

Shorter communication

The effect of stressor severity on outcome following group debriefing

Grant J. Devilly*, Tracey Varker

Clinical & Forensic Research Unit, Brain Sciences Institute, Swinburne University, PO Box 218, Hawthorn, Vic. 3122, Australia

Received 20 April 2007; received in revised form 18 September 2007; accepted 18 September 2007

Abstract

This analogue study reports data on the efficacy of group debriefing in the mitigation of distress for a stressful video, which had two levels of severity. It also provides a new procedure for use in experimental psychopathology studies. One hundred and nineteen participants were shown one of two stressful videos and, subsequently, 67 participants received group debriefing whilst 52 participants acted as a control. A statistical difference was found between the two groups for level of distress at follow-up, with those who had watched the more stressful video scoring higher on video distress and trauma-type symptomatology than those who watched the less stressful video. This was particularly the case for those who received debriefing—adding further caution to the longer-term effects of systematised group interventions following harrowing events.

© 2007 Elsevier Ltd. All rights reserved.

Keywords: Group debriefing; Video; Distress; Trauma; Experimental psychopathology

Introduction

The purpose of this study was to examine whether the efficacy of group debriefing to mitigate distress differed according to the severity of a stressful video of paramedics attending the scene of a car accident and whether video severity manipulation had any effect on distress. Psychological debriefing is a generic term for a range of brief crisis intervention models, which primarily aim to mitigate trauma-related psychopathology, particularly that of post traumatic stress disorder (PTSD; Devilly, Gist, & Cotton, 2006; McNally, Bryant, & Ehlers, 2003). Such interventions are usually provided within 72 h following the trauma and usually involve strategies that aim to promote some degree of client ventilation, education, and emotional processing. Group debriefing refers to the administration of psychological debriefing to a group of individuals. A recent analogue, randomised, controlled trial of group debriefing found that there was no significant difference on measures of affective distress and trauma symptoms for those who received group debriefing compared with those who did not after watching a reasonably distressing video (Devilley & Annab, 2006). This finding is consistent with the growing body of evidence that individualised psychological debriefing is at best ineffective

*Corresponding author. Tel.: +61 3 92145920; fax: +61 3 92145230.

E-mail address: gdevilly@swin.edu.au (G.J. Devilly).

and at worst harmful (Litz, Gray, Bryant, & Adler, 2002; McNally et al., 2003; Rose, Bisson, & Wessely, 2004; Van Emmerik, Kamphuis, Hulsbosch, & Emmelkamp, 2002). However, it could be claimed that an analogue study cannot be an ecologically valid substitute for real-life trauma as it is inherently unable to cause differential and trauma-like distress based upon stimulus-type presentation. This study also speaks to this concern. Were this methodology not ecologically valid we would not see any trauma-like symptoms in our participants and differential presentation severity would not lead to differential trauma-like symptom severity.

There is some evidence to suggest that those who receive debriefing following events that produce psychological disequilibria are at greater risk of failing to reach resolution. Mayou, Ehlers, and Hobbs (2000) found that for their group of car accident victims there was no difference between an individualised debriefing intervention and non-intervention 4 months post-injury, whilst 3 years later those who received the debriefing intervention were significantly more symptomatic—failing to improve over time. However, it was those who were most distressed following the event who appeared to be most deleteriously affected by debriefing. There are no direct research data that have looked at the possible causes for this result.

No previously published study has compared group debriefing for two differing levels of stressor severity due to the inability to manipulate this variable. The aim of the current research was to examine the impact of video severity upon the level of distress reported by participants: (a) directly after viewing the video; (b) after debriefing; and (c) 1 month later. This study also aimed to examine the impact of debriefing upon participant distress. It was hypothesised (1) that those in the ‘disfigured face’ group would report higher levels of distress both in session 1 and follow-up compared with the ‘body only’ group and (2) that, combined, these data would show no difference in the level of distress for those who received debriefing compared with those who did not.

Method

Participants and group allocation

One hundred and twenty-four participants, 56 males ($M_{\text{age}} = 28.52$, $SD_{\text{age}} = 10.84$) and 68 females ($M_{\text{age}} = 26.47$, $SD_{\text{age}} = 10.43$), were initially recruited through the University and local papers. Of these 124 participants, five were to be used for piloting the intervention, leaving 119 participants for intended analyses. Group 1 ($n = 58$) consisted of 18 males ($M_{\text{age}} = 24.44$, $SD_{\text{age}} = 7.42$) and 40 females ($M_{\text{age}} = 24.28$, $SD_{\text{age}} = 9.13$), and group 2 ($n = 61$) consisted of 34 males ($M_{\text{age}} = 29.96$, $SD_{\text{age}} = 11.56$) and 27 females ($M_{\text{age}} = 31.36$, $SD_{\text{age}} = 11.97$).

For the purpose of this report, two pre-existing data sets were used. As has been previously reported in Devilly and Annab (2006), group 1 watched a video of paramedics attending the scene of a car accident, and they were shown a deceased body at a distance (the ‘body only’ video). Group 2 watched the same video, with an additional 5 s of footage at the end, which showed the deceased’s severely disfigured face (the ‘disfigured face’ video). The same methodology that was used in Devilly and Annab (2006) was used for study 2. There were two overall aims of study 2: (1) to investigate the efficacy of group debriefing in mitigating stress reactions and (2) to investigate the impact of group debriefing upon eyewitness memory for a traumatic event. Results relating to the impact of group debriefing upon eyewitness memory for a stressful event have been previously reported in this journal by Devilly, Varker, Hansen, and Gist (2007). That aspect of the study showed that participants who had been debriefed with a confederate present who provided misinformation during the debriefing were more likely to incorporate this misinformation into their actual memory of the event and were more confident in such incorrect answers than for correct responses. However, the emotional response data described in this study had never been analysed or presented before. Furthermore, the data from study 1 and study 2 had never before been compared and, to our knowledge, no similar study had conducted such comparisons.

For the original study involving group 1, participants were randomly assigned to receive either group debriefing or given tea and biscuits and allowed to talk amongst themselves. For the original study involving group 2, participants were randomly assigned to one of three conditions: group debriefing; group debriefing with a confederate; or control (tea and biscuits). For the group debriefing with a confederate condition, a confederate provided participants with misinformation directly related to central and peripheral aspects of the video. This misinformation was given to test a hypothesis related to memory, and did not target the

participants' emotional interpretation of the event. Statistical examination revealed that there were no differences between the debriefing and the debriefing with a confederate group for any of the demographics, nor for distress in either session 1 or at follow-up. As such, the debriefing and debriefing with a confederate groups were combined for the present analyses.

Setting and procedure

Participants were randomly assigned to groups of between five and 11 people. These groups completed pre-trial assessment measures and then watched a video of paramedics attending the scene of a road traffic accident. Those in group 1 saw only up to a dead body lying on a sheet (see Devilly & Annab, 2006, for more details) while those in group 2 saw an extra 5 s, which focused on the disfigured face of the body (see Devilly et al., 2007, for more details). Participants then completed a few more questions and then either received group psychological debriefing (described below) or were allowed to mingle freely and talking amongst themselves, with access to tea and biscuits. After this conditional assignment, the group completed more questionnaires and were then followed-up 4 weeks later, sending their replies via the mail service.

Treatment groups were provided with a 40–50 min session of psychological debriefing based on the seven-stage Critical Incident Stress Debriefing (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. Treatment adherence ratings were made by the non-treating researcher present in study 1 and by an 'audio visual expert' in study 2. The study 2 'audio visual expert' acted as a confederate in that he helped to set up the video equipment and was also tasked with rating treatment adherence by the debriefer. This rating was conducted without the knowledge of the participants, but the debriefer was aware of this procedure.

Measures

Demographics

General demographics were collected and included age, sex, exposure to similar audio visual material and motor vehicle accidents, sensitivity to the sight of blood and mental health service history.

Distress

A single item was used to evaluate distress. Participants were asked to rate "How distressing did you find the video?" on a 5-point Likert-type scale (1 = *not at all*, 5 = *extremely*). This was assessed at three time points: (a) directly after the video before intervention was provided; (b) after the intervention; and (c) 1 month after viewing the video.

The PTSD Symptom Scale-Self-Report (PSS-SR)

Although no attempt was made to induce PTSD, an assessment was made of the level of intrusions, avoidance and arousal caused by the video 4 weeks after participants had viewed it using the PTSD Symptom Scale-Self-Report (PSS-SR; Foa, Riggs, Dancu, & Rothbaum, 1993). This questionnaire is based upon the Diagnostic and Statistical Manual of Mental Disorders—4th edition (DSM-IV; APA, 1994) criteria for trauma symptomatology, and contains items assessing the 17 PTSD symptoms, which make up the three clusters and assesses incapacity due to these problems. The measure displays excellent internal and temporal reliability (Foa et al., 1993). For this experiment 'the event' was the video, and for analysis purposes the summed intrusions, avoidance and arousal score was termed 'trauma-type symptomatology'.

The Depression Anxiety Stress Scale-21 (DASS-21)

The short-form version of the Depression Anxiety Stress Scale (DASS: Lovibond & Lovibond, 1995) self-report inventory was used to measure affective distress. Each of the three subscales assessing depression, anxiety and stress comprise 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*). The scale has demonstrated good reliability and validity (Antony, Bieling, Cox, Enns, & Swinson, 1998; Lovibond & Lovibond, 1995). This measure was used to gauge negative affect/underlying mood over the course of the study.

Results

Of the 124 participants, five people were removed from the analyses because they constituted a pilot sample and a further person was removed because she failed to complete the follow-up phase. A missing data point for age was found for a male participant. This value was replaced with the average score of all other males in this same condition. Analyses were conducted using the Statistica (version 6.1; Statsoft, 2004) and ClinTools (version 4.1; Devilly, 2007) statistical packages.

An examination of the demographic variables revealed a significant difference between study 1 and study 2 for age ($F(1,117) = 11.51, p < .001$), with study 2 ($M = 30.74, SD = 11.71$) significantly older than study 1 ($M = 24.33, SD = 8.57$; *Hedges' \hat{g}* = 0.62). Further analysis showed that there was not a significant correlation between age and distress, nor age and trauma-type symptomatology (as measured by the PSS). Likewise, there was a disproportionate gender ratio across the two studies, with study 1 having proportionately more females than males, and study 2 having more males than females ($\chi^2(n = 119) = 7.38, p < .01; \phi = 0.25$). Also, gender did not predict outcome within each study. Although these variables had no effect on the interpretation of the main hypotheses, both were used as covariates in all the following analyses as they did have an effect on the interpretation of mood change over time. No significant differences were found between study 1 and study 2 for any of the other demographic items. Means and standard deviations for distress and trauma-type symptoms caused by the video are presented in Table 1.

A 2 (video severity; 'disfigured face' \times 'body only') \times 2 (condition; debriefed \times non-debriefed) repeated measure (post-video, after condition, follow-up) ANCOVA was conducted on the reported distress caused by the video. A significant interaction effect was found for condition and video severity ($F(1,113) = 5.73, p < .02$), with debriefed and non-debriefed reacting differently depending upon the nature of the video shown. In particular, those who viewed the more severe 'disfigured face' video and who received debriefing reported significantly higher levels of distress (overall) than those who viewed the 'body only' video and received debriefing ($F(1,63) = 7.84, p < .007; \hat{g} = 0.70$). The full relationship is shown in Fig. 1.

There was also an interaction effect for time and video severity ($F(2,226) = 3.47, p < .04$). More specifically, looking at univariate analyses, those who saw the 'disfigured face' video reported significantly greater levels of distress by follow-up (corrected $M = 3.20, SD = 1.06$) than those who saw the 'body only' video (corrected

Table 1
Means and standard deviations for distress directly after watching video, after intervention and at follow-up

	Viewed 'body only' video ^a \bar{X} (SD)	Viewed 'disfigured face' video ^b \bar{X} (SD)
Distress directly after video	3.19 (.91)	3.38 (1.00)
Distress after intervention	3.14 (.91)	3.39 (1.14)
Distress at follow-up	2.74 (.98)	3.26 (1.03)**
PSS avoidance	1.66 (2.12)	2.69 (3.11)*
PSS intrusions	.69 (1.06)	1.95 (2.31)**
PSS arousal	1.88 (2.29)	1.90 (2.61)
PSS total	4.22 (4.61)	6.54 (7.05)*

Note: intervention = debriefing or given tea and biscuits; * $p < .05$; ** $p < .001$.

^a $n = 58$.

^b $n = 61$.

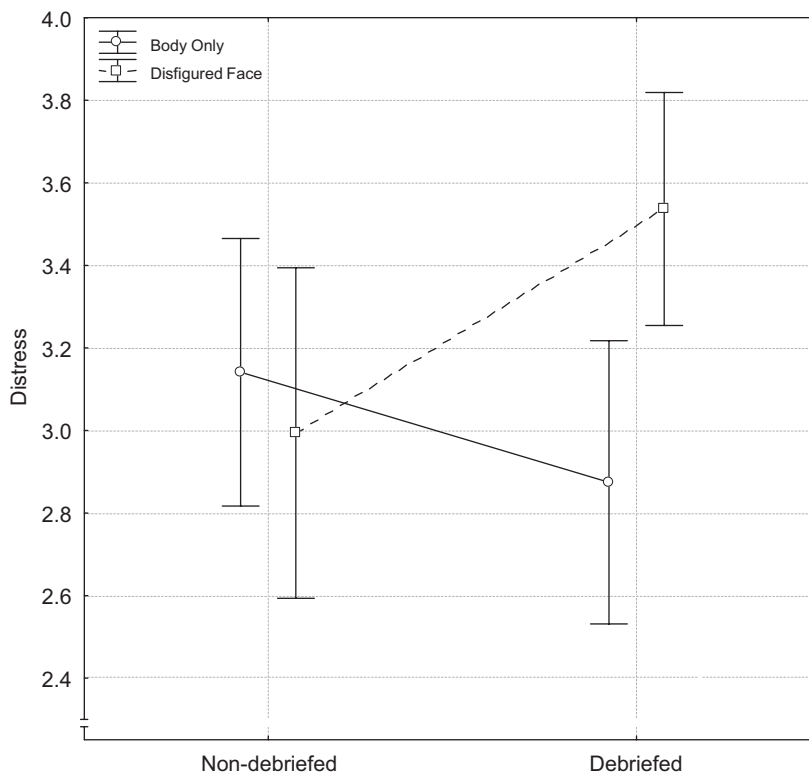


Fig. 1. Level of distress (overall) for those who did and did not receive debriefing.

$M = 2.73$, $SD = 1.01$; $F(1,115) = 5.57$, $p < .02$; $\hat{g} = 0.51$). As can be seen from Fig. 2, those who saw the 'disfigured face' reported a higher level of distress at all three time points, and this difference became accentuated over time.

A 2 (video severity; 'disfigured face' \times 'body only') \times 2 (condition; debriefed \times non-debriefed) MANCOVA was conducted on reported trauma-type symptomatology (as measured by the PSS at follow-up; Intrusion, Avoidance, Arousal). Once again, a significant effect was found for video severity. Those who viewed the 'disfigured face' video reported significantly higher levels of trauma-type symptomatology, related to the video, than those who viewed the 'body only' film ($F(3,111) = 5.34$, $p < .002$). In particular, univariate analysis demonstrated that those who saw the 'disfigured face' reported significantly more intrusions ($F(1,113) = 14.02$, $p < .001$; $\hat{g} = 0.69$) and avoidance behaviours ($F(1,113) = 5.42$, $p < .03$; $\hat{g} = 0.38$) than those who saw the 'body only'. Raw means and standard deviations are presented in Table 1. There was no significant difference in trauma-type symptomatology for the condition of those who received debriefing compared with those who did not receive debriefing ($F(3,111) = 0.92$, ns), and no interaction effect of video severity and condition ($F(3,111) = .69$, ns).

While distress and trauma-type symptomatology was related entirely to the video, general mood was gauged for two reasons: (1) to look for conditional differences and (2) to ensure that the research paradigm had not had more deep-seated negative effects on the participants. A 2 (video severity; 'disfigured face' \times 'body only') \times 2 (condition; debriefed \times non-debriefed) ANCOVA was conducted on negative affect (as measured by the DASS-21 before watching the video and at the 4-week follow-up). There were no significant effects related to mood over time, between video severity conditions or between intervention conditions ($p < .05$).

Discussion

This study aimed to examine the impact of video severity upon the level of distress reported by participants caused by the video. Contrary to expectation, those who saw the more severe video ('disfigured face') did not

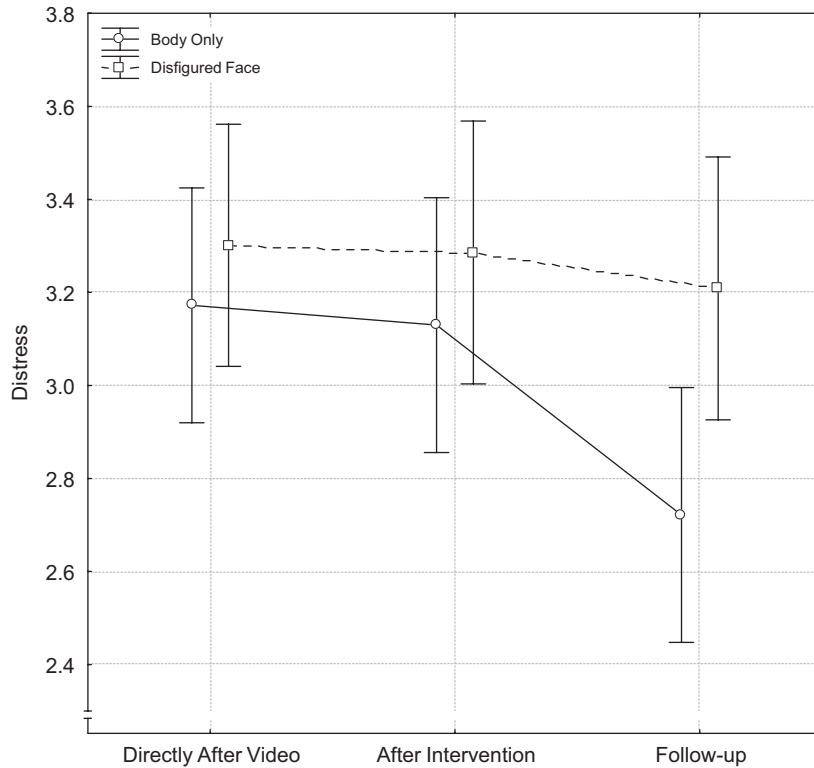


Fig. 2. Distress directly after video, after intervention and at follow-up for study 1 and study 2.

display higher levels of distress both (a) directly after the video and (b) after intervention (debriefing or being given tea and coffee), compared to group 1. However, those who saw the more severe video did report significantly higher levels of distress caused by the video at follow-up than those who viewed the less severe video ('body only'). This was further supported with the result that trauma-type symptoms were also more prevalent in the more severe video condition at follow-up. These results tend to support the hypothesis that manipulation of an analogue trauma stimulus can have affective and trauma-type symptomatic corollaries—supporting its use in experimental psychopathology studies. However, it also suggests that, generally, it takes time for these effects to incubate to the point where differences are apparent, an outcome paralleling clinical sample results (e.g., [Bisson, Jenkins, Alexander, & Bannister, 1997](#)).

It was also found that those who were shown the more severe video and received debriefing were those who rated the video as most distressing (overall)—possibly arguing in favour of the hypothesis that debriefing pathologises experiences once the event is past a certain threshold of distress. [Devilley et al. \(2006\)](#) have argued that the mechanisms that may be responsible for this include pathologising of normal human reactions through the priming and prepping of symptoms during the education phase of debriefing, heightening arousal and threat appraisal through serial revivification and not allowing distancing from the event (which has been found to be predictive of successful adaptation; [Charlton & Thompson, 1996](#)). It also appears that time had less of an attenuating effect on those in the more severe condition. These results need careful consideration in light of the results of [Mayou et al. \(2000\)](#). These researchers found that those who were the most distressed directly after an event, and who received individualised debriefing, were those who were most likely to not display resolution over time. Taken together with the current results, this suggests that presentation/trauma severity may require a threshold over which any negative effects of intervention become apparent. In other words, if one were to see debriefing as providing a message of human fragility to recipients and a certain level of threat/disgust is reached, then the internalising of the perceived horror by those most distressed (as might be occurring with debriefing) may explain some variance of the maintained distress. However, we should keep in

mind that it was only possible to collect trauma-type symptomatology at one time point for the present study; therefore the initial trauma-type score could not be used as a predictor of later response to debriefing, as did Mayou et al. (2000).

It should also be noted that in both studies negative affect did not significantly increase over time (the means in all conditions actually decreased). Such a result adds weight to the argument that this methodology does not create any long-lived harm to the participants or is ethically questionable. While the PSS and distress items were specifically related to the video content, the items of the DASS-21 measured underlying mood and pathological presentations. The results clearly show that our participants did not increase in *pathology* over time, whilst the debriefing did have a negative impact on how *distressing* people found the video in the more severe condition. It should also be stressed that rigorous safeguards were applied during this study, such as warning people who had recently lost loved ones in car accidents that continuation in this study was inadvisable, providing psychological referrals should anyone be unduly upset (as is the norm with CISD anyway) and conducting a 9-month email follow-up to check on the emotional welfare of the participants.

Overall, the findings of the current study suggest that group debriefing did not decrease participants' distress and adds to the growing body of literature which suggests that routine use of non-targeted, highly systemised debriefing is inadvisable (e.g. Van Emmerik et al., 2002). The present study also suggests that further analogue studies using differing levels of trauma severity are feasible and warranted, and that field studies into group debriefing and other new strategies aimed at preventing pathology should be conducted before their use is sanctioned.

Acknowledgement

The authors would like to thank Mr Shaun Seixas and Dr Karen Hansen for their assistance during data collection and two anonymous reviewers for their helpful suggestions.

References

- American Psychiatric Association (APA). (1994). *Diagnostic and statistical manual of mental disorders* (4th ed.). Washington, DC: American Psychiatric Association.
- Antony, M. M., Bieling, P. J., Cox, B. J., Enns, M. W., & Swinson, R. P. (1998). Psychometric properties of the 42-item and 21-item versions of the Depression Anxiety Stress Scales (DASS) in clinical groups and a community sample. *Psychological Assessment*, *10*, 176–181.
- Bisson, J., Jenkins, P., Alexander, J., & Bannister, C. (1997). Randomised controlled trial of psychological debriefing for victims of acute burn trauma. *British Journal of Psychiatry*, *171*, 78–81.
- Charlton, P. F. C., & Thompson, J. A. (1996). Ways of coping with psychological distress after trauma. *British Journal of Clinical Psychology*, *35*, 517–530.
- Devilly, G. J. (2007). *ClinTools software for Windows: Version 4.1 (computer programme)* <www.clintools.com>. Melbourne, Australia.
- Devilly, G. J. & Annab, R. A. (2006). A randomised controlled trial of group debriefing. *Journal of Behavior Therapy and Experimental Psychiatry*, in press, doi:10.1016/j.jbtep.2006.09.003.
- Devilly, G. J., Gist, R., & Cotton, P. (2006). Ready! Fire! Aim! The evolution of psychological debriefing services and intervention outcome. *Review of General Psychology*, *10*, 318–345.
- Devilly, G. J., Varker, T., Hansen, K., & Gist, R. (2007). An analogue study of the effects of psychological debriefing on eyewitness memory. *Behavior Research and Therapy*, *45*, 1245–1254.
- Foa, E. B., Riggs, D. S., Dancu, C. V., & Rothbaum, B. O. (1993). Reliability and validity of a brief instrument for assessing post-traumatic stress disorder. *Journal of Traumatic Stress*, *6*, 459–473.
- Litz, B. T., Gray, M. J., Bryant, R. A., & Adler, A. B. (2002). Early intervention for trauma: Current status and future directions. *Clinical Psychology: Science and Practice*, *9*, 112–134.
- Lovibond, S. H., & Lovibond, P. F. (1995). *Manual for the Depression Anxiety Stress Scales* (2nd Ed.). Sydney: Psychology Foundation.
- Mayou, R. A., Ehlers, A., & Hobbs, M. (2000). Psychological debriefing for road traffic accident victims. *British Journal of Psychiatry*, *176*, 589–593.
- McNally, R., Bryant, R., & Ehlers, A. (2003). Does early psychological intervention promote recovery from post traumatic stress. *Psychological Science in The Public Interest*, *4*, 45–79.
- Rose, S., Bisson, J., & Wessely, S. (2004). Psychological debriefing for preventing post traumatic stress disorder (PTSD) (Cochrane Review). In *The Cochrane Library, Issue 2*. Chichester, UK: Wiley.
- StatSoft, Inc. (2004). STATISTICA (data analysis software system), version 6 <www.statsoft.com>.
- Van Emmerik, A. A. P., Kamphuis, J. H., Hulsbosch, A. M., & Emmelkamp, P. M. G. (2002). Single session debriefing after psychological trauma: A meta-analysis. *The Lancet*, *360*, 766–771.