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Rose SC, Bisson J, Churchill R, Wessely S

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[Intervention Review]

Psychological debriefing for preventing post traumatic stress disorder (PTSD)

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ABSTRACT

Background

Over approximately the last fifteen years, early psychological interventions, such as psychological 'debriefing', have been increasingly used following psychological trauma. Whilst this intervention has become popular and its use has spread to several settings, empirical evidence for its efficacy is noticeably lacking. This is the third update of a review of single session psychological "debriefing", first having been undertaken in 1997.

Objectives

To assess the effectiveness of brief psychological debriefing for the management of psychological distress after trauma, and the prevention of post traumatic stress disorder.

Search methods

Electronic searching of MEDLINE, EMBASE, PsychLit, PILOTS, Biosis, Pascal, Occ.Safety and Health,SOCIOFILE, CINAHL, PSYCINFO, PSYNDEX, SIGLE, LILACS, CCTR, CINAHL, NRR, Hand search of Journal of Traumatic Stress. Contact with leading researchers.

Selection criteria

The focus of RCTs was on persons recently (one month or less) exposed to a traumatic event. The intervention consisted of a single session only, and involved some form of emotional processing/ventilation, by encouraging recollection/reworking of the traumatic event, accompanied by normalisation of emotional reaction to the event.

Data collection and analysis

15 trials fulfilled the inclusion criteria. Methodological quality was variable, but the majority of trials scored poorly. Data from 6 trials could not be included the meta-analyses. These trials are summarised in the text.

Main results

Single session individual debriefing did not prevent the onset of post traumatic stress disorder (PTSD) nor reduce psychological distress, compared to control. At one year, one trial reported a significantly increased risk of PTSD in those receiving debriefing (OR 2.51 (95% CI 1.24 to 5.09). Those receiving the intervention reported no reduction in PTSD severity at 1-4 months (SMD 0.11 (95%CI 0.10 to 0.32)), 6-13 months (SMD 0.26 (95%CI 0.01 to 0.50)), or 3 years (SMD 0.17 (95%CI -0.34 to 0.67)). There was also no evidence that debriefing reduced general psychological morbidity, depression or anxiety, or that it was superior to an educational intervention.

Authors' conclusions

There is no evidence that single session individual psychological debriefing is a useful treatment for the prevention of post traumatic stress disorder after traumatic incidents. Compulsory debriefing of victims of trauma should cease. A more appropriate response could involve a 'screen and treat' model (NICE 2005).

PLAIN LANGUAGE SUMMARY

Psychological debriefing for preventing post traumatic stress disorder (PTSD)

This review concerns the efficacy of single session psychological "debriefing" in reducing psychological distress and preventing the development of post traumatic stress disorder (PTSD) after traumatic events. Psychological debriefing is either equivalent to, or worse than, control or educational interventions in preventing or reducing the severity of PTSD, depression, anxiety and general psychological morbidity. There is some suggestion that it may increase the risk of PTSD and depression. The routine use of single session debriefing given to non selected trauma victims is not supported. No evidence has been found that this procedure is effective.

BACKGROUND

When a catastrophe occurs it appears to evoke a deep humanitarian need to want to help. Historically this help has been dominated by providing basic physical care e.g. shelter, first aid. However, since the mid 1980s, there has been increased interest in early psychological interventions following exposure to traumatic events. In particular, there has been a huge increase in use of 'one off' sessions of a procedure termed 'critical incident stress debriefing' (Mitchell 1983) or the alternate term 'psychological debriefing' (Dyregov 1989). Inevitably the use of such interventions came under rigorous scientific scrutiny and the first systematic review of the literature was published as Rose and Bisson (1996).

While there may be real humanitarian reasons for wishing to intervene using such procedure there are also other aspects that have bearing on the popularity of such early interventions. The notion of early and effective treatment reducing the onset of PTSD is a compelling one both for those affected as well as organisations and policy makers. A clear example of this is in the military where the original drive is to use early interventions to promote the return of combatants to the front-line as soon as possible.

We know, however, that traumatic events are an important cause of psychological morbidity. This is not only large scale disasters but arguably the more common day to day catertrophes such as road traffic accidents or assaults. Mayou 1993 reported that one year after a road traffic accident a quarter of those followed up had defined psychiatric disorder, with 11% showing evidence of post traumatic stress disorder (PTSD). The current best estimate of the prevalence of PTSD suggests it has a lifetime prevalence of 5% in males and 10% in females (Kessler 1995).

Given that the prevalence of initial distress following a traumatic event is far greater following a traumatic event than that of either acute stress disorder or PTSD, the potential exists to deliver interventions to people whose problems would spontaneously remit). As well as the time commitment required of the traumatised individual, interventions for traumatic stress generally involve confronting aspects of distressing experiences, the emotional cost of which might not warrant early intervention (NICE 2005). Central then to this issue is between those who would like to provide interventions for all those exposed to a life-threatening trauma as opposed to those who would like to target interventions at those at risk of developing chronic PTSD (Brewin 2003).

Understandably, efforts to try and prevent the onset of chronic PTSD continue. PTSD sufferers experience a range of distressing and debilitating symptoms such as involuntarily re-experienceing aspects of the traumatic event in a very vivid and distressing way. This includes flashbacks in which the person acts or feels as if the event were recurring; nightmares; and repetitive and distressing intrusive images or other sensory impressions from the event. Reminders of the traumatic event arouse intense distress and/or physiological reactions. PTSD sufferers often try to push memories of the event out of their mind and avoid thinking or talking about it in detail, particularly about its worst moments. On the other hand, many ruminate excessively about questions that prevent them from coming to terms with the event, for example about why the event happened to them, about how it could have been prevented, or about how they could take revenge. Symptoms of hyperarousal include hypervigilance for threat, exaggerated startle responses, irritability, difficulty concentrating and sleep problems, although PTSD sufferers also describe symptoms of emotional numbing. These include inability to have any feelings, feeling detached from other people, giving up previously significant activities, and amnesia for significant parts of the event. Many PTSD sufferers experience other associated symptoms including depression, generalised anxiety, shame, guilt and reduced libido, which contribute to their distress and impact on their functioning. PTSD shows substantial natural recovery in the initial months and years after a traumatic event. Whereas a high proportion of trauma survivors will initially develop symptoms of PTSD, a substantial proportion of these individuals recover without treatment in the following years, with a steep decline in PTSD rates occurring in the first year (e.g.Kessler 1995).

Debriefing is a psychological treatment intended to reduce the psychological morbidity that arises after exposure to trauma (Hodgkinson & Stewart, cited in Rose 1999). Its origins can be traced to efforts to maintain group morale and reduce psychiatric distress amongst soldiers immediately after combat. It became prominent in the 1980s when the principles were transferred to civilian life. More recently a more comprehensive approach to pre and post incident care termed Critical Incident Stress Management (Mitchell 1997) was developed. In Critical Incident Stress Management, Critical Incident Stress Debriefing (CISD), is described as the fourth component in a seven phase, structured group discussion, usually provided 1-10 days post crisis, and designed to mitigate acute symptoms, assess the need for follow-up and, if possible, provide a sense of post-crisis psychological closure.

Debriefing involves promoting some form of emotional processing/ catharsis or ventilation by encouraging recollection/ventilation/ reworking of the traumatic event. Mitchell 1983 and Dyregov 1989 have operationalised it in seven stages:

1. Introduction
2. The facts
3. Thoughts and impressions
4. Emotional Reactions
5. Normalisation
6. Planning for the future
7. Disengagement

Curtis (1995) suggests an eight stage approach:
1. Identification
2. Labelling
3. Articulation
4. Expression
5. Externalisation
6. Ventilation
7. Validation
8. Acceptance

Debriefing has been used in a considerable range of circumstances. The literature contains accounts of debriefing of police officers involved in shooting incidents, sailors after maritime collisions, Red Cross personnel, adolescents who have been secluded during psychiatric admissions, medical students whose patients have died, families whose children are undergoing bone marrow transplants, any rescue workers involved in any natural disaster, soldiers assigned to grave registration duties, drivers of trains who have witnessed people jumping under their trains, jurors involved in disturbing murder trials, burns victims, road traffic accident victims, rape victims, medical or paramedical staff involved in

failed resuscitations, patients who have recovered from testicular cancer, nurses involved in cancer care, children involved in any accident, casualty staff after traumatic incidents, workers who have experienced or witnessed an industrial injury, or who have colleagues who have been injured, Air Force personnel on bases where fatal accidents have occurred, children in schools where traumatic incidents have taken place (either on or off site) and, no doubt, many other situations.

Debriefing has been routinely offered in a number of settings internationally, including for victims of mass disasters, or individuals involved in traumatic incidents in the workplace. It is usually offered on a voluntary basis, but there are groups for whom it is compulsory following trauma, including bank employees in both the UK and Australia and some UK police forces. The assumption was that debriefing can prevent the onset of PTSD and that such a policy may reduce the threat of litigation over subsequent development of PTSD.

Debriefing has two principal intentions. The first is to reduce the psychological distress that is found after traumatic incidents. The second, related, intention is to prevent the development of psychiatric disorder, usually PTSD.

The effectiveness of debriefing in achieving either of these aims is very uncertain. Exponents of debriefing draw attention to its popularity, and claim that it is meeting important needs (example, Robinson 1995). Others are more cautious. Previous reviews (Shalev 1994; Raphael 1996; Rose 1999; Rick 1998; Litz 2002) have drawn attention to the limited evidence from randomised controlled trials and have raised the possibility that debriefing may actually be harmful.

This review concerns the efficacy of single session psychological "debriefing" in reducing psychological distress and preventing the development of post traumatic stress disorder (PTSD) after traumatic events. This is the third update of the review.

OBJECTIVES

To assess the effectiveness of brief psychological debriefing for the management of psychological distress after trauma and for the prevention of post traumatic stress disorder.

METHODS

Criteria for considering studies for this review

Types of studies

Randomised or quasi randomised trials.

Types of participants

Persons aged 16 and above exposed to a traumatic event. The index event must have taken place no more than 4 weeks prior to the intervention.

Types of interventions

Any single session psychological intervention that involves some reworking/reliving/recollection of the trauma and subsequent emotional reactions. These interventions may be described by trial authors as psychological debriefing; stress debriefing; critical incident stress debriefing; crisis intervention; psychiatric stress debriefing; multiple stressor debriefing; traumatic event debriefing; trauma debriefing. Some interventions labelled as cognitive or behavioural may also satisfy criteria.

Studies will be excluded if they involve:
1. Crisis intervention services for psychiatric patients and/or their families
2. Debriefing of research participants, such as psychology students recruited for studies involving deception
3. Perinatal grief support/bereavement counselling
4. Treatment for PTSD
5. N=1 and cross over designs
6. Interventions aimed at children

Types of outcome measures

1) Rates of PTSD
The Impact of Event Scale (IES) is the most widely used in to measure traumatic stress symptoms. It can be understood as a measure of how much a person is bothered by unpleasant memories of the trauma. These data form the primary outcome measure for this review. Where IES is unavailable, data on any comparable scales (such as Traumatic Neurosis Symptoms Scale or the Clinician Administered PTSD Scale) will be used.

2) General psychological morbidity
This may be measured using a variety of scales, including the Hospital Anxiety and Depression Scale (HADS), the Brief Symptom Inventory (BSI), and the Langer 22 Item Scale of psychiatric symptoms.

3) Depression
This may be measured using a variety of scales, including the Hospital Anxiety and Depression Scale - Depression Subscale (HAD-D), the Beck Depression Inventory (BDI), and the Edinburgh Postnatal Depression Scale.

4) Anxiety
This may be measured using a variety of scales, including Hospital Anxiety and Depression Scale- Anxiety Subscale (HAD-A), Spielberger State/Trait Anxiety, Gottschalk and Gleiser content analysis of anxiety, and Viney and Westbrook cognitive anxiety.

5) General psychiatric morbidity

6) Dropout from treatment

7) General functioning

Search methods for identification of studies

DATABASES; Medline; Psychlit; Embase;Pilots; PASCAL; Biosis; Sociofile; CDSR; Trials Register Cochrane Depression, Anxiety and Neurosis Group.
CINAHL; LILACS;NRR; PSYCINFO; PSYNDEX; SIGLE.

ELECTRONIC SEARCH STRATEGY
1. All references to debrief*, critical incident (no qualifiers), crisis intervention in all databases
2. Cochrane Medline optimal RCT search strategy was combined with key words "explode rape" in MeSH (trauma, traumatic stress, road accident, victim).

3. Cochrane Medline optimal RCT search strategy was combined with PTSD, post-traumatic, stress-prevention (although trials of the management of PTSD are excluded).
4. Embase Cochrane optimal RCT search strategy was combined with psychological debriefing, stress debriefing, crisis, crisis intervention, early psychological intervention, preventive, psychological, intervention, preventive psychological intervention
5. PsychLit, Embase, Sociofile (1974-1995), Biosis Previews (1985-1996), Occupational Safety and Health (1973-1996), PASCAL (1973-1996) for debriefing, stress debriefing, psychological debriefing, crisis intervention, early psychological intervention, preventive psychological intervention
6. The Cochrane Central trials register was searched with key words psychological debriefing; stress debriefing; crisis; crisis intervention; early, psychological intervention; preventive, psychological, intervention; preventive psychological intervention
7. A CCDANCTR search was performed. The search strategy used was; debrief* or 'critical incident' or crisis-intervention or 'crisis intervention or rape or trauma or 'traumatic stress' or 'road accident' or victim of PTSD or post-traumatic or stress-prevention or crisis or 'early psychological intervention' or 'preventive psychological intervention'.
Databases searched and date: CCTR Feb 2005; CCDANCTR Feb 2005.
8. Citation searches on located trials
9. Abstract search of Proceedings of the International Congress on Traumatic Stress
10. Citation search on Impact of Events Scale (Horowitz 1979)

CONTACTS
Contact with key individuals (Alexander, Bolton, Deahl, Dyregov, Kenardy, Malt, Marks, McFarlane, Mitchell, Turner, Watson,Yule).

HAND SEARCH
Journal of Traumatic Stress (all years)
Journal of the Emergency Medical Services (all years)
Journal of Human Stress (all years)
Mass Emergencies and Disasters (all years)

Data collection and analysis

Selection of Trials
The inclusion criteria were applied independently by at least three reviewers. Initially, abstracts of potentially eligible trials were assessed. Where there was uncertainty, the complete article was obtained. Disagreements were resolved through discussion.

Quality Assessment
This was carried out using three methods. First the traditional approach as described in the Cochrane Handbook, which considers method of randomisaton, allocation concealment and intention to treat. The second was the CCDAN scale for the assessment of quality in trials of psychiatric interventions Moncrieff 2001. The third was a scale derived from Kenardy 1996a giving proposed quality standards for trials of psychological debriefing.

Data Management
As far as possible, the analyses maintained the study groups according to the original randomisation procedure. The data was entered into Review Manager and checked by two reviewers independently. All data was then re-checked by a third reviewer.

Data synthesis
For dichotomous outcomes, such as the presence of PTSD, depression or anxiety caseness, the Peto method for computing the pooled odds ratio with 95% confidence intervals was used. For continuous outcomes, the Weighted Mean Difference (WMD) and 95% confidence intervals were calculated where all outcomes were measured using the same scale. Where different scales had been used, the Standardised Mean Difference (SMD) and 95% confidence intervals were calculated. The principal continuous measure used in all the modern trials was the Impact of Events Scale (IES) (Horowitz 1979). This is the most used measure of the impact of trauma in current research work. Chi squared statistic and I squared statistics were calculated to assess statistical heterogeneity.

RESULTS

Description of studies

Included trials
Fifteen trials are included in this review (Bisson 1997; Bordow 1979; Bunn 1979; Campfield 2001; Conlon 1999; Dolan; Hobbs 1996; Lavender 1998; Lee 1996; Litz 2004; Priest 2003; Rose 1999; Sijbrandij 2002; Small 2000; Stevens 1996); four of these have been included as part of this review update (Campfield 2001; Litz 2004; Priest 2003; Sijbrandij 2002). There were no disagreements between reviewers about trials to be included.

Description of study design
All trials were described as 'randomised'. Lee 1996 used alternate number allocated by the nurse recruiting the subjects. Thus, for the purposes of this review, this study was regarded as quasi-randomised.

Patient selection
The majority of these trials involved populations that were reasonably comparable. Most involved those admitted to hospital following trauma (Bisson 1997; Dolan; Hobbs 1996; Lee 1996; Bordow 1979; Stevens 1996), or attending trauma clinics (Sijbrandij 2002; Campfield 2001) or attending casualty (Conlon 1999). Rose 1999 recruited subjects via the local police and medical services. One study (Litz 2004) involved soldiers deployed on a peacekeeping mission. Most studies involved an excess of males, reflecting the epidemiology of trauma, although this was not the case with Dolan (unpublished trial) where there was a predominance of females. Three studies involved obstetric populations (Lavender 1998; Priest 2003 and Small 2000 - see Comparisons available for meta-analysis below). One trial, Bunn 1979, involved a completely different population who were parents or relatives of primary victims of trauma, rather than the primary victims themselves.

Cultural setting
Seven studies were undertaken in the United Kingdom (Bisson 1997, Dolan, Hobbs 1996, Lavender 1998, Lee 1996, Rose 1999, Stevens 1996); one in Ireland (Conlon 1999); one in the Netherlands (Sijbrandij 2002); five in Australia (Campfield 2001, Bordow 1979, Bunn 1979, Priest 2003, Small 2000); and one in the USA (Litz 2004).

Sample size
The number of patients randomised in the trials ranged from 30 to 1,745.

Time interval
All were single session interventions. Most took place shortly after the event (within 24 hours - Stevens 1996; within 48 hours -

Hobbs 1996, Lavender 1998, Small 2000; within 72 hours - Priest 2003; within 1 week - Bordow 1979; within 2 weeks - Lee 1996, Bisson 1997, Conlon 1999, Dolan (unpublished trial), Sijbrandij 2002; within 1 month - Rose 1999. The time period for Bunn 1979 was unclear, but was probably one day. Campfield 2001 compared immediate debriefing (less than 10 hours) with delayed debriefing (more than 48 hours). On the information currently available, the exact time between trauma and intervention in the Litz 2004 trial is unclear, but is known to be within 1 month.

Comparisons available for meta-analysis

Nine of the fifteen trials involved comparable populations and interventions and provided usable data for meta-analysis, enabling three comparisons, as follows:
Debriefing versus Control (Bisson 1997, Conlon 1999, Dolan, Hobbs 1996, Lee 1996, Rose 1999, Sijbrandij 2002, Stevens 1996); Debriefing versus Educational intervention (Rose 1999); Immediate debriefing versus delayed debriefing (Campfield 2001). The remaining trials were either not comparable with the other trials (due to clinical and methodological differences), or they did not provide sufficient data to be included in the meta-analysis.

One trial, Litz 2004, compared Critical Incident Stress Debriefing (CISD), Stress Education and Survey only. However, this was a cluster randomised trial, randomising platoons of soldiers to each intervention. Because individuals in one group may be more similar to each other than to individuals in other groups, the "effective sample size" is less than the number of participants. Therefore, if it were to be included in a meta-analysis as if it were an individually randomised trial, its sample size will be overestimated, it will be given too much weight and the overall estimate's confidence intervals will be too narrow. Methods for including cluster-randomised trials in meta-analyses are not routinely implemented in RevMan and The Cochrane Handbook Section on cluster-randomised trials is still being developed. The data cannot be included as part of this update, but it is hoped that we will be able to include it in an update in Issue 3, 2005. In the interim, the results are described.

Three studies (Lavender 1998, Priest 2003, Small 2000) were undertaken in an obstetric population and even within that two different birth populations. Lavender 1998 and Priest 2003 included only normal cephalic births, while Small 2000 only included operative deliveries. Furthermore, Lavender 1998 involved a high proportion of single mothers (of the total sample, 68 were single compared with 43 who were married). This study also reported an extremely high level of psychological morbidity in the control group, with half displaying worrying high anxiety and over half reporting high depression scores (>11) on the HADS. Given the likely differences between these three trials and the remaining 12, in terms of the participants and interventions involved, these trials do not contribute to the meta-analyses in this review. Since the original reviewers included them, for the sake of completeness, these studies have been included and summarised in this update. However, the intention is to remove these three trials and one other newly published trial that is currently awaiting assessment (Gamble 2005), and incorporate them into a separate review.

Two other trials (Bunn 1979 and Bordow 1979) did not appear to be comparable with the other studies in the review. These were older studies which tested an intervention that, although it appeared to fulfil the criteria outlined in the review protocol, was designed before the current formulations of debriefing. Bunn 1979 involved the relatives of victims, who might be considered "second level" victims. Furthermore, outcomes in this trial were measured only minutes after the intervention. The analysable data in Bordow 1979 compares brief with prolonged treatment, and has no placebo/non intervention arm. Neither Bordow 1979 nor Bunn 1979 used modern outcome instruments. Furthermore, these studies scored lowest methodological quality. Given the differences and limitations, the data from these two trials do not contribute to the meta-analyses in this review.

Excluded trials

These included non randomised design (Carlier, Chemtob 1997, Deahl 1994, Deahl 2000, Foa 1995a, Hytten 1989, Kenardy 1996a, Richards 2001, Resnick 1999, Robinson 1993, Matthews 1998, McFarlane 1988, Saari 1996, Amir 1998), not satisfying criteria for debriefing (Doctor 1994; Greenberg 1996, Polak 1975, Viney 1985); more than a single session intervention (Andre 1997, Brom 1993, Bryant 1998, Doctor 1994); treatment started too late (Brom 1993) or too early (Tadmor 1987).

Risk of bias in included studies

Quality Assessment

Methodological quality was rated independently by each reviewer.

Quality Assessment 1:
The first rating of quality used the methods described in Cochrane Collaboration Handbook.
Category A (adequate) is where the report describes allocation of treatment by any of the following procedures:
(i) some form of centralised randomised scheme, such as having to provide details of an enrolled participant to an office by phone to receive the treatment group allocation;
(ii) some form of randomisation scheme controlled by a pharmacy;
(iii) numbered or coded containers;
(iv) an on-site or coded computer system;
(v) if assignment envelopes were used, the report should at least specify that they were sequentially numbered, sealed, opaque envelopes.
Category B (intermediate) is where the report describes allocation of treatment by:
(i) use of a "list" of "table" to allocate assignments;
(ii) use of "envelopes" or "sealed envelopes";
(iii) stating the study as "randomised" without further detail.
Category C (inadequate) is where the report describes allocation of treatment by:
(i) alternation;
(ii) reference to case record numbers, dates of birth, day of week etc
(iii) any allocation procedure that is entirely transparent before assignment.

Six trials (Bisson 1997, Lavender 1998; Priest 2003; Rose 1999; Sijbrandij 2002; Small 2000) had adequate allocation concealment (computer generated random numbers/opening consecutively numbered sealed opaque envelopes/centralised telephone randomisation); 3 had intermediate (Stevens 1996; opaque envelopes) Dolan (unpublished trial; sealed envelope method) and Hobbs 1996. For the remaining trials, allocation concealment was either unsatisfactory or unclear.

Quality Assessment 2:

The studies were also rated using the CCDAN quality rating scale (Moncrieff 2001), where the maximum score is 46. Differences were resolved by discussion. Ratings are 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, outcomes measures, intention to treat, presentation of results, type of data presented, statistical analysis and control of baseline differences. Scores ranged between 8 and 38 (Sijbrandij 2002 - 38; Priest 2003 - 36.5; Rose 1999 - 27; Small 2000 - 24; Bisson 1997 - 23; Litz 2004 - 23; Conlon 1999 - 21; Campfield 2001 - 19; Dolan - 16; Lavender 1998 - 16; Hobbs 1996 - 15; Lee 1996 - 14; Bordow 1979 - 11; Stevens 1996 - 10; Bunn 1979 - 8)

Quality Assessment 3:
Finally, a quality measure developed specifically for studies of debriefing was used (Kenardy 1996a). This suggests that specific quality criteria include:

a) clear definition of the population to receive the intervention
*nature and extent of the exposure
* time since exposure
* premorbid vulnerability characteristics
* age, gender, other relevant demographic characteristics

b) delineation of appropriate goals of the debriefing. Possibilities include
* imparting information as to the nature of stress responses and their "normalisation"
* imparting information regarding what criteria indicate a need for specialist assistance and where to get it
* developing a sense of belonging with those of "shared" experience
* prevention of PTSD symptoms/signs or other symptoms/signs of relapse
* relief of PTSD/other symptoms/signs
* prevention or improvements in levels of disability linked to the stressor (eg absenteeism, family difficulties etc)
* perceived helpfulness

c) randomisation

d) use of both self report and objective assessments, the latter performed by a rater blind to debriefing condition, to obtain baseline measures of the phenomena which constitute the goals of the debriefing, employing instruments of demonstrable reliability and validity

e) thorough description of the debriefing procedures, ensuring that:
* they are compatible with the specified goals of the debriefing
* personnel conducting the debriefing are adequately trained in the procedure
* quality-control measures adequate to ensure that the debriefing is delivered (in a manual)
* the amount of exposure to debriefing is constant and delivered over a constant period

f) obtain outcome measures at times post debriefing that are regarded as appropriate given the nature of the target problems and the nature of the intervention, again using a combination of self-report and objective measurement by a rater blind to debriefing condition.

We developed a quantitative version of the variables suggested by Kenardy 1996a. The maximum score was 26. Disagreements were resolved by discussion (SW and JB). The ratings ranged from 8 to 22, with a median score of 14. See Table 1 for trial scores.

The differences between the more general Moncrieff 2001 and the specific Kenardy 1996a scales reflect that fact that the Moncrieff 2001scale emphasises general methodological issues relevant to all clinical trials, with a particular emphasis towards pharmacological trials, albeit relevant to psychiatry. The Kenardy 1996a scale gives more weight to specific issues concerning debriefing, and in particular the content of debriefing.

The studies were then ranked in quality order. One obstetric study (Small 2000) scored highly on the Moncrieff 2001scale because of its robust methodology, but scored lower on the Kenardy 1996a scale because of lack of consistency on the debriefing intervention. Indeed the content of the 'patient led' debriefing described in the two obstetric papers (Lavender 1998; Small 2000) makes comparison with the other studies problematic. It was decided that the Kenardy 1996a ratings should be used for the final ranking since it was specifically designed for trials of debriefing. These are provided against all trials contributing data to the meta-analyses.

Overall, methodological quality of the included studies was variable. This was partly due to incomplete data recording. Most gave reasonable information on a priori objectives, and source of sample. Information on allocation concealment was provided for Bisson 1997, Stevens 1996, Priest 2003, Small 2000, Lee 1996, Lavender 1998 and Rose 1999. Information on numbers/reasons for withdrawal was given in six trials. Bisson 1997, Conlon 1999, Priest 2003, Sijbrandij 2002 included an assessor blind to intervention. Stevens 1996 excluded individuals who displayed "undue distress" during the intervention, which may have introduced significant bias, whilst Hobbs 1996 did the opposite by excluding those without any psychological symptoms, thus also introducing bias.

Effects of interventions

Debriefing versus control

PTSD

Diagnosis - No significant differences were observed between debriefing and control at up to 3 months (OR 0.58 (95%CI 0.10 to 3.26)), 3-6 months (OR 1.17 (95%CI 0.70 to 1.98)) and 6-12 months (OR 0.93 (95% CI 0.35 to 2.46)). A significant difference in favour of the control arm was identified at 13 months (OR 2.51 (95%CI 1.24 to 5.09) - based on one study). No significant statistical heterogeneity was observed for any time-point.

Severity (self-report) - No significant differences were observed between debriefing and control at up to 1 month (SMD 0.12 (95%CI -0.08 to 0.32)) and 1-4 months (SMD 1.11 (95%CI -0.10 to 0.32)). A borderline difference in favour of the control arm was observed at 6-13 months (SMD 0.26 (95% CI 0.01 to 0.50)) and no difference was observed at 3 years (SMD 0.17 (95%CI -0.34 to 0.67)). There were no significant differences in self-reported PTSD symptoms at 1-4 months (based on one study only) or in clinician rated PTSD severity at 3 months based on one study only). No significant statistical heterogeneity was observed for any time-point.

Depression
Diagnosis - There were no significant differences at either 0-1 month or 2-5 months (both based on one study only).

Severity - No significant differences were observed at 0-1 month (SMD 0.01 (95%CI -0.33 to 0.34)), 1-4 months (SMD 0.00 (95%CI -0.27 to 0.26)), and a borderline difference in favour of the control arm was observed at 6-13 months (SMD 0.33 (95%CI 0.09 to 0.58)). No significant statistical heterogeneity was observed for any time-point.

Anxiety
Diagnosis - There were no significant differences at either 0-1 month or 2-5 months (both based on one study only).

General anxiety - There were no differences at 0-1 month (SMD 0.00 (95%CI -0.33 to 0.33)), 1-4 months (SMD 0.03 (95%CI -0.23 to 0.29)) or 6-13 months (SMD 0.25 (95%CI -0.05 to 0.55)). No significant statistical heterogeneity was observed for any time-point.

Travel anxiety - No difference was found at 2-5 months (based on one study only).

All psychiatric morbidity
No significant differences were observed at 0-1 month or 2-5 months (both based on one study only).

Reduced functioning
No significant differences were observed at 3 months (based on one study only).

Dropout
A significant difference in favour of the control arm was observed (OR 1.97 (95%CI 1.23 to 3.15)). No significant statistical heterogeneity was observed.

Debriefing versus educational intervention
PTSD
Diagnosis - No significant difference was observed at 6 months (based on one study only).

Severity (self-report) - No significant difference was observed at 6 months (based on one study only).

Depression
Severity - No significant difference was observed at 6 months (based on one study only).

Dropout
No significant difference was observed at 6 months (based on one study only).

Immediate debriefing versus delayed debriefing
PTSD
Severity (self-report) - A significant difference in favour of immediate debriefing (<10 hours after trauma) was observed (WMD -26.16 (95%CI -30.59 to - 21.73) - based on one study only).

Additional trial summaries
The data from two new trials have not yet been included in these meta-analyses. The findings of each are summarised below. Since neither study found an effect for debriefing, the inclusion of data from these studies are not expected to change the conclusions of this review.

Litz 2004, a cluster randomised trial involving group debriefing of soldiers on a peacekeeping mission, has not yet been included in these meta-analyses. This trial randomised to 1,050 from 19 platoons into 62 groups for three conditions; Debriefing (23 groups), Stress Education (20 groups) and No intervention (19 groups). Formal CISD was applied by trained professionals and the sessions were taped to check the reliability of interventions. Participants were followed up post-group and at 3 and 9 months. Litz 2004 report no differences between groups on all behavioural outcomes (Personal communication). We expect to be able to include data from this trial when updating this review for Issue 3, 2005.

Sijbrandij 2002, a 'dismantling' study of debriefing, randomised 236 participants within 2 weeks of a traumatic event, to one of three conditions; Emotional debriefing (N=76), Educational debriefing (N=79), or Control (N=81). Participants were followed up at 2 weeks, 6 weeks, and 6 months. The authors report that psychiatric symptoms decreased in all three groups over time, and that there were no significant differences between groups on symptoms of PTSD, anxiety or depression. Since the two 'active' interventions both involve integral components of debriefing versus control, we are consulting with a statistician about how both arms might be included in our first comparison (Debriefing versus Control). We hope to include these data in the meta-analysis when updating this review for Issue 3, 2005.

DISCUSSION

1. Quantitative findings
There is no evidence that debriefing reduces the risk of developing PTSD. At no time does any study suggest a significant reduction in IES in those receiving the intervention. On the other hand, the trials with the longest follow up (Hobbs 1996; Bisson 1997) both reported adverse effects. Results from the 3 year follow-up of Hobbs 1996 showed that follow-up participants (n=61) had been more severely injured at outset although there was no significant differences in terms of overall demographics and initial emotional response to the accident. The intervention group at 3 years had a significantly worse outcome of those with high original IES scores(>24 t(14) =2.56, p .23). There was no difference at 3 year follow-up of those with low initial IES scores. Results indicated that the negative effects of the intervention on patients with high initial IES scores were already present at 4 months post intervention and this was maintained at follow-up. This study shows that those at most risk of developing PTSD and other poor psychological outcomes are unlikely to be helped by a single PD session and indeed such an intervention may be harmful. However, although attrition was broadly similar between the control and treatment group it was high and conclusion from this study should therefore be limited. Bisson 1997 measured outcome at 13 months. This trial reported considerable variance in the data and differential loss to follow up between the treated and control groups. If those who were improved were less likely to remain in contact, then this may have introduced bias. Thus, the exact magnitude of the adverse effect is open to question. However, in the only 2 long-term studies identified to date, debriefing would appear to have increased long term traumatic distress. There is also no evidence that debriefing has any effect on any other psychological outcome, including depression, anxiety or general functioning. Although the confidence limits for dichotomous outcomes are wide and include

the possibility of both a positive and negative effect of treatment, the interpretation of no effect is supported by the studies which report continuous data. These data also demonstrate no effect of debriefing on broader outcomes. Comparing debriefing with an educational interventions produced similarly equivocal results on all outcomes (Rose 1999). There is evidence from one trial (Campfield 2001) suggesting a possible effect of timing on the outcome of debriefing.

2. Clinical and statistical heterogeneity
There were insufficient studies to undertake any formal sub-group analyses to explore potential sources of heterogeneity. However, the trials contributing data to this review used a similar intervention, the majority involved similar types of participants (in terms of trauma), and all come from similar cultural settings (United Kingdom & Ireland). Furthermore, the Chi square and I square tests of heterogeneity identified no evidence of ststatistical heterogeneity.

One possible exception was the study of Lee 1996, which reported substantially higher IES scores than other studies. This study was of women recovering from spontaneous miscarriages, and since miscarriages are associated with temporary high psychological morbidity (Friedman 1989), this may explain the observed differences.

Due to insufficient data, it was not possible to examine the potential influence of publication bias using a funnel plot. However, it should be noted that this review has been successful in identifying and acquiring unpublished data, which should at least partially address such concerns.

Comparison with other data sources
Some may be continued to be surprised by the lack of evidence of the efficacy of debriefing, given there are many positive uncontrolled studies of the efficacy of debriefing. However, the possibility that early psychological intervention for the victims of trauma might be ineffective has also been suggested in the literature prior to this review or its update. Non randomised studies of debriefing also exist that suggest a negative effect (ex Carlier), but are outside the scope of this review. Another related area is psychological intervention in schools following the suicide of a classmate, known as postvention. No randomised trials exist - the most recent assessment also noted a negative effect (Callahan 1996).

Crisis intervention has been excluded from this review. Crisis intervention predates the development of psychological debriefing, but is a strong influence upon it. The closest to modern formulations of debriefing appears to be the "person centered cathartic approaches" used by Polak and colleagues. A short term study showed no effect of intervention (Polak 1975), whilst the 18 month outcome indicated an adverse effect on bereavement (Williams & Polak, 1979).

Why might treatment have failed?
1. Were the interventions too short? This would not explain why treatment appeared to have an adverse effect on the IES scores, unless one postulates that the intervention lead to an increase in psychological distress by virtue of re exposure to the traumatic event, but without allowing time for habituation to occur. This "secondary trauma" argument will be discussed further. On the other hand, four studies that used more than a single session (Foa 1995a, Andre 1997and Bryant 1998a, Bisson et al. in press) do report a beneficial effect of CBT treatment. A more suitable strategy may be to target vulnerable individuals and give them more intensive interventions such as highlighted by Foa 1995a; Andre 1997; Bryant 1998, and Bisson et al. in press. It appears that there is an important role in acute stress disorder predicting the later onset of chronic PTSD (Bryant 1998; Bryant 1998; Brewin at al. 1999; Bisson et al, in press).

2. Was follow up too short? It is possible that longer follow up might have revealed more benefits to the treated group, but in the 2 longest trials (Hobbs 1996; Bisson 1997) differences between treated group and controls were widening over time.

3. Was randomisation ineffective? The vagaries of randomisation and/or inadequate allocation concealment meant that the treated group in the Bisson 1997 trial had significantly more initial trauma (as assessed by % burn and subjective life threat), whilst the treated group in the Hobbs 1996 trial also showed a higher mean injury score. On the other hand, adjustment for initial distress made no difference to the results of the burns unit study (Bisson 1997). When analysis of co variance using the presence and absence of debriefing and initial distress was performed, initial distress was a far stronger predictor of poor outcome than the presence or absence of debriefing.

4. Was the timing of the intervention wrong? It may be that more time is needed to allow physical recovery from the trauma before embarking on a psychological intervention. However, Campfield 2001 found greater benefit from immediate debriefing (<10 hours) than from delayed debriefing (>48 hours), whilst the two individual studies that reported an adverse outcome for debriefing, both gave the intervention close to the trauma. Lee 1996 and Rose 1999 found no neutral effects of treatment having given their intervention two weeks and three weeks, respectively, after the event.

5. Has the wider culture changed rendering debriefing unnecessary? There can be little doubt that awareness of the possible adverse psychological effects of trauma has altered over the years, at least in Western cultures. The randomised trials cited in this review are all relatively recent. It is therefore possible that the general themes underlying debriefing are now part of the accepted culture - hence there is sufficient general awareness of "psychological first aid", ether by the person themselves or their family and friends, that everybody experiences a "bit of debriefing" anyway, thus reducing the possibility of showing any effects from a formal intervention.

6. Why might treatment have an adverse effect? There are a number of possible reasons why debriefing might be associated with an adverse effect in some. Some might find it difficult to accept any adverse effect of treatment. However, it is a general finding that any effective treatment, even psychological treatments, must always carry a risk of adverse effects in some - the question at issue is always the balance of risk and effects. In has been argued that debriefing may carry benefits in terms of the management of traumatic incidents, rather than mitigating trauma symptoms, and that organisations need to think carefully about the objectives of continuing to use debriefing without having very clear and realistic aims and understanding the need to properly evaluate outcomes (Rick 2000a).

There are also some reasons why debriefing might have a specific adverse effect in some. There is the possibility of "secondary traumatisation". Debriefing involves intense imaginal exposure to a traumatic incident within a short time of the event. It is possible that in some individuals this serves as a further trauma, exacerbating their symptoms without assisting in emotional processing. Exposure therapy, as practiced for the treatment of established PTSD, may lead to an initial mild excerbation of symptomatology as distressing images are recollected. The principles of exposure therapy suggest that such distress lessens as habituation occurs over time. However, in a single intervention as reviewed here, such habituation may not occur unless the recipient engages in further self directed exposure. Another possible adverse reaction to PD could be hypothesised in those with a sense of shame as a reaction to the traumatic event. While there is no direct evidence that shame is implicated in the in the onset or course of PTSD there is some evidence that it is of predictive importance (Andrews 2000). It can however be hypothesised that those with a sense of shame might be more likely to experience some exacerbation of distressing symptoms when undertaking a verbal exposure to the event, particularly when the shame and/or the underlying reasons remain undisclosed. It would appear that aspects of shame in relation to the traumatic event can range from the relatively straightforward shame of modifiable behaviour e.g.such as suffering incontinence of urine/faeces on impact to the more complex characterlogical self blame (Janoff-Bulman 1992; Gold 1986). It could therefore be argued that undertaking interventions such as PD with those who are suffering from shame based reactions is contraindicated but it is difficult to see how a shame based reaction could be elicited without a skilled, attuned and sensitive therapist. It may however, indicate that a 'safer' way of handling early psychological interventions is to elicit a client led narrative without insisting on a clinician led re-exposure to the event. Clearly, more research is needed in this area.

Another explanation is that debriefing may 'medicalise' normal distress. It may also increase the expectancy of developing psychological symptoms in those who would otherwise not have done so. No matter how great the trauma, it is a constant finding of the traumatic stress literature that not everyone develops psychological distress, and it is usually only a minority who progress to formal long term psychiatric disorder. Debriefing, by increasing awareness of psychological distress, may paradoxically induce that distress in those who would otherwise not have developed it.

Debriefing also assumes that there is a uniform, and to a certain extent predictable, pattern of reactions to trauma. At the heart of the treatment is the concept that discussing the trauma is therapeutic, and that attempting to deny it is not. This is based on a 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' - attempting to forget/distance oneself may be an adaptive response. Intervention may interfere with adaptive defence mechanisms.

A further problem is that debriefing, by definition, focuses on the single trauma. However, even if all the victims of a disaster were exposed to a uniform event, they are certainly not uniform in any other respect. Focusing attention on the single traumatic event may divert attention away from other important psychosocial, non traumatic, factors that differ between victims.

AUTHORS' CONCLUSIONS

Implications for practice

1. At present the routine use of single session individual debriefing in the aftermath of individual trauma cannot be recommended in either military or civilian life. The practice of compulsory debriefing should cease pending further evidence. Even if further large scale trials do reveal a positive effect of debriefing that has not been detected in the trials to date, the evidence reviewed above suggest the likely treatment effect will be small.

2. We are unable to comment on the use of group debriefing, nor the use of debriefing after mass traumas. We are also unable to make recommendations about the use of debriefing in children.

3. It appears appropriate to continue to focus resources on identifying and treating those with recognisable psychiatric disorders arising after trauma, such as acute stress disorder, depression and PTSD. Emphasis should increasingly be placed on the early detection of those at risk of developing psychopatholgy and early interventions should be aimed at this group. Follow-up assessment should increasingly viewed as important and the use of screen and treat programmes should be increasingly developed (NICE 2005). The Psychological First Aid Model (Freeman in press) may offer an alternative approach, although clearly this needs evaluation. This model proposes an individually tailored response that encompasses practical and social support, any discussion of the event is again respondent led, use of a follow-up and, where necessary, appropriate referral to a mental health professional.

4. In terms of using the principles of evidence based practice where psychosocial interventions are used, even when (especially when) associated with clear need, high face validity and client satisfaction these should not be regarded as a substitute for evidence.

Implications for research

1. There is no information on the response of those with pre existing psychiatric disorder to psychological debriefing, since all studies used known psychiatric disorder as an exclusion.

2. Since the last issue of this review three further trials and a follow-up have been reported, but there remains a continuing need for more randomised studies. Three areas are a particular priority. First, the efficacy of debriefing in emergency workers. Second, the efficacy of group, as opposed to individual, debriefing. Third, the efficacy of debriefing after mass disasters/traumas, although it is accepted that such studies will be difficult to undertake. Currently the reviewers are not aware of the evidence base surrounding debriefing in children.

3. There is a need towards working with predictive questionnaires with differing populations to highlight those 'at risk' (e.g. Brewin 2003, NICE 2005).

4. At present the reviewers are aware of several ongoing RCTs, the results of which will be incorporated into this review as soon as they are available.

5. There are now four published trials of longer interventions (Foa 1995a, Andre 1997, Bryant 1998a, Bisson et al, in press). Preliminary information suggests that delivering more formalised interventions

over a longer period of time and aimed at those with overt distress may be worthwhile.

The results of this review contrast with the evidence for the effectiveness of psychological treatments in the management of several psychiatric disorders. Treatments that are effective in those with established disorder cannot be assumed to be effective in prevention, and the possibility of adverse effects must be remembered.

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* Indicates the major publication for the study

CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

Bisson 1997

Methods	Randomisation: randomised numbers generated by computer

Bisson 1997 *(Continued)*

	Allocation concealment: A Exclusion after randomisation; yes ITT: no
Participants	Setting: Burns Unit. Inclusion: Consecutive admissions to Burns Unit Exclusion: Major psychiatric or physical disorder
Interventions	Comparison: Psychological debriefing (Mitchell model) versus questionnaire only Time between event and intervention: 2 - 19 days
Outcomes	PTSD scale IES HADS
Notes	Assessor blind to intervention-- yes; Intervention standardised;--yes: No intention to treat; data provided on study completers only large SDs on IES

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	Low risk	A - Adequate

Bordow 1979

Methods	Randomisation: First 30 non randomly assigned to waiting list control: Next 40 allocated by 'random preset order' to brief or extended intervention Allocation concealment: not stated (B) Exclusion after randomisation: no ITT; yes (probably - no formal data on follow up)
Participants	Male inpatients after road traffic accidents
Interventions	Comparison: Extended (minimal emotional support (1 hr) + practical and social support (max 10 hrs) versus Brief "minimal emotional support" (1hr) Time between event and intervention: up to 1 wk
Outcomes	Langer 22 Item Work Adjustment Traumatic Neurosis Symptoms Pleasant and Unpleasant experiences Health deterioration
Notes	Trial of brief versus extended therapy (no randomly allocated control condition) No standard deviations for continuous measures; no cut offs for categorical measures. Not included in meta analysis

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	High risk	C - Inadequate

Bunn 1979

Methods	Randomisation: 'Randomly assigned' Allocation concealment - unclear (B) Exclusion after randomisation - unclear ITT; yes, but no follow up
Participants	Parents or relatives of primary victims of trauma admitted to a general hospital. Exclusions: frequent attenders
Interventions	Comparison: 20 minutes counselling versus nil Time between assessment and intervention: unclear, but probably hours or a few days
Outcomes	Gottschalk & Gleiser content analysis of anxiety (six categories) Viney and Westbrook cognitive anxiety
Notes	Assessment took place within minutes of end of intervention Assessments based on interpretation of five minute verbal samples. Interventions standardised: no Subjects were not primary victims

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	High risk	C - Inadequate

Campfield 2001

Methods	Randomisation: 'Randomly assigned' Allocation concealment: Not reported Exclusion after randomisation: no ITT: yes
Participants	Setting: Trauma Clinic Inclusion: Civilian victims of robbery in the workplace Exclusion: Victims of robberies involving physical injury, guns and those already receiving treatment for the effects of trauma
Interventions	Comparison: Immediate (<10hr) Critical incident stress debriefing (CISD) (Mitchell model) versus delayed CISD (>48 hrs) Time between event and intervention: <10 or >48 hrs
Outcomes	Post-traumatic Stress Diagnostic Scale (PDS)
Notes	Assessor blind to intervention: Not stated Intervention standardised

Campfield 2001 *(Continued)*

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	Unclear risk	D - Not used

Conlon 1999

Methods	Randomisation: coin toss Allocation concealment: C Exclusion after randomisation; no
Participants	Setting: Hospital trauma clinic Inclusion: RTA victims 16 to 65 Exclusion; injuries requiring hospital admission
Interventions	Comparison: 30 minute debriefing versus advice leaflet and telephone number Time between event and intervention: mean 7 days, range 3 to 14
Outcomes	IES Clinician administered PTSD scale
Notes	Assessor blind to intervention: Assessment standardised:

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	High risk	C - Inadequate

Dolan

Methods	Randomisation: Allocation concealment: Unclear Exclusion after randomisation:
Participants	Setting: Hospital trauma clinic Inclusion: those presenting with life-threatening or near life-threatening experiences e.g. RTA, assault, housefirem industrial accident. Exclusions: serious head injury, those too unwell to co-operate, those with no memory of the trauma. Those injured through sports injury, self-harm, DIY, fights or heavy alcohol intoxication at the time.
Interventions	Comparison: Psychological Debriefing (Mitchell/Dyregrov model) versus initial assessment
Outcomes	GHQ-28 HADS IES The Neo-5 Factor Personality Questionnaire The Defence Style Personality Questionnaire

Dolan *(Continued)*

	The Mast Abbreviated Injury Scale and the Injury Severity Score
Notes	Unclear assessor blind to intervention: Intervention standardised

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	Unclear risk	B - Unclear

Hobbs 1996

Methods	Randomisation: Random number table Allocation concealment: not stated Exclusion after randomisation: no ITT: no
Participants	Setting: Hospital Casualty Department Inclusion: Road accident victims Exclusion: unconscious, no memory of accident, no psychological symptoms, discharged before contact Three year follow up undertaken
Interventions	Comparison: debriefing (1 hr) + leaflet to subject and GP versus screening only Time between event and intervention: 1 to 2 days
Outcomes	Brief Symptom Inventory (Global Severity Index: GSI). IES and Distressing intrusive memories (approximation for PTSD) Travel anxiety
Notes	Subjects with no psychological symptoms at assessment excluded. Intervention standardised - yes

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	Unclear risk	B - Unclear

Lavender 1998

Methods	Randomisation: Computer generated Allocation concealment: Adequate Exclusion after randomisation: No
Participants	Setting: Hospital postpartum ward Included: Primigravidas with singleton pregnancies and cephalic presentations who were in spontaneous labout at term and proceeded to normal vaginal delivery of a healthy baby.

Lavender 1998 *(Continued)*

	Excluded: Those with 3rd degree perineal tear, manual removal of the placenta, baby admitted to special care baby unit and women requiring high dependency care.
Interventions	Comparison: interactive interview when women were encouraged to spend as much time as necessary discussing their labour, asking questions and exploring their feelings versus Time between event and intervention:
Outcomes	HADS
Notes	

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	Low risk	A - Adequate

Lee 1996

Methods	Randomisation: Alternate randomisation by odd and even numbers given by nurse recruiting (not the person treating) Allocation concealment: C Exclusion after randomisation: yes ITT: no
Participants	Setting: Gynaecology ward Inclusion: consecutive admissions with first episode of completed miscarriage, aged 18 or over Exclusion: no current psychiatric or psychological disorder
Interventions	Comparison: Psychological debriefing (Dyregov, Mitchell model) of 1 hr versus Questionnaire assessment only Time between event and intervention: 2 weeks
Outcomes	HADS, IES
Notes	Outcome caseness not given by intervention group. No PTSD criteria

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	High risk	C - Inadequate

Litz 2004

Methods	Randomisation: 'Randomly assigned' Allocation concealment: Not yet known Exclusion after randomisation: Not yet known
Participants	Setting: US arm

Litz 2004 *(Continued)*

	Inclusion: Platoons deployed on peacekeeping mission Exclusion: None reported
Interventions	Comparison: Critical incident stress debriefing (CISD) (Mitchell model) versus stress education versus survey only Time between event and intervention: Not reported
Outcomes	Post traumatic stress (PCL); Depression (CES-D), General well-being (GHQ); Aggressive behaviour; Marital satisfaction; Perceived Organisational Support; Morale.
Notes	Assessor blind to intervention: Unclear Assessment standardised:

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	Unclear risk	B - Unclear

Priest 2003

Methods	Randomisation: 'Randomised' Allocation concealment: Adequate Exclusion after randomisation: No
Participants	Setting: Two large maternity hospitale in Perth Inclusion: Women delivered at or near term Exclusion: Insufficient English, already under psychological care, less than 18 years or with infant needing neonatal care
Interventions	Comparison: Standardised debriefing (Mitchell model) versus standard post-natal care TIme between event and intervention: Within 72 hours of delivery
Outcomes	Depression; PTSD using DSMIV
Notes	Assessor blind to intervention: Done but not tested

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	Low risk	A - Adequate

Rose 1999

Methods	Randomisation: computer generated list by statistician Allocation concealment: yes Exclusion after randomisation: no ITT: yes
Participants	Setting: 2161 victims of violent crime identified from police and casualty Inclusion: over 18 Exclusion: domestic violence, living outside study area, more than one month after crime
Interventions	Comparison: Debriefing (Dyregov, Mitchell model):1 hr versus Education only (30 minutes) Control: Assessment only
Outcomes	PSS IES BDI
Notes	Only 11% of those contacted agreed to intervention Time between incident and intervention: max one month. Most outcomes telephone, but also postal and home visits Intervention standardised: yes

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	Low risk	A - Adequate

Sijbrandij 2002

Methods	Randomisation: Randomised using computer Allocation concealment: Adequate Exclusion after randomisation: Yes
Participants	Setting: Trauma outpatient clinic Inclusion: Single traumatic event, 18 years or more and proficient in Dutch Exclusion: suicidal ideation, already treated for effects of trauma
Interventions	Comparison: Emotional debriefing versus the psychoeducational debriefing versus no debriefing Time between event and intervention: Approximately 2 weeks
Outcomes	SI-PTSD HADS-D HADS-A PDEQ
Notes	Assessor blind to intervention:

Sijbrandij 2002 *(Continued)*

	Done but not tested

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	Low risk	A - Adequate

Small 2000

Methods	Randomisation: Computer generated telephone ransomisation Allocation concealment: Adequate Exclusion after randomisation: Unclear
Participants	Setting: 908 women on postnatal ward, large Maternity Hospital, Australia Inclusion: women who had given birth by LSCS, forceps or vacuum extraction. Excluded: women who had not had operative births, stillbirths or those who had babies weighing <1500gms, those with insufficient english, those ill themselves, very ill babies and those whose private obstetrician refused access
Interventions	Comparison: Debriefing 'provided women with the opportunity to discuss labour, birth and post-delivery events and experiences. +pamphlet on sources of other assistance of 1 hour versus Brief visit from midife to give out pamplet.
Outcomes	EPDS SF-36
Notes	No baseline measures

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	Low risk	A - Adequate

Stevens 1996

Methods	Randomisation: 'randomly assigned' Allocation concealment; opaque sealed envelopes (B) Exclusion after randomisation; yes ITT: no
Participants	Casualty attenders after road traffic accident, dog bite or assault Exclusions: non English speakers, not physically fit to be interviewed; need immediate psychiatric referral, homeless, intoxicated
Interventions	Comparison: debriefing versus questionnaires Time between event and intervention; <24 hrs
Outcomes	PTSD (DSM-III). BDI. Spielberger

Stevens 1996 *(Continued)*

	IES
Notes	Losses to follow up not by group. PTSD and other psychiatric disorders grouped together Intention to treat: no Intervention standardised: yes

Risk of bias

Bias	Authors' judgement	Support for judgement
Allocation concealment?	Unclear risk	B - Unclear

BDI: Beck Depression Inventory
IES; Impact of Events Scale
ITT: Intention to treat
HAD: Hospital Anxiety and Depression Scale
PSS: Post-traumatic Stress DIsorder Symptom Scale
PTSD: Post traumatic stress disorder
EPDS: Edinburgh Postnatal Depression Score
SF-36: 36 Item Short-form Health Survey

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Amir 1998	Non randomised group intervention
Andre 1997	Not single session; CBT
Brom 1993	Multiple sessions Time between trauma and intervention > 1 month
Bryant 1998	Sample selected on the basis of acute stress disorder - not a random sample of victims. Intervention four sessions
Carlier	Non randomised
Chemtob 1997	Non randomised. Time between trauma and intervention >1 month
Deahl 1994	Non randomised
Deahl 2000	
Doctor 1994	Intervention not related to traumatic event; Intervention not debriefing (12 sessions of group counselling)
Foa 1995	Non randomised
Greenberg 1996	Not debriefing
Hytten 1989	Non randomised
Kenardy 1996	Non randomised

Study	Reason for exclusion
Matthews 1998	Non randomised
McFarlane 1988	Non randomised
Polak 1975	Crisis intervention, not debriefing
Resnick 1999	Not randomised
Richards 2001	Not an RCT
Robinson 1993	Not randomised
Saari 1996	Non randomised
Tadmor 1987	Pre trauma intervention
Viney 1985	Not debriefing

Characteristics of ongoing studies *[ordered by study ID]*

Alexander 2002

Trial name or title	Alexander 2002
Methods	
Participants	120
Interventions	Psychological Intervention +// Unclear Intervention +
Outcomes	
Starting date	
Contact information	
Notes	

Stallard 2003

Trial name or title	Stallard 2003
Methods	
Participants	276
Interventions	Trauma discussion intervention// Usual Care +//
Outcomes	
Starting date	

Stallard 2003 *(Continued)*

Contact information
Notes

DATA AND ANALYSES

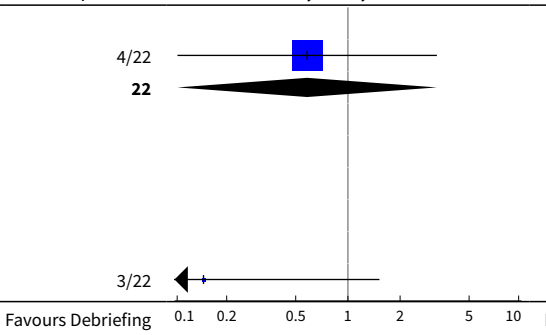
Comparison 1. Debriefing versus Control

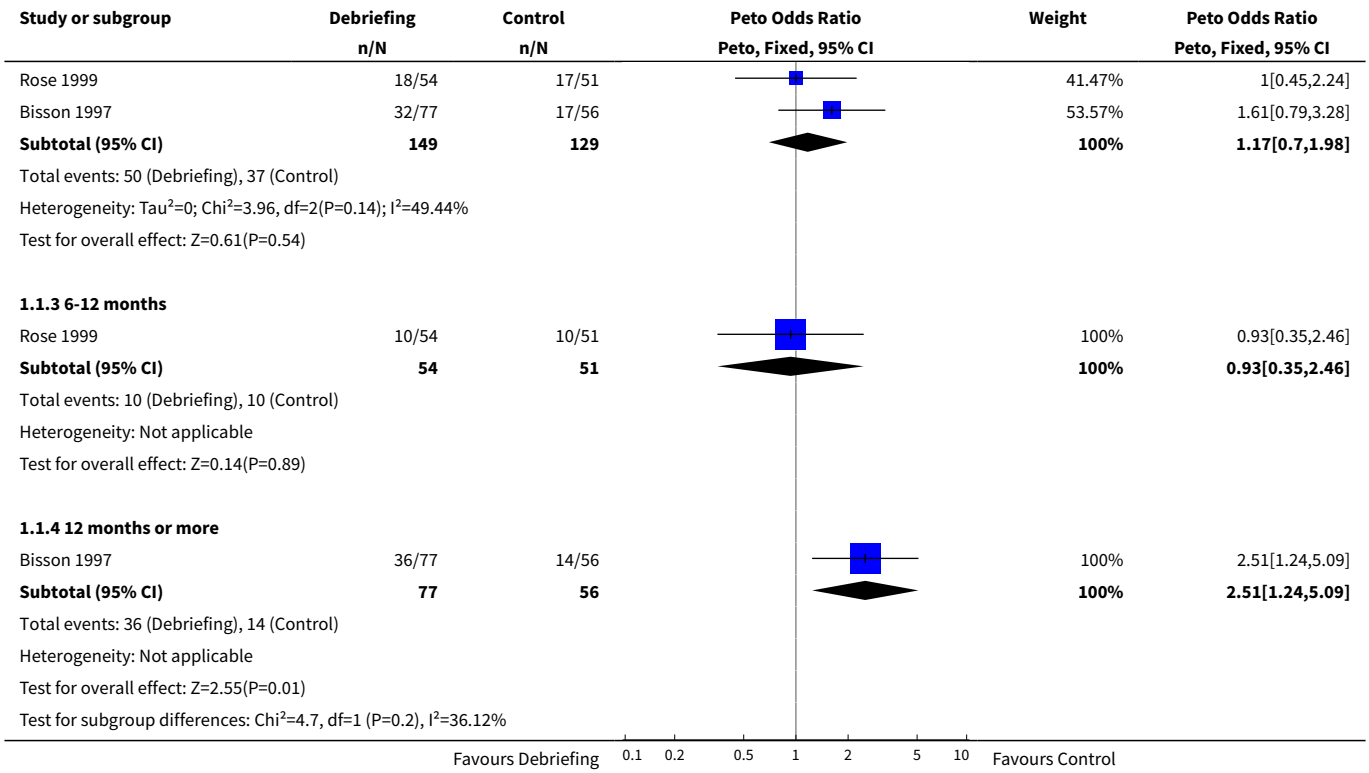
Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 PTSD diagnosis - ITT data	3		Peto Odds Ratio (Peto, Fixed, 95% CI)	Subtotals only
1.1 Up to 3 months	1	40	Peto Odds Ratio (Peto, Fixed, 95% CI)	0.58 [0.10, 3.26]
1.2 3-6 months	3	278	Peto Odds Ratio (Peto, Fixed, 95% CI)	1.17 [0.70, 1.98]
1.3 6-12 months	1	105	Peto Odds Ratio (Peto, Fixed, 95% CI)	0.93 [0.35, 2.46]
1.4 12 months or more	1	133	Peto Odds Ratio (Peto, Fixed, 95% CI)	2.51 [1.24, 5.09]
2 PTSD severity - using self-report measures	6		Std. Mean Difference (IV, Fixed, 95% CI)	Subtotals only
2.1 Initial	5	393	Std. Mean Difference (IV, Fixed, 95% CI)	0.12 [-0.08, 0.32]
2.2 1-4 months	5	356	Std. Mean Difference (IV, Fixed, 95% CI)	0.11 [-0.10, 0.32]
2.3 6-13 months	3	265	Std. Mean Difference (IV, Fixed, 95% CI)	0.26 [0.01, 0.50]
2.4 3 years	1	61	Std. Mean Difference (IV, Fixed, 95% CI)	0.17 [-0.34, 0.67]
3 PTSD severity - clinician rating measures	1		Mean Difference (IV, Fixed, 95% CI)	Subtotals only
3.1 3 months	1	32	Mean Difference (IV, Fixed, 95% CI)	-6.0 [-16.49, 4.49]
4 PTSD - self-reported symptoms	1		Peto Odds Ratio (Peto, Fixed, 95% CI)	Subtotals only
4.1 4 months	1	106	Peto Odds Ratio (Peto, Fixed, 95% CI)	1.84 [0.60, 5.63]
5 Depression diagnosis - completers only	1		Peto Odds Ratio (Peto, Fixed, 95% CI)	Subtotals only
5.1 0-1 month	1	39	Peto Odds Ratio (Peto, Fixed, 95% CI)	1.72 [0.17, 17.75]
5.2 2-5 months	1	39	Peto Odds Ratio (Peto, Fixed, 95% CI)	0.11 [0.01, 1.81]
6 Depression severity	4		Std. Mean Difference (IV, Fixed, 95% CI)	Subtotals only

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
6.1 0-1 month	2	142	Std. Mean Difference (IV, Fixed, 95% CI)	0.01 [-0.33, 0.34]
6.2 1-4 months	3	225	Std. Mean Difference (IV, Fixed, 95% CI)	-0.00 [-0.27, 0.26]
6.3 6-13 months	3	265	Std. Mean Difference (IV, Fixed, 95% CI)	0.33 [0.09, 0.58]
7 Anxiety diagnosis	1		Peto Odds Ratio (Peto, Fixed, 95% CI)	Subtotals only
7.1 0-1 month	1	39	Peto Odds Ratio (Peto, Fixed, 95% CI)	0.79 [0.22, 2.89]
7.2 2-5 months	1	39	Peto Odds Ratio (Peto, Fixed, 95% CI)	1.71 [0.43, 6.79]
8 General Anxiety	3		Std. Mean Difference (IV, Fixed, 95% CI)	Subtotals only
8.1 0-1 month	2	142	Std. Mean Difference (IV, Fixed, 95% CI)	-0.00 [-0.33, 0.33]
8.2 1-4 months	3	225	Std. Mean Difference (IV, Fixed, 95% CI)	0.03 [-0.23, 0.29]
8.3 6-13 months	2	172	Std. Mean Difference (IV, Fixed, 95% CI)	0.25 [-0.05, 0.55]
9 Travel anxiety	1		Peto Odds Ratio (Peto, Fixed, 95% CI)	Subtotals only
9.2 2-5 months	1	106	Peto Odds Ratio (Peto, Fixed, 95% CI)	1.12 [0.50, 2.53]
10 All psychiatric morbidity	1		Peto Odds Ratio (Peto, Fixed, 95% CI)	Subtotals only
10.1 0-1 month	1	63	Peto Odds Ratio (Peto, Fixed, 95% CI)	2.24 [0.70, 7.19]
10.2 2-5 months	1	63	Peto Odds Ratio (Peto, Fixed, 95% CI)	0.70 [0.24, 2.08]
11 Reduced Functioning	1		Peto Odds Ratio (Peto, Fixed, 95% CI)	Subtotals only
11.1 3 months	1	103	Peto Odds Ratio (Peto, Fixed, 95% CI)	1.53 [0.59, 3.92]
12 Dropout	4	444	Peto Odds Ratio (Peto, Fixed, 95% CI)	1.97 [1.23, 3.15]

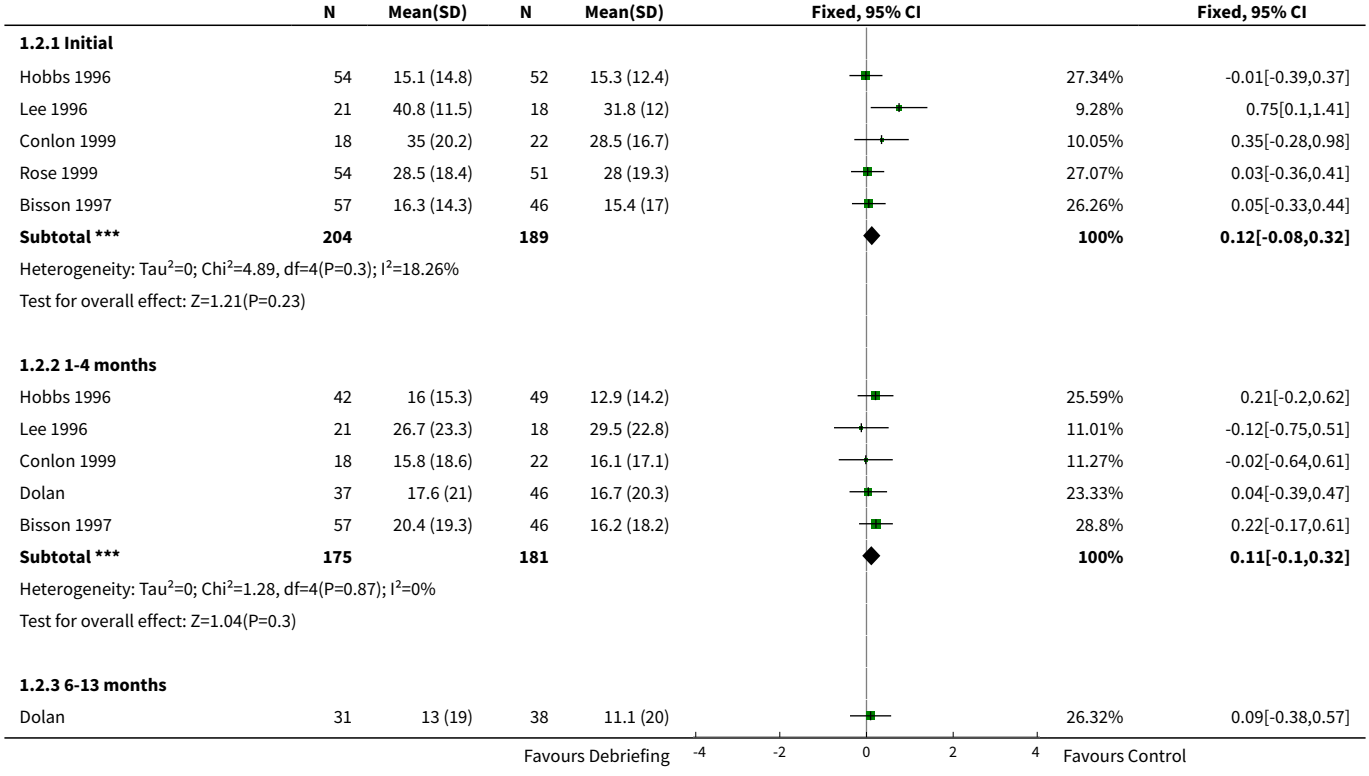
Analysis 1.1. Comparison 1 Debriefing versus Control, Outcome 1 PTSD diagnosis - ITT data.

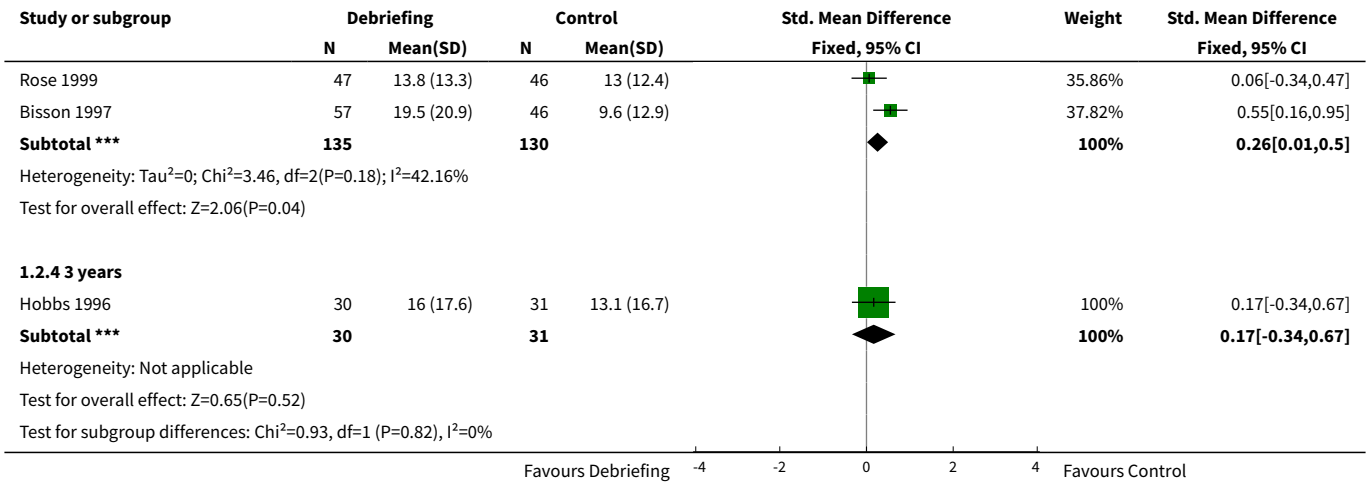
Study or subgroup	Debriefing n/N	Control n/N	Peto Odds Ratio Peto, Fixed, 95% CI	Weight	Peto Odds Ratio Peto, Fixed, 95% CI
1.1.1 Up to 3 months					
Conlon 1999	2/18	4/22		100%	0.58[0.1,3.26]
Subtotal (95% CI)	**18**	**22**		**100%**	**0.58[0.1,3.26]**
Total events: 2 (Debriefing), 4 (Control)					
Heterogeneity: Not applicable					
Test for overall effect: Z=0.62(P=0.54)					
1.1.2 3-6 months					
Conlon 1999	0/18	3/22		4.96%	0.15[0.01,1.52]



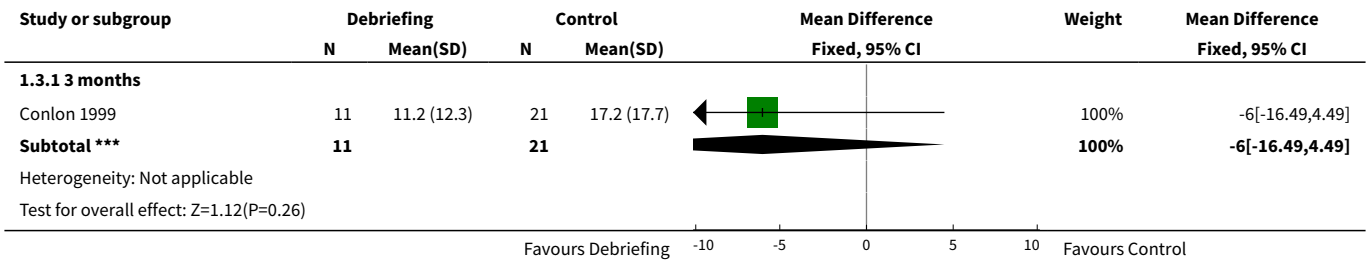


Analysis 1.2. Comparison 1 Debriefing versus Control, Outcome 2 PTSD severity - using self-report measures.

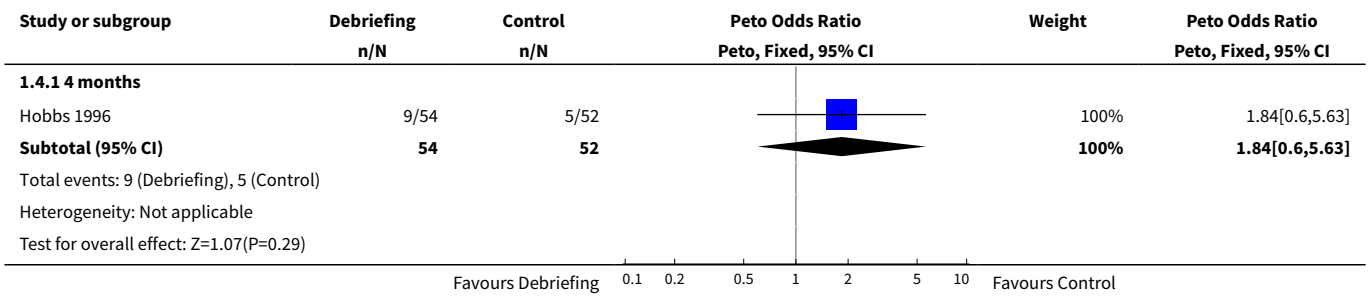




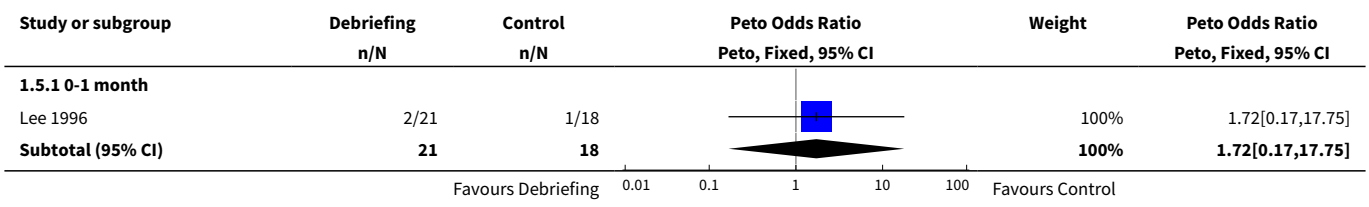
Analysis 1.3. Comparison 1 Debriefing versus Control, Outcome 3 PTSD severity - clinician rating measures.

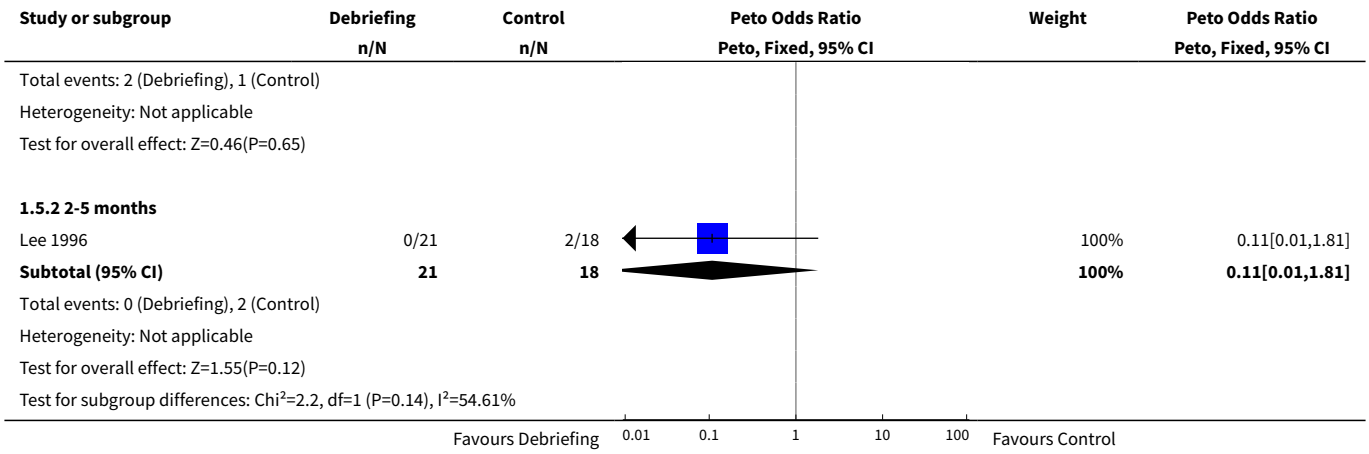


Analysis 1.4. Comparison 1 Debriefing versus Control, Outcome 4 PTSD - self-reported symptoms.

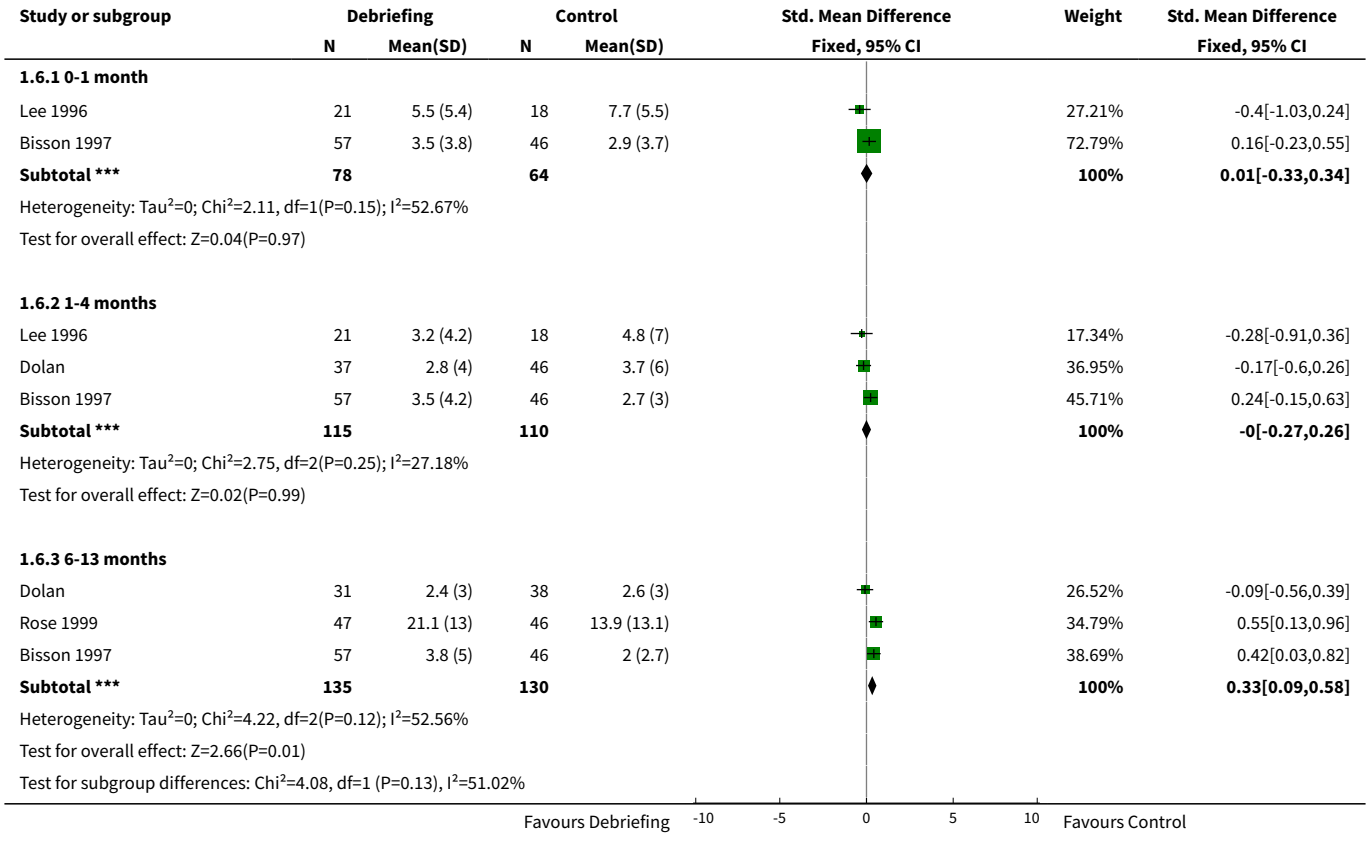


Analysis 1.5. Comparison 1 Debriefing versus Control, Outcome 5 Depression diagnosis - completers only.

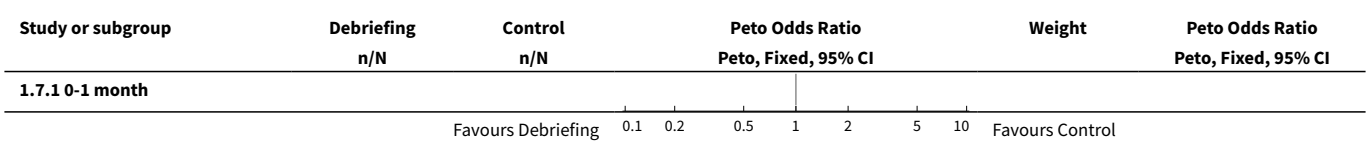


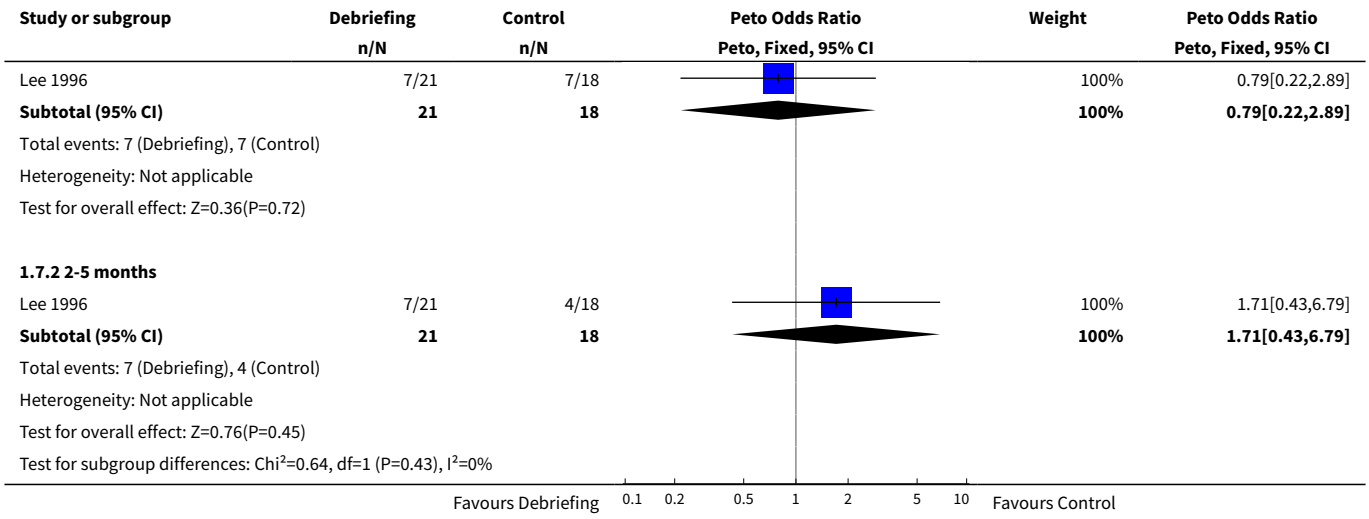


Analysis 1.6. Comparison 1 Debriefing versus Control, Outcome 6 Depression severity.

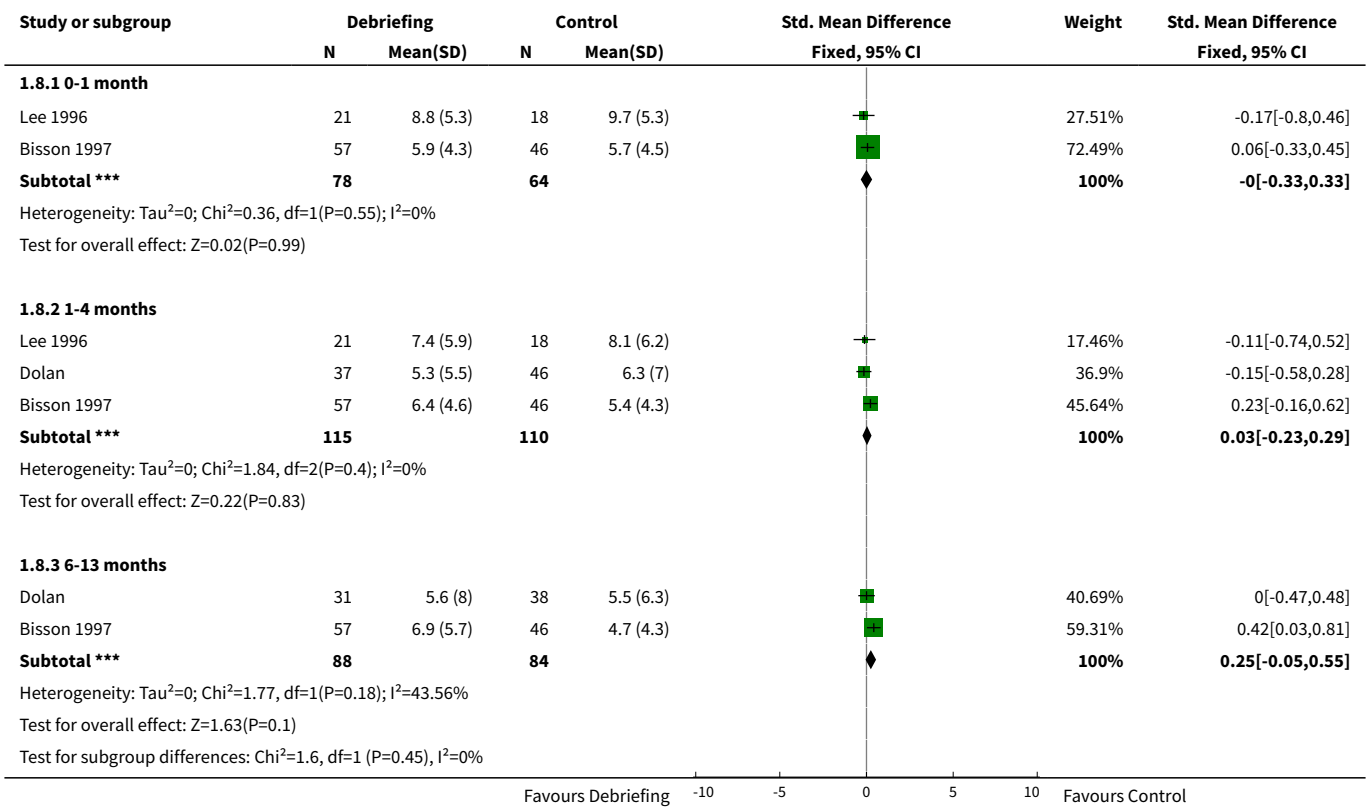


Analysis 1.7. Comparison 1 Debriefing versus Control, Outcome 7 Anxiety diagnosis.





Analysis 1.8. Comparison 1 Debriefing versus Control, Outcome 8 General Anxiety.

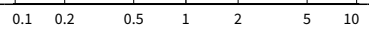


Analysis 1.9. Comparison 1 Debriefing versus Control, Outcome 9 Travel anxiety.

Study or subgroup	Debriefing n/N	Control n/N	Peto Odds Ratio Peto, Fixed, 95% CI	Weight	Peto Odds Ratio Peto, Fixed, 95% CI
1.9.2 2-5 months					
Hobbs 1996	18/54	16/52		100%	1.12[0.5,2.53]
Subtotal (95% CI)	**54**	**52**		**100%**	**1.12[0.5,2.53]**
Total events: 18 (Debriefing), 16 (Control)					
Heterogeneity: Not applicable					
Test for overall effect: Z=0.28(P=0.78)					

Analysis 1.10. Comparison 1 Debriefing versus Control, Outcome 10 All psychiatric morbidity.

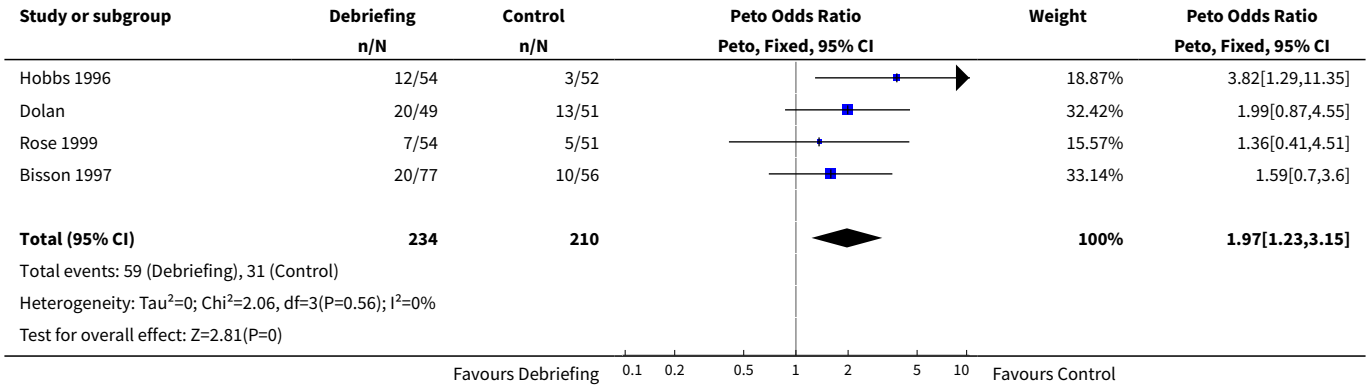
Study or subgroup	Debriefing n/N	Control n/N	Peto Odds Ratio Peto, Fixed, 95% CI	Weight	Peto Odds Ratio Peto, Fixed, 95% CI
1.10.1 0-1 month					
Stevens 1996	33/45	10/18		100%	2.24[0.7,7.19]
Subtotal (95% CI)	**45**	**18**		**100%**	**2.24[0.7,7.19]**
Total events: 33 (Debriefing), 10 (Control)					
Heterogeneity: Not applicable					
Test for overall effect: Z=1.36(P=0.17)					
1.10.2 2-5 months					
Stevens 1996	21/45	10/18		100%	0.7[0.24,2.08]
Subtotal (95% CI)	**45**	**18**		**100%**	**0.7[0.24,2.08]**
Total events: 21 (Debriefing), 10 (Control)					
Heterogeneity: Not applicable					
Test for overall effect: Z=0.63(P=0.53)					
Test for subgroup differences: Chi^2=2.03, df=1 (P=0.15), I^2=50.77%					



Analysis 1.11. Comparison 1 Debriefing versus Control, Outcome 11 Reduced Functioning.

Study or subgroup	Debriefing n/N	Control n/N	Peto Odds Ratio Peto, Fixed, 95% CI	Weight	Peto Odds Ratio Peto, Fixed, 95% CI
1.11.1 3 months					
Bisson 1997	14/57	8/46		100%	1.53[0.59,3.92]
Subtotal (95% CI)	**57**	**46**		**100%**	**1.53[0.59,3.92]**
Total events: 14 (Debriefing), 8 (Control)					
Heterogeneity: Not applicable					
Test for overall effect: Z=0.88(P=0.38)					

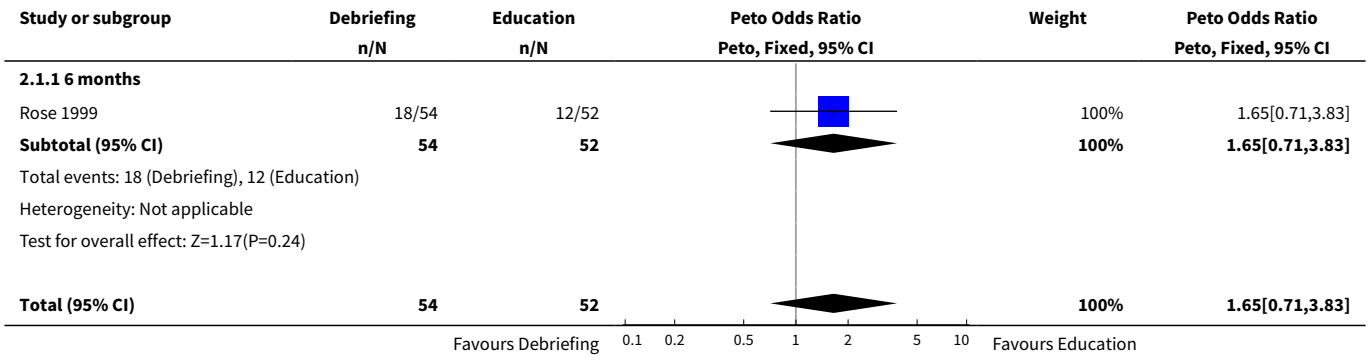
Analysis 1.12. Comparison 1 Debriefing versus Control, Outcome 12 Dropout.

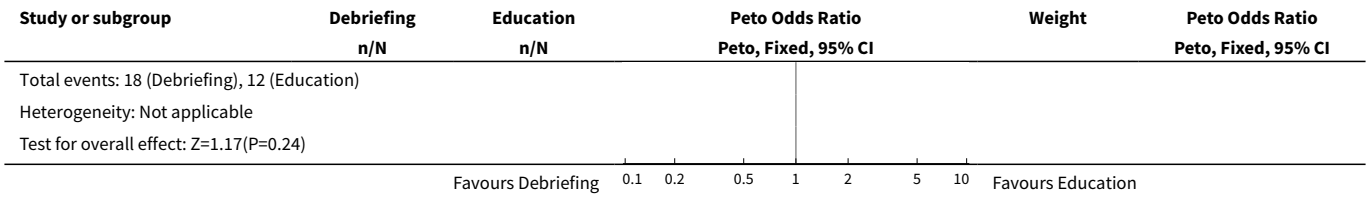


Comparison 2. Debriefing versus Educational intervention

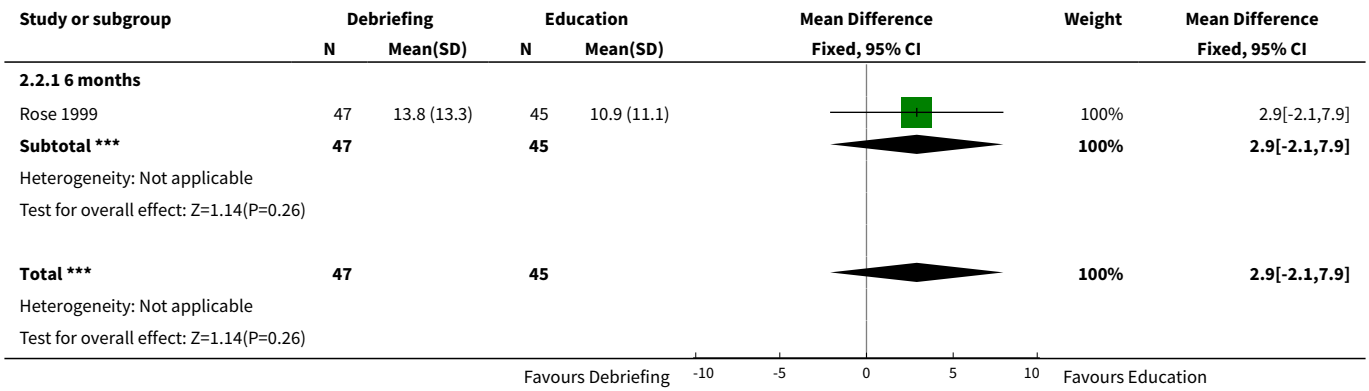
Outcome or subgroup title	No. of studies	No. of partici-pants	Statistical method	Effect size
1 PTSD diagnosis - ITT data	1	106	Peto Odds Ratio (Peto, Fixed, 95% CI)	1.65 [0.71, 3.83]
1.1 6 months	1	106	Peto Odds Ratio (Peto, Fixed, 95% CI)	1.65 [0.71, 3.83]
2 PTSD severity - using self-report measures - completers	1	92	Mean Difference (IV, Fixed, 95% CI)	2.90 [-2.10, 7.90]
2.1 6 months	1	92	Mean Difference (IV, Fixed, 95% CI)	2.90 [-2.10, 7.90]
3 Depression severity - completers	1	92	Mean Difference (IV, Fixed, 95% CI)	2.90 [-2.10, 7.90]
3.1 6 months	1	92	Mean Difference (IV, Fixed, 95% CI)	2.90 [-2.10, 7.90]
4 Dropout	1	106	Peto Odds Ratio (Peto, Fixed, 95% CI)	0.96 [0.31, 2.93]
4.1 6 months	1	106	Peto Odds Ratio (Peto, Fixed, 95% CI)	0.96 [0.31, 2.93]

Analysis 2.1. Comparison 2 Debriefing versus Educational intervention, Outcome 1 PTSD diagnosis - ITT data.

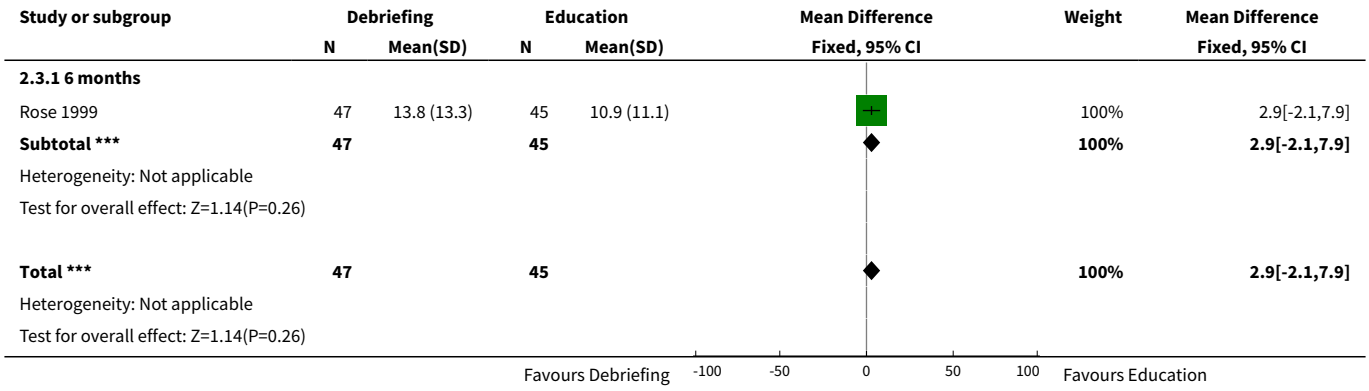




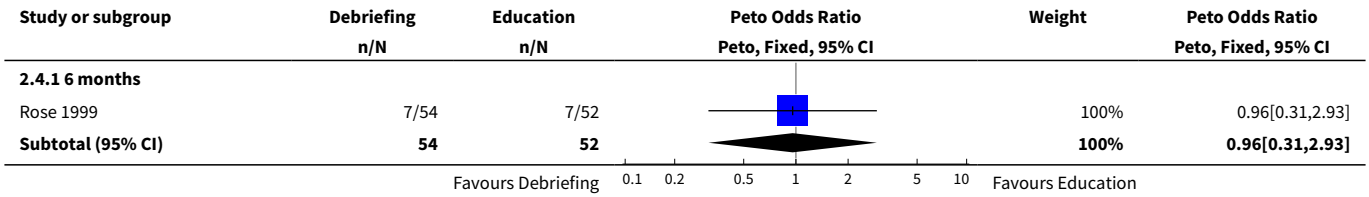
Analysis 2.2. Comparison 2 Debriefing versus Educational intervention, Outcome 2 PTSD severity - using self-report measures - completers.

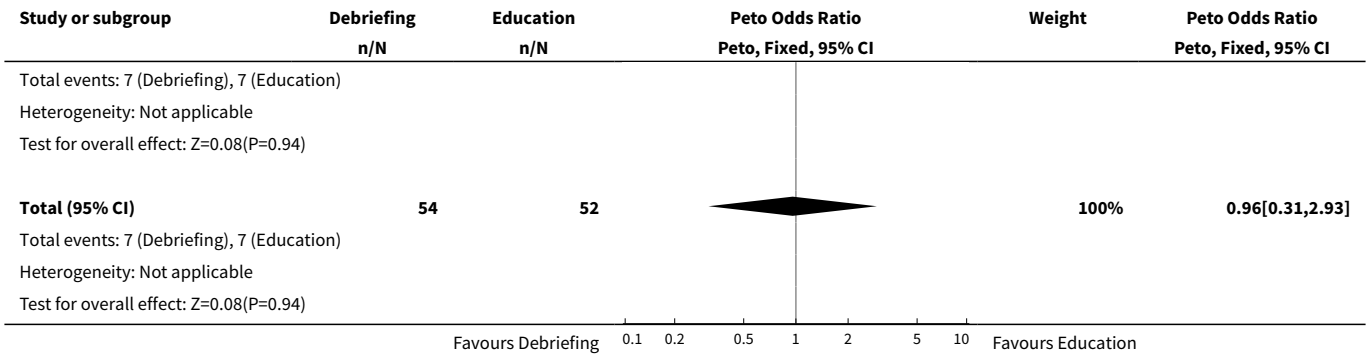


Analysis 2.3. Comparison 2 Debriefing versus Educational intervention, Outcome 3 Depression severity - completers.



Analysis 2.4. Comparison 2 Debriefing versus Educational intervention, Outcome 4 Dropout.

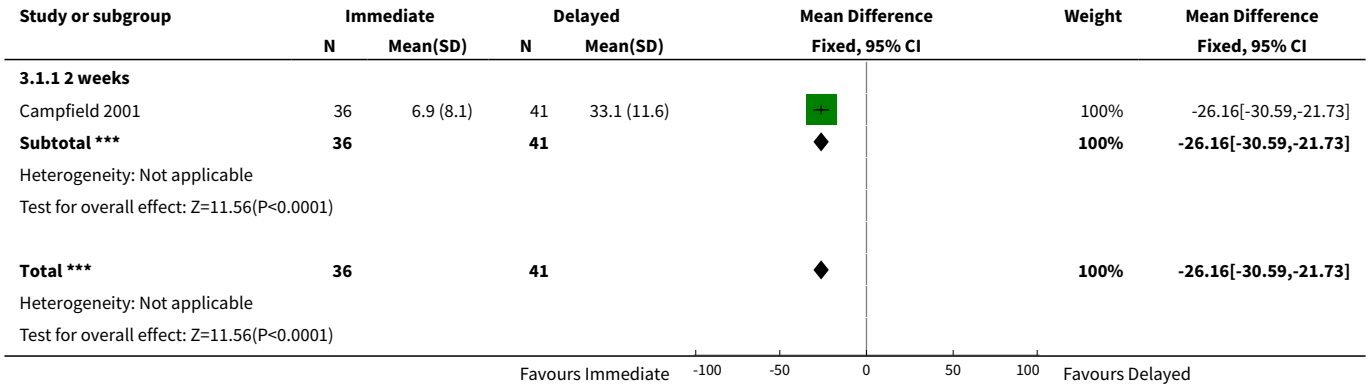




Comparison 3. Immediate Debriefing versus Delayed Debriefing

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 PTSD severity - self-report	1	77	Mean Difference (IV, Fixed, 95% CI)	-26.16 [-30.59, -21.73]
1.1 2 weeks	1	77	Mean Difference (IV, Fixed, 95% CI)	-26.16 [-30.59, -21.73]

Analysis 3.1. Comparison 3 Immediate Debriefing versus Delayed Debriefing, Outcome 1 PTSD severity - self-report.



ADDITIONAL TABLES

Table 1. Methodological ratings for each study using Kenardy scale

Study ID	Total score
Bisson 1997	22
Priest 2003	22
Rose 1999	19

Table 1. Methodological ratings for each study using Kenardy scale *(Continued)*

Dolan	18
Sijbrandij 2005	17.5
Campfield 2001	15
Conlon 1999	15
Lee 1996	14
Litz 2005	14
Hobbs 1996	13
Stevens 1996	13
Bordow 1979	11
Small 2000	11
Lavender 1998	10
Bunn 1979	8

F E E D B A C K

Debriefing discussion - Kenardy

Summary

Since the available evidence of randomised trials of debriefing has been based on procedures that fall into the broad definition of debriefing, it might be that the results arise from the application of an inadequate form of debriefing. Thus it has been argued that if a more prescribed form, such as CISD or its descendant Critical Incident Stress Management (CISM), were used the outcomes would be different. However, to my knowledge, there has been no published RCT employing such prescribed approaches. Certainly, there has been no direct comparison of types of debriefing intervention using RCT methodology. Therefore until this evidence is forthcoming there is no support for one type of debriefing approach over any other.

Debriefing is a "grassroots" type of intervention that has face validity and popular support amongst many health and allied practitioners. I believe that some practitioners are likely to continue to advocate its use in spite of the lack of empirical support for it. Furthermore some organisations are likely to maintain its use since there is no other comparable intervention to serve the purpose of a broadly acceptable early intervention at relatively low cost. This may not be as important an issue (other than to taxpayers and shareholders) if the studies to date were to have found that psychological debriefing had at least no impact on the recovery process. However it would seem that this is not the case. Work by our group indicates that within a community sample post-trauma response is generally one of recovery over time (aside from anniversary effects) stabilizing at levels commensurate with initial exposure1. For debriefing to be worthwhile it should at least alter the downward trajectory of distress such that the process is accelerated over time. What should be of concern to practitioners, organisations and researchers is that not only does the evidence indicate that this is not happening, but that there continues to be indications of a deceleration of recovery associated with debriefing.

Why should this be happening? From the literature there are certain factors that probably impact on that recovery process, such as perceived severity of the trauma in terms of life-threat and significant loss, pre-morbid psychiatric disorder, and significant ongoing stressors1, 2. These are likely to be indicators, in those individuals who have experienced a trauma, for direction to significantly more care than would be available within a debriefing. The challenge is to develop workable and valid methods of detecting such individuals. Other factors may also effect recovery, for example expectations concerning one's responses and reactions. Thus it has been suggested that debriefing "medicalises" normal distress3 by generating in an individual an expectation of pathological responding. Early response to psychological trauma may need to balance minimal intervention with information that helps individuals to self-refer. Personality and coping style may also interact with the process of debriefing and thus affect recovery. However this relationship is likely to be complex. For example avoidance coping style (tendency to avoid rather than confront emotionally distressing experiences) is associated with poorer outcomes following trauma1, suggesting that such individuals should be carefully assisted in undergoing exposure to elements of the

trauma without associated avoidance. However these individuals may be very reluctant to engage in an exposure-based program. These issues are still hypotheses without substantive evidence. But since they bear directly on how an early psychological intervention following a trauma might proceed they are worthy of attention. There is little known about why debriefing might adversely affect recovery, but this information is crucial for the development of an effective early intervention following trauma.

Contributors

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References

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2. MacFarlane AC. The longitudinal course of posttraumatic morbidity: the range of outcomes and their predictors. Journal of Nervous and Mental Disease. 1988; 176:30-39.

3. Wessely S, Rose S. Bisson J. A systematic review of brief psychological interventions ("debriefing") for the treatment of immediate trauma-related symptoms and the prevention of post traumatic stress disorder (Cochrane Review). In The Cochrane Library, Issue 4 1999. Oxford: Update Software.

Psychological Debriefing: Controversy and Challenge
Extracted from JANZPsych. Paper in press

RCT methodology to evaluate debriefing - Deahl

Summary

Outcome research into the effectiveness of acute interventions such as debriefing raises important questions about the ethics as well as the status of conventional RCT methodology as the imprimatur of Evidence Based Medicine (EBM). RCTs have become the dominant paradigm of treatment outcome studies to the virtual exclusion of observational or case studies. CISD was designed for groups of emergency service workers following traumatic events. Conducting a methodologically rigorous RCT of group debriefing would be extremely difficult given that group trauma generally only occurs in unpredictable and often chaotic circumstances such as war or disaster. In emergency situations such as these the operational imperative is paramount and investigators must do the best they can with the available material under difficult and at times extremely fraught circumstances. Irrespective of whether or not debriefing reduces long-term morbidity many individuals find it subjectively helpful at the time (1). Under these circumstances can it therefore be ethically justifiable to employ "non-intervention" controls denying individuals short-term support whatever the long-term outcome? In conflict, following disaster or accident, naturalistic studies, often conducted opportunistically remain useful and have considerable heuristic value despite methodological shortcomings particularly relating to sample selection and randomisation to different treatment conditions. Applying the stringent criteria demanded by the arbiters of EBM such as the Cochrane library to trials of preventive interventions means that much useful work might go unpublished. Clinicians might well lament that in attempting to satisfy such rigorous methodological criteria RCTs have become so divorced from clinical reality that their findings become meaningless. It is noteworthy that even in the most robust RCTs subjects are seldom selected from epidemiological samples. Researchers may be forgiven for forsaking such methodologically challenging research entirely in favour of more biologically oriented research where variables can be more easily controlled, confounding factors minimised and publishable outcomes virtually guaranteed. RCTs are not the sine qua non of EBM and debriefing studies which challenges their hegemony and lend credibility to observational studies has important implications for the ways in which the quality and value of research evidence is assessed both in social psychiatry and empirical science in general.

1.Bisson JI and Deahl MP. Psychological debriefing and preventing post traumatic stress. British Journal of Psychiatry 1994; 165: 717-720.

Contributors

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St. Bartholomew's and Royal London School of Medicine and Dentistry, Queen Mary and Westfield College, University of London.
01 September 2000

Misleading 'Reviewers' conclusions'

Summary

The article is helpful except for a very important point related to the Reviewers' Conclusions.
"There is no current evidence that psychological debriefing is a useful treatment for the prevention of post traumatic stress disorder after traumatic incidents."

should surely read (amendment in capitals):
There is no current evidence that SINGLE SESSION INDIVIDUAL psychological debriefing is a useful treatment for the prevention of post traumatic stress disorder after traumatic incidents.
This conclusion is then precise relative to the study's methodology and less likely to allow the misinterpretation (as has been heard) that the Cochrane review indicated that psychological debriefing (implication: any/all) does not work. Unfortunately some people do only read the 'headlines', so I believe this degree of specification is important.

Reply

The authors would like to thank Dr Elliott for making this important point. The text has been altered accordingly.

Contributors

Dr Colin Elliott
Consultant Clinical Psychologist
colin.elliott@cd-tr.wales.nhs.uk
12/06/2003 15:13:35
I certify that I have no affiliations with or involvement in any organisation or entity with a direct financial interest in the subject matter of my criticisms.

Psychological debriefing for PTSD

Summary

The sentence page 1 under, Main results, line 2-3 does not make sense. Is it that those who received the intervention showed no significant short term increased risk of PTSD?

Reply

This sentence has now been amended.

Contributors

A O'Neill-Kerr
xew46@dial.pipex.com
04/11/2001
I certify that I have no affiliations with or involvement in any organisation or entity with a direct financial interest in the subject matter of my criticisms

Está demostrado que el debriefing es inefectivo

Summary

Una de las características base del debriefing es su brevedad, en la mayoría de los casos de una sesión, por lo que la revisión es correcta y las conclusiones también. Yo también he ehcho revisiones sobre el tema y parece que la evidencia es clara: debriefing doesn,t work!!!!!!!!

'One of the basic characteristics of debriefing is its brevity, in the majority of cases only one session, and that's why this review and its conclusions are correct. I have also done a review on this theme and it appears that the evidence is clear: debriefing does't work!'

I certify that I have no affiliations with or involvement in any organisation or entity with a direct financial interest in the subject matter of my criticisms.

Reply

N/A

Contributors

Sender Beatriz
Sender Description pshycologist
Sender Email beveposeck@yahoo.com
Sender Address
Date Received 03/12/2003 18:15:45

WHAT'S NEW

Date	Event	Description
5 November 2008	Amended	Converted to new review format.

HISTORY

Protocol first published: Issue 1, 1997
Review first published: Issue 2, 1998

Date	Event	Description
3 December 2001	New citation required and conclusions have changed	Substantive amendment

CONTRIBUTIONS OF AUTHORS

SW, SR and JB developed the original protocol, undertook the review and provided the first update. RC subsequently made alterations to the originally updated review and added additional data to provide a second update.

DECLARATIONS OF INTEREST

Both JB and SR were responsible for two of the trials included in this review.
SW and RC have no conflict of interest.

SOURCES OF SUPPORT

Internal sources

- King's College School of Medicine Strategy Fund (Trials Register) SW, UK.

External sources

- NHS Management Executive and Berkshire Healthcare NHS Trust (SR), UK.

NOTES

The trials examining the effects of psychological debriefing for the prevention of PTSD following childbirth are to be removed from this review and published in a separate review to be made available shortly.

INDEX TERMS

Medical Subject Headings (MeSH)

*Crisis Intervention; Randomized Controlled Trials as Topic; Stress Disorders, Post-Traumatic [*prevention & control]

MeSH check words

Humans