

# A Systematic Review of Single-Session Psychological Interventions ('Debriefing') following Trauma

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## Key Words

Psychological debriefing · Evidence-based practice · Systematic literature review · Critical incident stress debriefing · Randomised controlled trial

## Abstract

**Background:** Single-session psychological interventions such as psychological debriefing have become widely used following traumatic events. The evidence for their effectiveness has been widely debated. This review aimed to consider the evidence for the effectiveness of one-off early interventions within 1 month of a traumatic event. **Methods:** A systematic review using the standard Cochrane Collaboration methodology. Literature searches of various databases were performed to identify randomised controlled trials. The methodological quality of the studies identified was determined using standard measures, and the results were pooled to consider the overall evidence for effectiveness. **Results:** Eleven randomised controlled trials were found, all of individual or couple interventions. Three studies associated the intervention with a positive outcome, 6 demonstrated no difference in outcome between intervention and non-intervention groups and 2 showed some nega-

tive outcomes in the intervention group (these studies had the longest follow-up periods). The methodological quality of the studies varied widely, but was generally poor. This review suggests that early optimism for brief early psychological interventions including debriefing was misplaced and that it should not be advocated for routine use. There remains an urgent need for randomised controlled trials of group debriefing and other early interventions.

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## Introduction

Post-traumatic stress disorder (PTSD) has become a well-recognised psychiatric disorder that occurs following exposure to a major traumatic event. Characteristic symptoms include re-experiencing phenomena such as distressing recollections and nightmares of the trauma, avoidance of reminders, numbing of general responsiveness and hyperarousal including increased irritability and hypervigilance. Criterion A of the DSM-IV classification [1] states that a PTSD sufferer must have experienced, witnessed, or have been confronted with an event that involved actual or threatened death or serious injury, or a

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threat to the physical integrity of self or others and that the individual's response involved intense fear, helplessness, or horror. There remains some debate on the types of trauma that should be included. Uncomplicated childbirth would probably be excluded. This is important when considering the research in this area, as two randomised controlled trials have considered the effectiveness of psychological debriefing (PD) in the prevention of psychological difficulties following childbirth. In order to consider as much evidence as possible regarding the effectiveness of single-session early interventions following traumatic events, this paper has included studies following childbirth and miscarriage.

The National Comorbidity Survey in the USA [2] interviewed a representative sample of 5,877 Americans aged between 15 and 54 years. 60.7% of males and 51.2% of females reported having been involved in a significant traumatic event. The lifetime prevalence of PTSD was 10.4% in females and 5.0% in males. Over one third of sufferers continued to describe PTSD 6 years after diagnosis irrespective of whether or not they had received treatment.

The recognition that individuals may develop PTSD and other mental health difficulties following traumatic events has led to attempts to develop interventions to prevent the development of psychological sequelae. Critical Incident Stress Debriefing (CISD) and other forms of PD have become the most written about, widely practised and well-recognised forms of early psychological intervention following trauma over the last 15 years. CISD was first described by Mitchell [3] in 1983 as an individual or group intervention for ambulance personnel following exposure to traumatic situations in their work. It was described as a form of crisis intervention as opposed to a form of psychological treatment and therefore does not have the same philosophy, i.e. debriefing does not explicitly treat a pathological response. CISD and other models of PD have become recognised as semi-structured interventions designed to reduce initial distress and to prevent the development of later psychological sequelae such as PTSD following traumatic events by promoting emotional processing through the ventilation and normalisation of reactions and preparation for possible future experiences. Further aims are to identify individuals who may benefit from more formalised treatment and to provide early support. It has generally been considered that any individual exposed to a traumatic event is eligible for PD irrespective of the presence of psychological symptoms. It is, however, apparent that many participants of debriefings would have fulfilled the criteria

for acute stress disorder or have symptoms of PTSD, anxiety and depression.

PD have been used with survivors, victims, relatives, emergency care workers and providers of psychological care. During a PD, participants are encouraged to give a full narrative account of the trauma that encompasses facts, cognitions and feelings. In addition, emotional reactions to the traumatic event are considered in some detail with the emphasis on normalisation. Individuals are reassured that they are responding normally to an abnormal event, prepared for later emotional reactions, how to deal with them and where to find further support if necessary. Mitchell [3] initially commented that a follow-up CISD may be necessary several weeks or months after a critical incident with some or all of those initially involved to deal with unresolved issues if present. More recently, he has argued that debriefing should be considered as one part of a comprehensive, systematic, multicomponent approach to the management of traumatic stress [critical incident stress management (CISM)] and that it should not be used as a one-off stand-alone intervention [4]. Despite this assertion, PD has been used as a stand-alone intervention by many practitioners.

Despite its widespread use, there is a noticeable dearth of empirical evidence supporting the use of PD, and 3 reviews have called for further rigorous research in this area [5–7]. A systematic review by Rose and Bisson [8] had originally been published in 1998 and material from this systematic review formed the basis for a protocol and subsequent Cochrane Collaboration Review of 'debriefing' [9]. However, no evidence was found for the effectiveness of one-off interventions in the prevention of psychological sequelae following traumatic events. Indeed, there was some evidence that PD could be harmful to some participants. Since then, there had been further randomised controlled trials of PD and therefore we decided to update the Cochrane Collaboration Review using the rigorous methodology that ensures the systematic reviews produced by the Collaboration provide an accurate reflection of the current evidence for the intervention they consider. The objective of the review was to consider the evidence for the effectiveness of one-off early interventions within 1 month of a traumatic event designed to prevent the later development of psychological sequelae.

## Method

This review used the standard Cochrane Collaboration methodology in conducting a Cochrane systematic review [10].

**Table 1.** Studies examined but not eligible for inclusion in the review

Study	Year	Reason for exclusion
Amir et al. [12]	1998	non-randomised, group intervention
André et al. [13]	1997	not single session, cognitive-behavioural treatment
Brom et al. [14]	1993	multiple sessions, intervention > 1 month
Bryant et al. [15]	1998	sample selected on the basis of acute stress disorder – not a randomised sample of victims; intervention 4 sessions
Carlier et al. [16]	1998	non-randomised
Chemtob et al. [17]	1997	non-randomised, intervention > 1 month
Deahl et al. [18]	2000	non-randomised
Deahl et al. [19]	1994	non-randomised
Doctor et al. [20]	1994	intervention not related to a traumatic event, intervention not PD, 12 sessions of group counselling
Foa et al. [21]	1995	non-randomised
Hytten and Hasle [22]	1989	non-randomised
Kenardy et al. [23]	1996	non-randomised
Matthews [24]	1998	non-randomised
McFarlane [25]	1988	non-randomised
Polak et al. [26]	1975	crisis intervention, not PD
Robinson and Mitchell [27]	1993	non-randomised
Saari et al. [28]	1996	non-randomised
Tadmor et al. [29]	1987	pretrauma intervention
Viney et al. [30]	1985	not PD

#### *Criteria for Considering Studies for this Review*

The criteria for inclusion of a study in this review were: exposure to a traumatic event (childbirth and miscarriage were included); clear criteria for the inclusion and exclusion of study participants; random assignment to experimental and control groups; clearly defined outcomes including the use of standardised/valid measures; delivery of the intervention within 28 days of the trauma; single-session intervention; participants aged 16+, and structured/semi-structured intervention that involved reviewing the traumatic event, discussing cognitions and emotions, normalising them and discussing future coping strategies (i.e. containing key components of PD as described by Mitchell [3] although not necessarily adhering totally to this method).

#### *Search Strategy for the Identification of Studies*

The search strategy was based on the method of systematic reviews recommended by Chalmers and Altman [11]. The six steps listed below were as follows. Firstly, electronic searches were made of the following databases: EMBASE (1985–1996, issue 27), MEDLINE (1970–1995), PsycLIT (1974 to June 1996), SOCIOFILE (January 1974 to December 1995), BIOSIS PREVIEWS (1985–1996/June W4), OCC. SAFETY & HEALTH (1973–1996/April Q1), and PASCAL (1973–1996/June). When undertaking the update for this review, the following electronic searches were made: CCTR (Cochrane Collaboration Trial Register) (Issue 2, 2000 April 00), CINAHL (Cumulative Nursing and Allied Health Literature) (Update code 20000201 Feb-00), EMBASE (Update code 0018 Jun-00), LILACS (Nov. 1999 Nov-99), MEDLINE (Update code 2000073 Jul-00), NRR (Issue 2, 1999), PSYCINFO (Update code 20000401 Jun-00), PSYINDEX (Oct. 1999 Oct-99) and SIGLE (a database of 'grey' literature) (1999). This updated database search was performed by the

Cochrane Collaboration Anxiety and Neurosis Group Trials Register Facilitator.

Within each of these databases, the following headings were used: (Evaluation, Trial, Study, Studies) – subheadings, psychological, debriefing, psychological debriefing, stress, debriefing, stress debriefing, crisis, intervention, crisis intervention, early, psychological intervention, early psychological intervention, preventive, psychological intervention, preventive psychological intervention. Secondly, the reviewers communicated with known experts in the field – namely Alexander, Bolton, Deahl, Dyregrov, Kenardy, Malt, Marks, McFarlane, Mitchell, Turner, Watson, and Yule. Thirdly, the Journal of Traumatic Stress (vol. 1 No. 1. to vol. 13 No. 2) was hand-searched by one of the reviewers (S.R.). Fourthly, additional information was requested via the electronic Trauma Forum based at Oregon University, USA (traumatic-stress@freud.apa.org). Fifthly, references within the identified studies were also inspected for more studies and lastly, relevant conference papers were examined.

#### *Methods following Identification of a Study*

All studies identified as being potentially eligible were considered by the reviewers to ensure that they fulfilled the inclusion criteria. Additionally, each study was examined for its year of publication, country of origin, funding and source of paper discovery, stated objectives, type of traumatic event, demographics, nature/description and setting of the intervention, subjective evidence of effectiveness and cost analysis.

#### *Studies Examined but Not Eligible for Inclusion in the Review*

Other studies which on initial examination were thought to fit the criteria used for this review but did not are briefly listed in table 1.

**Table 2.** Summary of randomised controlled trials of one-off early psychological interventions

Authors (year)	Target population	Time following trauma	Duration min	Types of control and treatment groups	Sample	Outcome	Follow-up period
Bordow and Porritt [33] (1979)	MVA victims	< 1 week	60	standard care, immediate review, 3-month social worker input	70	social worker input fared best followed by immediate review	3–4 months
Bunn and Clarke [34] (1979)	relatives of seriously ill/injured	< 12 h	20	standard care, individual counselling	30	intervention group fared better	5 min
Mayou et al. [35] (2000)	MVA victims	24–48 h	60	standard care, debriefing	106	intervention group fared worse	3 years
Lee et al. [36] (1996)	miscarriage	14 days	60	standard care, debriefing	39	no significant difference	4 months
Hobbs and Adshead [37] (1996)	MVA, assault or dog bite	< 24 h	60	standard care, individual counselling	42	no significant difference	3 months
Bisson et al. [38] (1997)	acute burn trauma victims	2–19 days	30–120	standard care, debriefing	103	intervention group fared worse	13 months
Conlon et al. [39] (1999)	MVA victims	< 14 days	30	advice and leaflet debriefing	40	no significant difference	3 months
Lavender and Walkinshaw [40] (1998)	mothers following childbirth	< 48 h	30–120	interactive interview	114	intervention group fared better	3 weeks
Dolan et al. (in press) [41]	accident and emergency attenders	< 14 days	45–120	debriefing	69	no significant difference	6 months
Rose et al. [42] (1999)	victims of violence	< 1 month	60	debriefing	105	no significant difference	11 months
Small et al. [43] (2000)	mothers following childbirth	< 48 h	not stated	debriefing	1,041	no significant difference	6 months

### Quality Rating of Studies

Methodological quality was assessed by three separate assessment ratings undertaken separately by the three authors. Firstly, quality ratings were made using the methods outlined in the Cochrane Collaboration Handbook, which examines the quality of the trial and, in particular, the quality of randomisation. Secondly, each author rated the studies using quality ratings devised by Churchill [31] designed for studies of psychiatric interventions, where the maximum score is 37. Ratings were made on objectives of trial, sample size, length of follow-up, power, randomisation, standardisation of treatment, blinding, source of population, recruitment procedures, exclusion criteria, demographic descriptions, blinded assessments, reasons for withdrawal, outcome measures, intention to treat, presentation of results, types of data presented, statistical analysis and control for baseline differences. Lastly, a quality measure developed by Kenardy and Carr [32] specifically for studies of debriefing was used. This quality assessment examines the population who will receive the intervention, delineation of the goals of debriefing, randomisation, use of self-report and objective measures and a description of the debriefing procedures including the stated goals, personnel conducting the intervention, manualisation, amount of exposure to PD and use of outcome measures by raters blind to the intervention conditions. Using these three quality measures, differences were resolved by discussion. When data appeared to be missing, the researchers were asked to provide additional information.

### Results

The total number of eligible randomised controlled trials was 11 (table 2). All the studies included were of an individual intervention except one [38] that used PD with both individuals and couples. The studies evaluated a single-session early intervention following a variety of traumatic events. All the studies reviewed concerned adults although specific age parameters were not always given. Three of the studies [33, 34, 43] originated in Australia, one was conducted in Ireland [39] and the rest were conducted in the UK.

### Randomisation/Use of a Control Group

In all the studies, the subjects were randomly allocated to an early single-session intervention or no intervention at all. The study by Bordow and Porritt [33] includes a third group in which individuals received assistance from social workers for a 3-month period in addition to the early single-session intervention and no intervention groups.

### *Clearly Defined Outcomes (Including the Use of Follow-Up) and the Use of Valid Measures*

In 2 of the studies [40, 43], no pre-intervention measures were taken. These 2 obstetric studies utilised the Edinburgh Postnatal Depression Scale [44] as the main outcome measure rather than the Impact of Event Scale (IES). The main objective in these studies was to prevent the onset of postnatal depression.

The other studies used pre- and postintervention standardised/valid instruments and other measures. The follow-up periods were variable, ranging from 1 month to 3 years. Bunn and Clarke [34] considered the outcome immediately after intervention with no follow-up. Bisson et al. [38] considered the outcome at both 3 and 13 months after the trauma, while Mayou et al. [35] included a 3-year follow-up.

### *Criteria for Inclusion/Exclusion*

All the studies reviewed mentioned inclusion/exclusion criteria although the level of information given was variable. For example, 1 of the studies which uses PD following childbirth [43] included women giving birth by LSCS, forceps or vacuum extraction and excluded those women who had not had operative births, had stillbirths or babies weighing <1,500 g, those women who were ill themselves, babies who were ill, or those whose obstetrician refused permission to participate. The other obstetric study [40] included primigravidae with singleton pregnancies and cephalic presentations who were in spontaneous labour at term and who proceeded to have normal vaginal deliveries of a healthy baby. Excluded were those with a third-degree perineal tear, those with a baby requiring special care, a manual removal of the placenta and women requiring high dependency care. Mayou et al. [35] included consecutive admissions to an Accident and Emergency Ward, excluded those who had no memory of the road traffic accident and those who had been discharged before the researchers could make contact. Conlon et al. [39] included ambulant trauma clinic attenders with minor road traffic injuries (mainly soft tissue injury) while excluding those with head injury. Rose et al. [42] included victims of violent crime (actual or attempted physical or sexual assault or bag snatch) and excluded those who had been assaulted by someone from their own household. Dolan et al. [41] included patients presenting at an Accident and Emergency Department in a large Scottish Hospital following a life-threatening or nearly life-threatening trauma, e.g. road traffic accidents, assault, house fire or industrial accident, while they excluded those who had a serious head injury, those too

unwell to co-operate and those injured through the following activities: sports, self-harm, DIY, fights or those who were heavily intoxicated at the time. While such wide and differing exclusion criteria reduce the generalisability of results, with successful randomisation, this should not affect the validity.

### *Timing of the Intervention*

Timing of the intervention varied. In 1 study, the intervention took place immediately following admission of the seriously injured or ill relative [34], in other studies, the intervention occurred 24–48 h after the road traffic accident or when their physical state allowed [37], 2 days postpartum [40], 3–4 days postpartum [43], during the first week of hospital admission [33], 6–12 days following the traumatic experience [41], 3–14 days after the accident (mean = 7 days) [39], 2–19 days (mean  $\pm$  SD = 6.3  $\pm$  3.6) following admission to a regional burns unit [38] and within 24 h of attendance at a hospital casualty department [37]. In 2 studies, the intervention took place a little later. In 1, the intervention took place approximately 2 weeks after miscarriage [36], in the other 21 days after the crime (mean  $\pm$  SD = 21  $\pm$  5.6 days, range 9–31 days) [42].

### *Description of the Intervention*

The type of intervention used is included in table 2. All the interventions involved discussion of the traumatic experience. Four studies [33, 34, 37, 40] used interventions with similarities to the debriefing technique described above but without attempting to follow the formal semi-structured method of debriefing.

### *Methodological Quality of the Studies*

Table 3 provides the results of the methodological quality. Overall, the methodological quality of the studies included was variable. This is partly explained by incomplete data recording. Most gave reasonable information on a priori objectives, and the source of the sample. Four trials [38, 40, 42, 43] had adequate allocation concealment (computer-generated random numbers/opening consecutively numbered sealed opaque envelopes/centralised telephone randomisation); 2 had intermediate [37, 41] (opaque envelopes). For the rest, allocation concealment was either unsatisfactory or unclear. Only 3 studies [38, 39, 42] used a categorical diagnostic category for PTSD. Only 1 study used a true intention-to-treat analysis [35].

**Table 3.** Summary of methodological quality ratings of one-off early psychological interventions

Authors (year)	Churchill scale (max. score 37)	Kenardy scale (max. score 26)
Bordow and Porritt [33] (1979)	11	11
Bunn and Clarke [34] (1979)	8	8
Mayou et al. [35] (1996)	20	13
Lee et al. [36] (1996)	14	14
Hobbs and Adshead [37] (1996)	10	13
Bisson et al. [38] (1997)	23	22
Conlon et al. [39] (1990)	21	15
Lavender and Walkinshaw [40] (1998)	16	10
Dolan et al. (in press) [41]	16	18
Rose et al. [42] (1999)	27	19
Small et al. [43] (2000)	24	11

### *Main Findings of the Studies*

Overall, the results revealed 3 studies with a positive outcome [33, 34, 40], 6 studies with a neutral outcome [36, 37, 39, 41–43] and 2 studies with a negative outcome [35, 38]. Overall, the results are neutral. The 2 studies with the longest follow-ups [35, 38] showed an adverse effect of the intervention. The most commonly used outcome measure was the IES [45]. The variance of the IES results was considerable, with only 1 study [8] having a mean IES score more than 1.6 times the standard deviation making meta-analysis difficult to perform. The meta-analyses performed revealed a neutral outcome except for follow-ups of greater than 1 year when the intervention group fared worse than the non-intervention group, although only the 2 studies with a negative outcome [35, 38] were eligible to be included.

### *Subjective Assessment of the Intervention*

Subjective reports of effectiveness were assessed in 6 studies. Bisson et al. [38] stated that 52% of the respondents found PD ‘definitely useful’. In Lee et al. [36], women who received PD were asked to rate its helpfulness on a 100-mm scale from ‘extremely unhelpful’ (0) to ‘extremely helpful’ (100). The mean score was 74. Hobbs and Adshead [37] stated that 66% of those counselled found the session useful, while 33% said that they did not. The latter gave their reasons, e.g. they felt that the counselling had been offered too early, or that personally, they felt they had not needed it. Rose et al. [42] recorded subjective assessment of the intervention at 6 months of follow-

up and participants were asked to rate on a scale from 1 to 10 how helpful they had found the original interview (0 = unhelpful, 5 = neutral, 10 = helpful). Overall, the mean score to this question was 7 (SD 2.1;  $n = 138$ ) and only 6 rated helpfulness as  $<5$ . There was no significant difference on reported perceived helpfulness between the intervention/non-intervention groups. Small et al. [43] reported very positive views of PD with only 26/463 (5.6%) rating the session as ‘unhelpful’, 200 (43.2%) rating it as ‘very helpful’ and 237 (51.2%) as ‘helpful’.

### *Methodological Quality of Studies*

The studies were ranked in quality order using the three separate quality assessments and then viewed collectively. Overall, the trials by Bisson et al. [38] and Rose et al. [42] scored highest, those by Dolan et al. [41], Conlon et al. [39], Lee et al. [36], Mayou et al. [35] intermediate, and the others low. It was decided to use the Kenardy ratings [32] for the final ranking since this was specifically designed for trials of debriefing. The final rankings were therefore: 1st Bisson et al. [38], 2nd Rose et al. [42], 3rd Dolan et al. [41], 4th Conlon et al. [39], 5th Lee et al. [36], 6th Mayou et al. [35], 7th Hobbs and Adshead [37], joint 8th Bordow and Porritt [33] and Small et al. [43], 9th Lavender and Walkinshaw [40] and lastly, Bunn and Clarke [34].

## **Discussion**

The 11 randomised controlled trials of single-session early psychological intervention following trauma now allow more confident conclusions to be drawn about the effectiveness of individual PD. Unfortunately, there remains an absence of randomised controlled trials of group debriefing. The identified studies have methodological shortcomings including absence of blindness at review (i.e. raters at follow-up not being blind to original treatment conditions), small sample sizes (i.e. lack of statistical power) and variation in techniques used. Given the popularity and continued use of PD, this review shows how an intervention can become commonly used without the evidence to support it. The studies provide little evidence that an early psychological intervention prevents psychopathology following trauma. Some outcomes following individual PD were negative (notably in the studies with the longest follow-ups [35, 38]), but overall, the impact of the single-session early psychological interventions in the review when considered collectively was neutral.

Interestingly, there is a disparity between the subjective and objective findings. Where subjective views of the intervention were asked for, they were usually positive. It may be that there is a widely held perception that to talk about something is useful. It may also be that the opportunity to do so was symbolic of being helped more and hence was well received. It is obviously difficult to compare the intervention with the non-intervention groups, as it is difficult to see how individuals who receive no intervention can be expected to find this helpful, potentially causing some bias. Caution is therefore required in the interpretation of these results.

The reason for the absence of a positive effect is worthy of further exploration. It is important to consider methodological factors that may have contributed to this finding. Differences between the general [31] and the specific [32] methodological quality scales reflected the fact that the Churchill scale emphasises general methodological issues relevant to all clinical trials, with a particular emphasis towards pharmacological trials, albeit relevant to psychiatry. The Kenardy scale gives more weight to specific issues concerning debriefing and in particular the content of the debriefing. The small sample sizes would have increased the possibility of failure to find a real difference as would limited follow-up periods, but this seems an unlikely explanation given the fact that the two studies that used the longest follow-up [35, 38] gave negative results. Uneven groups following randomisation and failure to account for drop-outs were also methodological problems in several studies that could have affected the outcome. Most of the studies failed to measure the quality of the intervention in any meaningful way.

With regard to other possible explanations for the results, it is possible that the interventions were too short and that the intervention led to an increase in psychological distress by virtue of re-exposure to the traumatic event (a form of secondary traumatisation) without allowing time for habituation as happens over several sessions of exposure therapy. Another possibility is that the interventions were not delivered properly and provided by inexperienced therapists. The experience of the therapists was variable, but several studies made attempts to ensure robust delivery of a standard intervention. For example, one of the studies [42] audiotaped the PD which was manualised. The audiotapes were rated by two external experts to ensure adherence to the protocol. A review of therapist experience has suggested that therapist inexperience has a small negative impact on the outcome in psychological treatment studies [46].

Why might treatment have had an adverse effect? There are a number of reasons why PD might be associated with an adverse effect for some. The possibility of the intervention causing secondary traumatisation has already been mentioned. PD by its very nature involves intense imaginal exposure to the traumatic incident within a short time of the event. It is possible that, for some individuals, this serves as a further trauma, exacerbating their symptoms without assisting in emotional processing. Exposure therapy for established PTSD has been shown to cause an adverse reaction in some individuals [47]. A similar mechanism of retraumatisation may be occurring in this population. This possible explanation can be further extended for individuals who develop a sense of shame as a reaction to the traumatic event. Shame is particularly important to consider following certain types of trauma, such as rape or when an individual feels (s)he has omitted to act in a certain way. Re-exposure to the event may increase the sense of shame with possible negative results unless shame is adequately addressed, which would be difficult in a single-session intervention.

Another explanation is that PD may 'medicalise' normal distress. It may also increase the expectancy of developing psychological symptoms in those who might not otherwise have done so. It is a constant finding that no matter how severe the trauma not everyone develops distressing symptoms and only a minority goes on to develop formal long-term psychiatric disorder. PD, by increasing awareness of psychological distress, may paradoxically induce distress in those who otherwise would not have developed it. There is also the danger that PD may be seen, by some, as a substitute for the traditional support given by friends and family. Sometimes it appears that the value of early professional practical support (such as facilitating family contact) is minimised and psychological interventions such as PD become regarded as the optimal response to trauma despite the lack of evidence for this.

PD assumes that there is a uniform, and to a certain extent predictable, pattern of reactions to trauma. At the heart of the intervention is the concept that discussing the trauma is therapeutic, and that attempting to deny it is not. This is based on the time-honoured tradition of psychological thought. However, it does not follow that this is true in every case. Recalling the event may be a 'secondary trauma' for some individuals. Attempting to forget or distance oneself may be an adaptive response. A PD-like intervention may then interfere with adaptive defence mechanisms such as denial.

Whatever the reason for the disappointing evidence for the effectiveness of single-session early interventions, it is important to suggest an appropriate response for individuals involved in traumatic events given the current evidence base. Although offering early psychological support is both humanitarian and instinctive, it would now appear sensible to restrict current 'across the board' immediate psychological interventions, while ensuring early practical support is offered to all those affected. This practical support would include issues such as safety, providing food and shelter, helping get home, helping contact relatives, allowing time off work and (where appropriate) arranging site visits. It also seems appropriate to target those who appear to be at highest risk of developing PTSD and offer them more intensive intervention programmes that have been shown to be effective [13, 15].

Acute stress disorder [1] appears to be an important predictor of chronic PTSD [15, 48]. Research recently completed has developed a simple checklist that predicts onset of later PTSD. The checklist records levels of re-experiencing and hyperarousal symptoms at 1 month after the trauma rather than trying to make the relatively complex diagnosis of acute stress disorder [49]. Such developments could lead to a screen/treat programme where those identified as at risk 1 month after the trauma could be offered a short specific intensive programme of treatment. Such a programme is not intended as a 'one-off' but part of a more focused, specific psychological intervention. Clearly, while this method of working appears to offer important preventive potential, it needs rigorous evaluation and is currently being subjected to a Cochrane systematic review.

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## Conclusion

This updated review has again highlighted the absence of evidence to suggest that single-session early interventions are effective. Indeed, the new studies included since the original Cochrane review [9] have not shown it to be effective. Two conclusions can be drawn: firstly, as discussed in the Cochrane review [10], PD as a blanket intervention for all exposed to trauma has had its day; secondly, psychosocial interventions should only be routinely used when there is evidence that they are effective even when associated with clear need, high face validity and client satisfaction. None of these is a substitute for evidence.

It is important to remember the scope of this review. The conclusions can only be applied to one-off early interventions and not to other interventions, for example of longer than one session following a traumatic event. It should be noted that no comment is made as to the use of PD with children, as this systematic review has not addressed this important group. With regard to adults, the current evidence suggests that an appropriate early response would take care not to make things worse and focus on those at highest risk of psychological sequelae. There is now a need to develop screening measures that can detect the important minority who are likely to go on to develop longer-term mental health problems early on and to be in a position to offer them early preventive interventions that work.

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