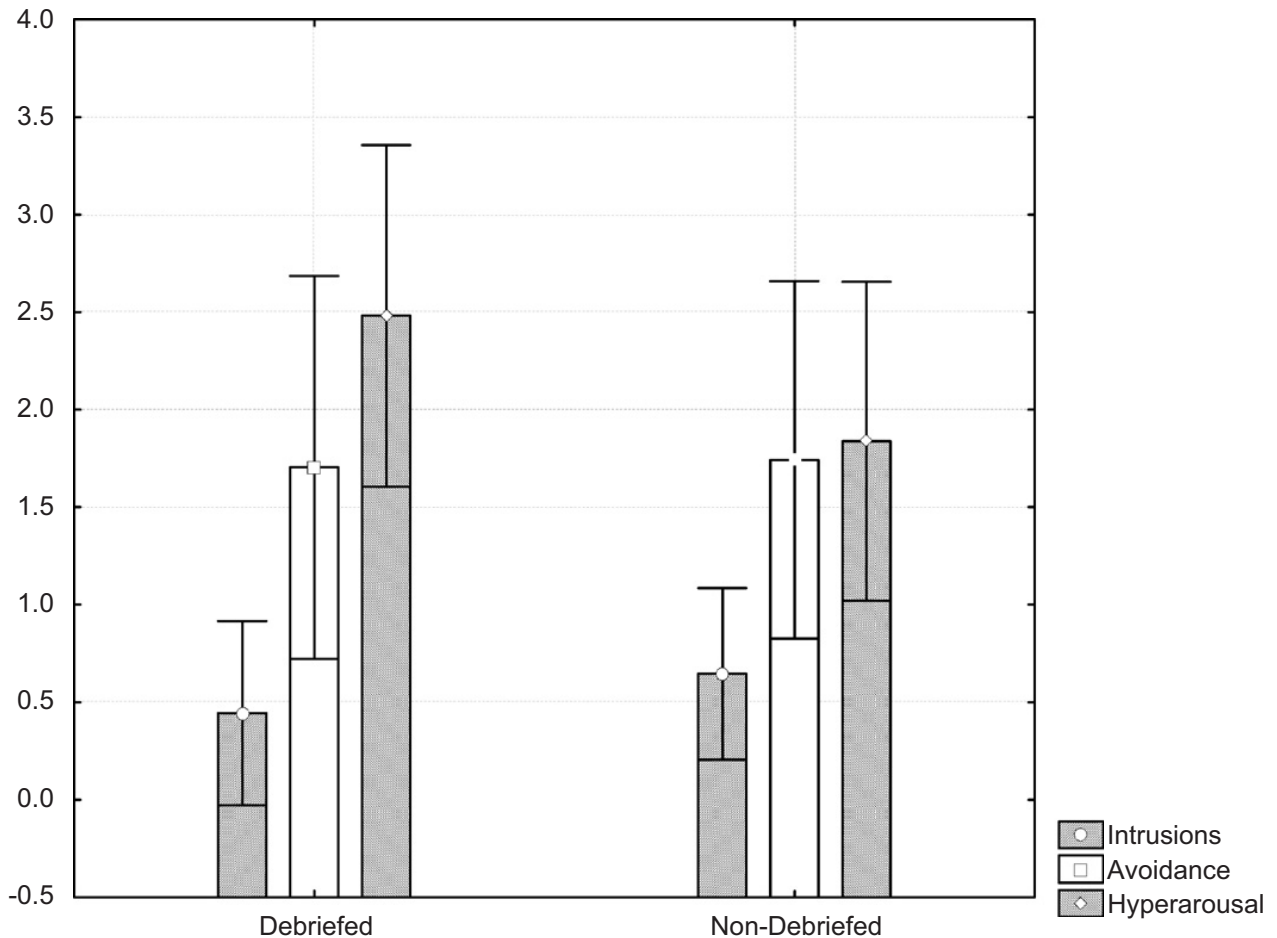


Fig. 3. Post-traumatic stress symptom cluster severity by condition at follow-up. *Note:* Vertical bars denote .95 confidence intervals.

which participants discussed the video with other people over the month's follow-up ( $U(n = 58) = 411$ , ns).

#### 4. Discussion

This study represents an analogue experiment which attempted to assess the utility of PD following viewing of an emotionally stressful video. Graduate students watched a video of paramedics attending the scene of a real road traffic accident where victims had been either injured or killed. Half of the samples were debriefed using the CIRD (Mitchell, 1983) and then followed-up 4 weeks later. There were no differential reactions noted between the two groups on affective distress or trauma-type symptomatology and both groups displayed similar minimisation of how distressing they had found the video over time. However, at follow-up, those that had received debriefing reported to have wanted to discuss the film more, directly after the viewing, than those who did not receive the debriefing.

Overall, these results suggest that group debriefing, following the viewing of a stressful video, has no obvious effects on affective distress and no impact on later psychological health. People in both conditions tended to report very few intrusions, avoidance and hyperarousal phenomena in relation to the video and there evidenced no differ-

ential responses in relation to depression, anxiety and stress levels. These results are similar to those of individual debriefing trials with personally experienced stressful events (e.g., Conlon, Fahy, & Conroy, 1999; Lee, Slade, & Lygo, 1996). They are also consistent with the results obtained by Litz et al. (2004). In a trial of group debriefing of returning peacekeepers these authors presented at conference no impact in either direction from the debriefing.

Our results would suggest that cognitive dissonance (Festinger, 1957) may be playing some part in people's "appreciation of the gesture". In effect, having been debriefed individuals appear to over-estimate, in retrospect, the degree to which they wanted to be debriefed. One hypothesis for the paradoxical effects of debriefing noted in some randomised clinical studies may relate to this phenomenon: if people believe they wanted to discuss the event more following debriefing this may elevate their appraisal of how distressing the event had been. This, in turn, may decrease these individuals' self-perceived ability to cope with the challenges of life and, again, negatively effect outcome. Our current results do not support the first part of this hypothesis—the debriefed group remembered wanting to have discussed the video more than those who were not debriefed, yet this rating did not correlate with how distressing they remembered the video as being at the time. However, we did not assess for the second part of this hypothesis: people's self-perceived ability to cope with what they saw. These aspects need further investigation in a field trial where the stressor is more severe and unexpected.

Indeed, the impact which debriefing may have on one's memory of events is an important area of research and one which also requires further investigation. Eye witness testimony and threat appraisal may be negatively influenced where misinformation is supplied or fear reactions are expressed by other people. Indeed, Devilly, Varker, Hansen, and Gist (in press) have found that the provision of misinformation during debriefing leads to memory error and a misplaced and increased confidence in these errors by the participant. To what degree eye witness testimony can be tainted by debriefing and to what extent this affects long-term stress of the witness is still being investigated.

This research has some limitations. Firstly, an analogue sample was utilised due to lack of compliance from government and private sector organisations in obtaining a representative clinical sample. As such, we believe that it is appropriate that an analogue sample be used for the first randomised, controlled trial of group debriefing to be published. Also, on average, when asked how distressing people found the video directly after watching it, they rated it as between "somewhat" and "very much" (mode of "somewhat"). This would give some credence to the belief that the stimuli was appropriate. In the case where the stimuli is more severe by virtue of being completely unexpected and truly shocking (occurring outside of the laboratory) one might hypothesise that any effects would be magnified—a point worth considering given the hyperarousal means. Another aspect worth keeping in mind is that people may be more at risk of being influenced by any inaccuracies of other people during the debriefing process, as discussed above.

Secondly, only one therapist (G.D.) provided the debriefing. Whilst it may be desirable to weigh therapist effects, under the current design there was a systematic and consistent delivery of the intervention. The mean satisfaction rating with the debriefing was 7.85, with a median of 8.5 and a mode of 9—the highest score possible. This would suggest that the debriefing was at least conducted "sensitively".

In summary, this research found that group debriefing after watching a stressful video had no observable effect on psychological health in comparison to not being debriefed. The only significant effect appeared to be in increasing the degree to which participants later recalled having wanted to talk to someone about the stimuli. Cognitive dissonance is forwarded as a likely explanation for this result.

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