

# A randomized controlled trial of individual psychological debriefing for victims of violent crime

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## ABSTRACT

**Background.** It has been suggested that giving people the opportunity to talk about a traumatic experience may prevent the development of later disorder. We tested the efficacy of two brief interventions, education and psychological debriefing, designed to prevent adverse psychological reactions to criminal victimization.

**Methods.** Individuals who had been the victims of a violent crime within the past month were written to and invited to take part in a study of their attitudes to crime and punishment: 2161 were contacted and 243 replied, of whom 157 were eligible and were randomly assigned either to an education condition, to a psychological debriefing plus education condition, or to an assessment only condition. Education involved providing information about normal post-traumatic reactions. Debriefing involved in-depth probing about events, thoughts and feelings experienced during the crime. Subjects were recruited from police and hospital sources and interviewed in their own homes: 138 were followed up at 6 months, and 92 at 11 months.

**Results.** Outcome was assessed using a DSM-III-R diagnosis of PTSD, the Post-traumatic Symptom Scale, the Impact of Event Scale and the Beck Depression Inventory. All groups improved over time but there were no between-group differences.

**Conclusions.** No evidence was found to support the efficacy of brief one-session interventions for preventing post-traumatic symptoms in individual victims of violent crime.

## INTRODUCTION

In contrast to the well-established efficacy of psychological treatment for post-traumatic stress disorder (PTSD) (Rothbaum & Foa, 1996), little evidence yet exists from randomized controlled trials (RCTs) to support the effectiveness of brief preventive interventions that encourage trauma victims to speak in detail about their experiences soon after they have occurred (Bisson & Deahl, 1994; Raphael *et al.* 1995; Rose & Bisson, 1998; Wessely *et al.* 1998). Nevertheless, there is a widespread belief that talking about trauma in a structured way, and in a supportive setting, can help to prevent sub-

sequent post-traumatic reactions (Everly & Mitchell, 1997; Parkinson, 1997). Mitchell (1983) formulated critical incident stress debriefing as a semi-structured group intervention with emergency personnel exposed to a common traumatic event. Dyregrov (1989) termed the process 'psychological debriefing' (PD) and the terms have come to be used interchangeably although there are some differences in the interventions (Rose, 1997). Although originally designed as a group intervention, increasingly psychological debriefing has been used in a considerable range of circumstances both with groups and individuals (Parkinson 1997; Wessely *et al.* 1998). This reflects the fact that many traumas, such as road traffic accidents and assaults, are typically experienced by one or two individuals rather than a group. In this

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paper we present the results of a trial of individual psychological debriefing for victims of violent crime, a group who are known to be at high risk of adverse psychological reactions (Kilpatrick *et al.* 1985; Bisson & Shepherd, 1995).

Of four previous RCTs of psychological debriefing, two were reported by Hobbs & Adshead (1996), one study followed up 42 victims of various traumas, the other followed up 86 road traffic accident victims. In both cases debriefing was carried out within 24 hours of the index event. Lee *et al.* (1996) studied 39 women 2 weeks after having a miscarriage. Bisson *et al.* (1997) examined 110 burn victims, some of whom were debriefed on average 6 days after the injury. No RCT found any benefit for debriefing, and Bisson *et al.* found that debriefing group to have a worse outcome, although there was some suggestion of greater vulnerability at baseline.

There is a clear need for more studies of debriefing carried out on sufficiently large samples of victims of different kinds of trauma, for example victims of crime. In addition, this study goes beyond the previous literature in simultaneously evaluating another kind of brief intervention that might be equally effective, education about post-traumatic symptoms. Although it has been recommended that group debriefing take place 48–72 hours post-trauma, it is unclear whether this delay is appropriate for individual debriefing, and it has been argued that intervening too soon after the trauma may result in a poorer outcome (Bisson *et al.* 1997). We therefore decided to delay the intervention beyond the immediate post-trauma period. Previous trials have sampled consecutive series of patients referred to medical facilities, without establishing their preferred ways of coping with the trauma. However, if some individuals would prefer not to talk about their trauma they may be unlikely to benefit from debriefing, and to include this group in the trial would, therefore, reduce the probability of demonstrating the effectiveness of debriefing. Given evidence that many victims of crime are difficult to engage in treatment (Koss & Burkhart, 1989), perhaps because they do not wish to talk about it, we therefore opted to evaluate the effect of debriefing on participants who identified themselves as willing to talk about their traumatic experience.

## METHOD

### Sample description

To qualify, victims of a violent crime (actual or attempted physical or sexual assault, or bag snatch) had to be over 18 years old, and to have been assaulted by someone who was not a member of their household. Local police and medical services assisted in identifying potential participants, who were sent a letter asking them to contact the research team if they, as recent crime victims, would be willing to take part in a study of attitudes to crime and punishment. No mention of debriefing or of possible psychological benefit was made at this time, in order not to exclude individuals who did not believe themselves to be ill or in need of treatment. After obtaining consent initial interviews were then scheduled, and were in all cases conducted within one month of the crime (mean = 21 days post-crime, range 9–31 days, s.d. = 5.6 days).

Power analysis conducted prior to the study suggested that each group should consist of 52 people in order to achieve an 80% probability of detecting a medium effect size ( $d = 0.5$ ) with alpha set at 0.05 (Cohen, 1988). A total of 2161 letters of invitation were sent, to which 243 (11.2%) responses were received. Some of these responses were inappropriate in that they did not meet the inclusion criteria stated in the outreach letter, either because too long had elapsed since the trauma, they lived outside the study area, they were too young, or they had been assaulted by a member of their household ( $N = 80$ ). After excluding a further six individuals who were too ill or otherwise incapacitated to complete the assessment part of the interview, 157 complete interviews were achieved (7.3% of the initial sample contacted by letter). Eighty-eight per cent of these 157 were successfully followed up at 6 months. By the end of the 3-year study period, when funding for further follow-ups ceased, we had also completed 92 11-month interviews. Individuals followed up at 11 months did not differ significantly from those not followed up at 11 months in terms of their symptom scores at baseline or at 6-month follow-up, largest  $t = 1.35$ ,  $P > 0.10$ . We therefore concluded that they were likely to be representative of the original 157 in terms of their symptom severity.

The 157 participants who entered the study

(118 men, 39 women) had an average age of 35 years (S.D. = 13 years, range 18–76). Data from the police referrals, but not from the hospital referrals, were available in order to check on their representativeness. In comparison with those crime victims identified by the police who did not enter the study, those crime victims whom we interviewed were significantly older,  $t(940) = 4.04$ ,  $P < 0.001$ , but did not differ in gender, type of offence, or extent of injury, largest  $\chi^2(2) = 3.29$ ,  $P > 0.10$ . We also compared our interviewed sample with figures derived from the 1996 British Crime Survey (BCS) for people over age 18 reporting violent crime (excluding domestic assault) in England and Wales (Mayhew, personal communication; Mirrlees-Black *et al.* 1996). Compared with our gender distribution of 75%:25%, the BCS figure was 68%:32%, and the BCS mean age was 33 years. In our sample 45% were married or cohabiting, 38% were single and 18% were either separated, divorced or widowed. Level of education was as follows: 47% of the sample had ended full-time education by the age of 16, with 53% continuing their education beyond this age. Eighty-six per cent of the sample were born in the UK. Overwhelmingly, the sample had experienced actual (94%) or threatened (4%) physical assault. Four per cent had experienced actual or threatened sexual assault. Ten per cent had no injuries, 8% minor weals or abrasions, 49% cuts or severe bruising, and 33% more severe injuries such as broken bones.

## Measures

### *Post-traumatic stress*

This was assessed using the self-report version of the widely-used Post-traumatic Stress Disorder Symptom Scale (PSS) (Foa *et al.* 1993). Seventeen items corresponding to DSM-III-R (American Psychiatric Association, 1987) symptoms are rated on 4-point scales (0, not at all/for less than 2 days; 1, once per week or less/a little bit/once in a while; 2, 2–4 times per week/somewhat/half the time; 3,  $\geq 5$  times per week/very much/almost always). Diagnoses based on the PSS have been shown to be highly concordant with diagnoses based on structured interviews (Foa *et al.* 1993). As well as providing a total score based on summing the items, the PSS was therefore used to generate a diagnosis of PTSD at 6 and 11 months (for details see

Brewin *et al.* 1999, who also report on the relationship between symptoms and diagnoses at baseline and at follow-up in the sample as a whole).

### *Impact of Event Scale (IES)*

This is a 15-item scale of current subjective distress, related to a specific incident (Horowitz *et al.* 1979). It is widely used in research on post-traumatic stress disorder and contains two subscales measuring intrusion and avoidance symptoms.

### *Beck Depression Inventory (BDI)*

This is a widely-used 21-item self-rating measure of depression severity (Beck *et al.* 1961). Respondents rated their depression over the past week.

## Procedure

Eligible participants were randomly assigned, prior to contact with the interviewers, to one of three different types of interview, using a predetermined, computer-generated schedule. The interviews involved either assessment only, education about post-traumatic symptoms, or psychological debriefing followed by education. All were conducted by one of two interviewers. The investigator (S.R.) who designed the interventions had had many years of experience of PD both in group and individual format and had received instruction from leading exponents of the intervention including Jeffrey Mitchell. A second interviewer whose background was in social work (M.K.) was taught the intervention and subsequently supervised by S.R. All conditions included a standard set of measures and brief questions about the nature of the assault. In addition participants were questioned about previous periods of illness to determine whether they met DSM-IV criteria for major depressive episode ( $N = 66$ ), as well as about assaults meeting research criteria for childhood physical and sexual abuse ( $N = 65$ ) (Andrews *et al.* 1990). Participants also rated the helpfulness of the interview on a 10-point scale. All interview material, including telephone follow-up was tape recorded.

### *Educational intervention*

This was based on a specially prepared leaflet written by S.R. that included information on normal reactions to traumatic events and where

and when to find help. Information was related to participants' own experiences and was tailored to the nature of the assault, for example victims of sexual assault received some extra information not provided to robbery victims. The intervention lasted for approximately 30 min.

#### *Psychological debriefing*

Participants assigned to this intervention were asked to give a more detailed account of their trauma. They were instructed that this part of the interview was optional, and that they should not feel obligated to say more than was comfortable for them at the moment. Of the participants assigned to this condition, none refused and all completed the debriefing, enabling us to employ an intention-to-treat analysis.

A manual was written which was loosely based on Mitchell's (1983) protocol, adapted for work with individuals. Unlike the other interventions, which did not provide this opportunity, the aim was for participants to verbalize a full narrative account of the trauma which encompassed facts, cognitions and feelings. Participants were prompted if necessary with specific questions designed to elicit these details, and were encouraged to express negative emotions such as fear, guilt or shame which might in normal circumstances have remained hidden. The debriefing, which lasted for approximately 1 h, aimed to achieve a depth which would not normally occur in everyday conversations the victim might have about the assault. The integrity of the intervention was not formally assessed, but in addition to the ongoing supervision the research team reviewed all the debriefing sessions to ensure that procedures had been correctly followed.

#### **Follow-up**

Follow-up at 6 months and 11 months was achieved by post in cases where we could not visit in person or obtain telephone interviews. Mean scores on the symptom questionnaires completed by these three different methods did not differ from each other either at 6 months, largest  $F(2, 135) = 1.56$ ,  $P > 0.10$ , or at 11 months, largest  $F(2, 88) < 1$ . The proportion of respondents lost to follow-up did not differ between the three groups, either at 6 or 11 months, largest  $\chi^2(2) = 2.45$ ,  $P > 0.10$ .

## **RESULTS**

### **Randomization and interviewer effects**

In order to assess the effectiveness of the randomization, the three groups were compared on demographic and prior history variables (Table 1) and outcome variables (Table 2) at baseline, using  $\chi^2$  and one-way ANOVA tests. With one exception, there were no significant differences between the groups, indicating that the randomization had been successful. The exception was that there was a higher proportion of individuals with education after age 16 in the Education group than in the Assessment Only or Debriefing groups,  $\chi^2(2) = 7.99$ ,  $P < 0.05$ . In order to assess interviewer effects we compared participants seen by each of the two interviewers on the outcome variables, both at baseline and follow-up. No significant differences were found, largest  $F = 2.30$ ,  $P > 0.10$ .

Table 1. *Demographic characteristics of the three groups*

|                        | Assessment only     | Education           | Debriefing plus education |
|------------------------|---------------------|---------------------|---------------------------|
| <i>N</i>               | 51                  | 52                  | 54                        |
| Gender ratio (M:F)     | 42:9                | 39:13               | 37:17                     |
| Age                    | 37.3<br>(s.d. 13.8) | 34.9<br>(s.d. 13.2) | 35.4<br>(s.d. 13.8)       |
| Education after age 16 | 43%                 | 69%                 | 48%                       |
| Past depression        | 46%                 | 40%                 | 41%                       |
| History of child abuse | 39%                 | 42%                 | 43%                       |

Table 2. *Means (standard deviations) of outcome measures at baseline and follow-up in the three groups*

|                                | Assessment only | Education   | Debriefing plus education |
|--------------------------------|-----------------|-------------|---------------------------|
| Baseline ( <i>N</i> = 157)     |                 |             |                           |
| PSS                            | 15.6 (12.6)     | 16.0 (13.2) | 16.8 (13.9)               |
| IES                            | 28.0 (19.3)     | 24.2 (19.0) | 28.5 (18.4)               |
| Six months ( <i>N</i> = 138)   |                 |             |                           |
| PSS                            | 13.0 (12.4)     | 10.9 (11.1) | 13.8 (13.3)               |
| IES                            | 23.3 (20.2)     | 16.7 (18.6) | 19.7 (19.9)               |
| BDI                            | 13.9 (13.1)     | 9.8 (9.2)   | 12.1 (13.0)               |
| Eleven months ( <i>N</i> = 92) |                 |             |                           |
| PSS                            | 11.5 (12.2)     | 9.6 (10.9)  | 11.3 (12.6)               |
| PES                            | 15.9 (19.4)     | 14.7 (19.5) | 15.9 (16.0)               |
| BDI                            | 12.2 (13.1)     | 8.0 (10.1)  | 10.4 (10.2)               |

PSS, Post-Traumatic Stress Scale; IES, Impact of Event Scale; BDI, Beck Depression Inventory.

### Effects of the interventions

At 6 months 28 (20%) of the sample met criteria for PTSD (12 (26%) in the assessment group, 5 (11%) in the education group, 11 (23%) in the debriefing group:  $\chi^2(2) = 3.58, P > 0.10$ ). Following the 6-month interview those found to be suffering from PTSD were advised to seek psychological help and relevant referrals were made with the participants' consent. Subsequently at 11 months we found that ten participants had received or were receiving NHS treatment for their PTSD (two in the assessment group, four in the education group, and four in the debriefing group), and a further ten participants had received or were receiving private treatment for their PTSD (three in the assessment group, six in the education group, and one in the debriefing group). At 11 months eleven (12%) of the sample met criteria for PTSD but this figure should be viewed with caution in view of the treatment being received. The numbers meeting criteria for PTSD at 11 months were too small to permit further analysis at the level of diagnosis, and outcome is therefore assessed using the continuous measures.

Scores on the continuous outcome measures are shown in Table 2. The three groups were compared on PSS scores at baseline and 6 months with repeated-measures analysis of covariance, controlling for baseline scores on the PSS. A similar analysis was carried out on the IES scores. In both analyses there was a significant effect of time, smallest  $F(1, 135) = 21.7, P < 0.001$ , but no effect of group and no interaction, largest  $F(2, 134) = 1.19, P > 0.10$ . As the BDI was not measured at baseline, a simple ANOVA was conducted on the BDI scores at 6 months, but the main effect for group was also nonsignificant,  $F(2, 133) = 1.31, P > 0.10$ . Similar analyses were conducted on the 11 month outcome variables, controlling where possible for baseline scores, and these also failed to show any significant group effects or interactions involving group, largest  $F = 1.59, P > 0.10$ . The analyses on 11-month outcome were repeated excluding participants who had received or were receiving treatment for their PTSD, but the results were unchanged. There was no significant difference in the perceived helpfulness of the three interviews at 6 months  $F(2, 137) = 1.3, P > 0.10$ .

As the data were positively skewed, square-root transformations were performed. This did not result in any change to the findings. Intention-to-treat analyses were also conducted on the PSS and IES scores, substituting baseline scores for 6 and 11 month scores in the repeated-measures analyses of covariance whenever participants were not able to be followed up. Once again there was no change to the pattern of findings, the largest effect attributable to intervention group or group  $\times$  time interaction falling well short of significance,  $F(2, 153) < 1$ . We tested whether the slightly greater age of our sample relative to all crime victims coming in contact with the police might have affected the results by examining the correlations of age with outcome at baseline and follow-up. However, age was unrelated to outcome at any time both in the sample as a whole (largest  $r = 0.14, P > 0.05$ ), and in the subsample who received debriefing (largest  $r = 0.21, P > 0.05$ ). Finally, exploratory analyses were carried out to investigate whether a treatment effect might be confined to a particular subset of participants. This was done by including a second between-group factor in the above analyses of variance and covariance. Based on the previous literature, three dichotomous factors were investigated: gender, previous depression and childhood abuse. There was no evidence of any consistent interaction between the group effect and any of these factors.

### DISCUSSION

This was the first large-scale randomized controlled trial of individual psychological debriefing specifically for victims of violent crime. We failed to find any significant effect of the intervention on standardized outcome measures. Another brief preventive intervention, education about the effects of trauma and about post-traumatic symptoms, also failed to have any noticeable impact on symptoms at follow-up.

The study had a number of limitations, chief among which was the low initial response rate. Similar low response rates among crime victims have been described in the literature (Koss & Burkhardt, 1989), and may reflect a general unwillingness to disclose details of the experience of victimization to strangers. For this reason we

have no ways of establishing how representative our sample was in terms of their experience of crime, their demographic characteristics, or their past history of psychiatric illness and childhood abuse. It should also perhaps be considered that debriefing may be more likely to benefit participants who are unwilling to talk about their trauma, who were probably under represented in our sample. This reluctance perhaps signals the existence of avoidance, which debriefing might assist in overcoming. On the other hand, this group might be very reluctant to accept such an intervention, particularly prior to the development of any disorder.

Another limitation was the absence of standardized interviews to assess PTSD. However, the PSS has been widely used to generate diagnoses of PTSD, and in this study the rate of PTSD among our female participants was very similar to that found in community studies of female assault victims (Kilpatrick & Resnick, 1993). This similarity with rates of PTSD in non-clinical samples is also relevant to the possibility that our failure to find any group differences arose because all three groups benefited positively from the opportunity to discuss their experience with a counsellor, reflect on the crime, complete symptom measures, and so on. This possibility is hard to rule out entirely but, if it were the case, rates of PTSD should have been lower in our study than in community studies of victims who did not routinely receive treatment.

Despite these potential limitations, our results are consistent with every other previous well-controlled study of individual debriefing, many of which used consecutive series of patients and were therefore not vulnerable to selection bias (Hobbs & Adshead, 1996; Lee *et al.* 1996; Bisson *et al.* 1997). It is important to clarify that, like these, our study does not speak to the efficacy of group debriefing, which has not yet been assessed with a randomized controlled trial. However, with the exception of one study in which there was a considerable delay before intervention (Chemtob *et al.* 1997), non-randomized group comparisons have generally found that group debriefing was either no better or resulted in deterioration on standardized symptom measures (e.g. Carlier *et al.* 1988; Hytten & Hasle, 1989; Deahl *et al.* 1994; Kenardy *et al.* 1996).

Our findings are, nevertheless, of considerable interest because of the ways in which our study differed from previous investigations. It is likely that individuals opted in because they were willing to talk about crime and punishment from the perspective of recent crime victims. It could be argued that this made them more, rather than less, likely to benefit from debriefing. Debriefing did not occur immediately after the trauma, but after a period in which some adjustment may have occurred. The sample also contained substantial subgroups with high levels of previous psychopathology and of previous assaults in childhood, and we were able to show that debriefing was not differentially effective or ineffective for these subgroups (or for men and women).

Our data are also consistent with the literature on the effects of expressing emotion on health, which suggests that simply venting or re-experiencing emotion is not always sufficient to bring about clinical improvement. Rather, the emotional material must be recast or restructured, with new meaning leading to clinical improvement (Littrell, 1998). For example, Pennebaker (1993) reported that those participants who benefited from writing on three consecutive days about traumatic experiences were the ones who tended to end up using more cognitive words, being more accepting, and changing their views about themselves. Although a change in beliefs may not be a prerequisite for clinical improvement, many types of psychotherapy can be conceptualized in terms of bringing about change in the meaning of traumatic experiences (Power & Brewin, 1997). It is possible that individual one-session debriefings are insufficient to bring about such changes among people, for example those with previous histories of trauma or psychiatric disorder, who are prone to develop post-traumatic reactions. If this is the case, a more appropriate strategy may be to target vulnerable individuals and give them a more intensive intervention. Foa *et al.* (1995) have recently shown a positive impact on recent assault victims from a brief 4 × 2 h preventive programme of cognitive-behaviour therapy.

This research was supported by a grant from the National Health Service Executive Research and Development Programme.

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