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Debriefing interventions for the prevention of psychological trauma in women following childbirth (Review)

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[Intervention Review]

Debriefing interventions for the prevention of psychological trauma in women following childbirth

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ABSTRACT

Background

Childbirth is a complex life event that can be associated with both positive and negative psychological responses. When giving birth is experienced as particularly traumatic this can have a negative impact on a woman's postnatal emotional well-being. There has been an increasing focus on women's psychological trauma symptoms following childbirth, including the relatively rare phenomenon of post-traumatic stress disorder (PTSD), and the benefit of debriefing interventions to prevent this. In this review we examined the evidence for debriefing as a preventative intervention for psychological trauma following childbirth.

Objectives

To assess the effects of debriefing interventions compared with standard postnatal care for the prevention of psychological trauma in women following childbirth.

Search methods

The trials registers of the Cochrane Depression, Anxiety and Neurosis Group (CCDANCTR-References and CCDANCTR-Studies) and the Cochrane Pregnancy and Childbirth Group were searched up to 4 March 2015. These registers include relevant randomised controlled trials from the following bibliographic databases: the Cochrane Library (all years to date), MEDLINE (1950 to date), EMBASE (1974 to date), and PsycINFO (1967 to date). Additional searches were conducted in CENTRAL, MEDLINE, EMBASE, PsycINFO, and Maternity and Infant Care. The reference lists of all included studies were checked for additional published reports and citations of unpublished research. Experts in the field were contacted.

Selection criteria

We included randomised controlled trials (RCTs) and quasi-randomised trials comparing postnatal debriefing interventions with standard postnatal care for the prevention of psychological trauma of women following childbirth. The intervention consisted of at least one debriefing intervention session, which had the purpose of allowing women to describe their experience and to normalise their emotional reaction to that experience.

Data collection and analysis

Three authors independently assessed trial quality and extracted data. Meta-analysis was conducted where there were more than two trials examining the same outcomes.

Main results

We included seven trials (eight articles) from three countries (UK, Australia and Sweden) that fulfilled the inclusion criteria. The number of women contributing data to each outcome varied from 102 to 1745. Methodological quality was variable and most of the studies were of low quality. The quality of evidence for the prevalence of psychological trauma (primary outcome) and the prevalence of depression symptoms was rated low or very low, based on few studies (ranging from a single study to three studies) with high risk of bias in main domains such as performance bias, random sequence generation, allocation concealment and incomplete outcome data. The quality of evidence for the remaining outcomes (that is prevalence of anxiety, prevalence of fear of childbirth, prevalence of general psychological morbidity, health service utilization and attrition from treatment) was not assessed as data were not available.

Among women who had a high level of obstetric intervention during labour and birth, we found no difference between standard postnatal care with debriefing and standard postnatal care without debriefing on psychological trauma symptoms within three months postpartum (RR 0.61; 95% CI 0.28 to 1.31; n = 425) or at three to six months postpartum (RR 0.62; 95% CI 0.27 to 1.42; n = 246). The results were based on two trials, respectively. Among women who experienced a distressing or traumatic birth, there was no evidence of an effect of psychological debriefing on the prevention of PTSD (measured by the MINI-PTSD) at four to six weeks postpartum (RR 1.15; 95% CI 0.66 to 2.01; n = 102) or at six months (RR 0.35; 95% CI 0.10 to 1.23; n = 103). The results were based on one small trial. One trial involving low-risk women who delivered healthy infants at or near term reported no significant difference between the intervention group and the control group in the proportion of women who met the diagnostic criteria for psychological trauma during the year following childbirth (RR 1.06; 95% CI 0.88 to 1.28; n = 1745). We did not find any information about attrition rates.

Authors' conclusions

We did not find any high quality evidence to inform practice, with substantial heterogeneity being found between the studies conducted to date. There is little or no evidence to support either a positive or adverse effect of psychological debriefing for the prevention of psychological trauma in women following childbirth. There is no evidence to support routine debriefing for women who perceive giving birth as psychologically traumatic.

Future research should provide greater detail of the outcome measures used, and with scales for measuring psychological trauma validated against clinical diagnostic interviews. High rates of obstetric intervention in some birth settings may mean that women require improved emotional care from health professionals to reduce the risk of childbirth being experienced as traumatic. As all included trials excluded women unable to communicate in the native language of the study setting, there is no information on the response of these women to psychological debriefing. No included studies were conducted in low or middle-income countries.

PLAIN LANGUAGE SUMMARY

Debriefing interventions for the prevention of psychological trauma in women following childbirth

Why is this review important?

Having a baby is a complex life event. While many women view their experiences of giving birth as very positive, childbirth can sometimes be experienced as a traumatic event. If a birth is experienced as traumatic, it could have a negative impact on a woman's long-term emotional well-being. Relationships between mother and child may be affected, as can the women's relationships with other family members. One intervention that is commonly used with the aim of reducing psychological trauma (that is anxiety, trauma or depressive symptoms) and preventing the development of post-traumatic stress disorder following birth is debriefing. Debriefing includes a variety of post-birth discussions that provide women an opportunity to talk about their birth experience. In this review we examined the evidence for debriefing as a preventative intervention for psychological trauma following childbirth.

Who may be interested in this review?

- Women who have recently given birth, their families and friends.

- Midwives, health visitors and other medical professionals who have close contact with women who are pregnant or have just given birth.

What questions does this review aim to answer?

Is debriefing more or less effective than standard postnatal care in preventing psychological trauma among women who have recently given birth.

Which studies were included in the review?

We searched databases to find all studies (specifically randomised controlled trials) published before 4 March 2015 that investigated debriefing for the prevention of psychological trauma in women following childbirth. We included seven studies with a total of 3596 women. The studies were published between 1998 and 2005 and all were conducted in high-income countries (UK, Australia and Sweden).

What does the evidence from the review tell us?

There was no evidence of a difference between debriefing and standard postnatal care in preventing psychological trauma up to three months post-birth or at three to six months after birth. We did not find any information to tell us whether debriefing led to women leaving the studies early. The quality of the evidence presented in the included studies was generally low. There were a number of limitations in the way the studies were designed (for example some had small sample sizes) and reported (for example incomplete data were presented). Further well-designed studies are needed for us to more clearly understand whether debriefing can minimise the psychological impact of a traumatic birth experience and ensure that it poses no harmful effects.

SUMMARY OF FINDINGS FOR THE MAIN COMPARISON [Explanation]

Psychological debriefing compared with usual postnatal care for the prevention of psychological trauma in women following childbirth

Patient or population: Women of any age who had given birth within one month of the intervention being offered
Settings: Hospital settings
Intervention: Psychological debriefing
Comparison: Usual postnatal care

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of participants (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk	Corresponding risk				
	Usual postnatal care	Psychological debriefing				
Prevalence of psychological trauma (short term: up to 3 months postpartum) Measured by various scales - Impact of Event Scale (IES), MINI-PTSD	**Universal**					
	Data not available					
	Selected - low level of obstetric intervention					
	Data not available					
	Selected - high level of obstetric intervention					
	288 per 1000	**173 per 1000** (98 to 306)	**RR 0.60** (0.34 to 1.06)	338 (2 studies)	⊕◯◯◯ **very low**[1,2,3,4]	
	Indicated					
	302 per 1000	**347 per 1000** (199 to 607)	**RR 1.15** (0.66 to 2.01)	102 (1 study)	⊕⊕◯◯ **low**[3,4]	

Prevalence of depression or depressive symptoms (short term: up to 3 months postpartum) Measured by various scales - EPDS, HADS	**Universal**				
	Data not available				
	Selected - low level of obstetric intervention				
	554 per 1000	**89 per 1000** (39 to 205)	**RR 0.16** (0.07 to 0.37)	114 (1 study)	⊕◯◯◯ **very low**[3,4,5]
	Selected - high level of obstetric intervention				
	Data not available				
	Indicated				
	340 per 1000	**326 per 1000** (187 to 567)	**RR 0.96** (0.55 to 1.67)	102 (1 study)	⊕⊕◯◯ **low**[3,4]
Prevalence of anxiety (short term: up to 3 months postpartum) Measured by HADS	**Universal**				
	Data not available				
	Selected - low level of obstetric intervention				
	500 per 1000	**69 per 1000** (4 to 134)	**RR 0.14** (0.05 to 0.37)	114 (1 study)	⊕◯◯◯ **very low**[3,4,5]
	Selected - high level of obstetric intervention				
	Data not available				
	Indicated				
	Data not available				

Prevalence of fear of childbirth (short term: up to 3 months postpartum)	**Universal**	
	Data not available	
	Selected - low level of obstetric intervention	
	Data not available	
	Selected - high level of obstetric intervention	
	Data not available	
	Indicated	
	Data not available	
Prevalence of general psychological morbidity (short term: up to 3 months postpartum)	**Universal**	
	Data not available	
	Selected - low level of obstetric intervention	
	Data not available	
	Selected - high level of obstetric intervention	
	Data not available	
	Indicated	
	Data not available	
Health service utilization (short term: up to 3 months postpartum)	**Universal**	
	Data not available	
	Selected - low level of obstetric intervention	
	Data not available	

	Selected - high level of obstetric intervention	
	Data not available	
	Indicated	
	Data not available	
Attrition from treatment (short term: up to 3 months postpartum)	**Universal**	
	Data not available	
	Selected - low level of obstetric intervention	
	Data not available	
	Selected - high level of obstetric intervention	
	Data not available	
	Indicated	
	Data not available	

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: Confidence interval; **RR:** Risk Ratio

GRADE Working Group grades of evidence
High quality: Further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: We are very uncertain about the estimate.

[1] Downgraded one point because of risk of bias caused by inadequacy of random sequence generation and allocation concealment (Ryding 1998).
[2] Downgraded one point because of risk of bias caused by incomplete outcome data, selecting reporting and/or imbalances in the intervention and control groups at baseline (Kershaw 2005).

[3] Downgraded one point because of risk of bias (performance and information bias) caused by the unblinding for debriefing providers and recipients.
[4] Downgraded one point because of imprecision (with a wide 95% confidence interval) caused by small sample or a single trial.
[5] Downgraded one point because of risk of bias (selection bias) as shown in a high proportion of single mothers.

BACKGROUND

Description of the condition

Childbirth is a complex life event that can be associated with both positive and negative psychological responses. Having a baby has been associated with a range of mental health problems, including postnatal depression and postnatal psychosis. Some women who become pregnant may have pre-existing mental health problems (such as anxiety and depression) or develop new problems during their pregnancy, which continue post-birth.

Concerns about mental health issues following birth have, to date, largely focused on postnatal depression (PND), which affects around 6% to 13% of women post-birth (Gavin 2005; Ohara 1996). However, a range of psychological problems may actually be experienced by women who have recently given birth (Bick 2003) including anxiety disorders, such as post-traumatic stress disorder (PTSD) and obsessive compulsive disorder, eating disorders such as anorexia nervosa, schizophrenia and bi-polar disorder (NICE 2007). While severe postnatal depression and puerperal psychosis are the most serious psychological disorders associated with childbirth, co-morbidity with other psychological trauma in the postnatal period may be missed, or its relationship with postnatal depression overlooked (Creedy 1999).

Recently, there has been increased recognition of other conditions such as PTSD following childbirth (Andersen 2012). PTSD is an anxiety disorder that may follow an extremely traumatic stressor. PTSD symptoms may include flashbacks, sleep disturbances, panic attacks, numbness and hypervigilance (APA 1994; Horowitz 1979). The prevalence of these symptoms has typically been measured within the first six months postpartum, but there is evidence suggesting the potential longevity of post-traumatic stress responses in some women (McDonald 2011).

According to the Diagnostic and Statistical Manual of Mental Disorders, Fifth Edition (DSM-5), American Psychiatric Association (APA 2013), PTSD is classified as a trauma or stressor related disorder that encompasses a history of exposure to a traumatic event which meets specific stipulations and symptoms from each of four symptom clusters, rather than the three symptom clusters included in DSM-IV. The four symptom clusters are: intrusion or re-experiencing the traumatic event, avoidance of reminders of the event, negative alterations in cognitions and mood, and alterations in arousal and reactivity. DSM-5 diagnostic criteria also consider the duration of symptoms; whether the disturbance causes impaired functioning; and clarification of symptoms as not attributable to the physiological effects of substance misuse (medication, alcohol) or co-occurring medical condition. For clinical diagnosis, symptoms must be present for more than a month, with the disorder being described as chronic if symptoms are present for more than three months. In addition, the effects of the disorder must cause significant distress and disturbance to personal functioning in areas such as social or occupational realms (APA 2000). PTSD sufferers may also experience other associated symptoms, including generalised anxiety, guilt and depression (Rose 2002).

People of all ages can have PTSD. However, women may be at increased risk of PTSD because they are more likely to experience the kinds of trauma that can trigger the condition. Whereas childbirth is a naturally occurring event within the range of usual experience for the majority of women, for some women childbirth is physically and psychologically traumatic and can trigger emotional stress reactions of sufficient intensity to cause PTSD (Ballard 1995; Boorman 2014; Czarnocka 2000; Moleman 1992; Reynolds 1997; Wijma 1997). It is accepted that childbirth is, at least in some instances, a complex event that may lead to a variety of psychological responses. Women may perceive their birthing experience as traumatic as a result of the mode of birth, intervention during the labour or birth, or the way they were treated by healthcare professionals (Allen 1998). The DSM-5 definition of stressors for the development of PTSD was revised in 2012 to include the trigger to PTSD as exposure to actual or threatened death, serious injury or sexual violation. This definition can clearly be applied to certain experiences of childbirth, whether the perceived threat is subjective or objective. However, the onset of PTSD following childbirth has been a somewhat controversial topic.

Recent systematic reviews of risk factors for psychological trauma and post-traumatic stress following childbirth found that the level of obstetric intervention during labour and birth could increase the risk of psychological trauma, with emergency caesarean section and instrumental birth identified as important predictors of PTSD (Andersen 2012; Olde 2006). Women who underwent an emergency caesarean section or instrumental vaginal delivery were more likely to report symptoms of PTSD than women who had an elective caesarean section or a normal vaginal birth (Andersen 2012); and they were also more likely to report the experience of constantly reliving the birth (Brown 1998). However, having a spontaneous vaginal birth can be experienced as psychologically traumatic for some women (Goldbeck-Wood 1996; Scott 1994; Soderquist 2002; Soderquist 2006; Soet 2003), making it difficult to define what constitutes a psychologically traumatic birth simply by the mode of birth or level of obstetric intervention experienced. A systematic review of risk factors for developing PTSD following birth, which included 31 primarily observational studies, reported that infant complications, little support during labour and birth, psychological problems during pregnancy and previous traumatic experiences were also important risk factors for the development of PTSD (Andersen 2012).

An increase in the number of women giving birth in developed countries who sustain severe morbidity during or after pregnancy, such as pre-eclampsia or postpartum haemorrhage (Norman 2011; Waterstone 2001; Wen 2005), has also raised the question of whether they may be more at risk of experiencing PTSD, with some evidence of an association (Cohen 2004; Engelhard 2001; Engelhard 2002; Furuta 2012). Further research is needed to confirm this.

Although most women with a psychologically traumatic birth experience do not go on to develop the full PTSD syndrome, estimates of the proportion of women giving birth who met the previous DSM-IV criteria for acute PTSD ranged from 1% to 9%. This is based on findings of studies from low or middle-income countries (for example Adewuya 2006), high-income countries (Allen 1998; Ayers 2001; Ayers 2004; Beck 2011; Creedy 2000; Czarnocka 2000; Olde 2005; Soet 2003; Stramrood 2011; Wijma 1997) and self-selected samples (Beck 2011; Stramrood 2011). Despite the range of studies and settings reported, psychological trauma following childbirth is a public health concern. Research has demonstrated that the incidence of psychological trauma tends to be higher when assessed in the early postnatal period, as reported by Ayers and Pickering (Ayers 2001) and Wijma et al (Wijma 1997), and decreases over time. However, a small subset of women will experience symptoms of persistent psychological trauma for several months or even years (Ballard 1995; Slade 2006).

Childbirth related trauma has been associated with maternal postnatal morbidity in relation to anger and guilt (Olde 2006), depression (Bener 2012), suicidal thoughts (Howard 2011; Lindahl 2005) and PTSD symptoms (Denis 2011). Further psychosocial implications of a traumatic birth on maternal well-being have been described, such as relationship difficulties with partners and offspring, and could affect future pregnancies and childbirth (Fenech 2014). Some women feel so traumatized by giving birth that fear may alter their wishes for a future pregnancy (Hofberg 2003) or influence decisions about mode of birth, with implications for birth outcome (Dennett 2003). Although fear of birth is more common in nulliparous women, women who have a negative or traumatic experience are almost five times more likely to report fear of birth in a subsequent pregnancy, and they are more likely to want epidural anaesthesia or caesarean section (Storksen 2013). Evidence of a range of psychological traumas following birth is now compelling and postnatal debriefing is one intervention that has been implemented in some places in an attempt to reduce this morbidity.

Description of the intervention

The term 'debriefing' is used to describe a semi-structured conversation with an individual who has recently experienced a stressful or traumatic event. In psychology research, debriefing describes a structured psychological treatment intended for primary prevention of acute psychological morbidity as a result of experiencing a traumatic event (Dyregrov 1989; Rose 2002). In the 1980s, debriefing, also known as critical incident stress debriefing, was widely adopted as a therapeutic response for people who experienced a wide variety of traumatic events (Mitchell 1983), including personnel involved with major trauma incidents, victims of rape and rescue workers following natural disasters. In most cases, the purpose of debriefing is to reduce the possibility of psychological harm by them being informed about their experience, or allowing them to talk about it. However, there remains debate about the possible benefits or harms of debriefing (Wessely 2003).

Psychological debriefing is a formal therapy for providing emotional and psychological support immediately following a traumatic event, and involves the promotion of emotional processing by encouraging active recollection of the traumatic event (Dyregrov 1989) linked to overt emotional responses. It aims to help women externalise their thoughts and feelings while allowing simultaneous experiencing of the full emotional response to the traumatic event. It may be operationalised in several stages, such as 1) introduction, 2) the facts, 3) thoughts and impressions, 4) emotional reactions, 5) normalisation, 6) planning for the future and 7) disengagement (Dyregrov 1989; Mitchell 1983); or 1) identification, 2) labelling, 3) articulation, 4) expression, 5) externalisation, 6) ventilation, 7) validation and 8) acceptance (Curtis 1995).

The term 'postnatal debriefing' has been used to describe a variety of post-birth discussions, implemented with the intention of providing women with an opportunity to talk about their birth experiences (Smith 1996). This approach does not aim to elicit every detail of the woman's experience, nor to explicitly link the experience to emotional response. It is led by the woman and may focus on only one question she has about a particular event, or provide an opportunity for the woman to 'tell her story'. This sometimes less-structured approach to debriefing in the childbirth arena has led to some confusion about the purpose and effectiveness of such interventions (Alexander 1998; Inglis 2002; Rowan 2007). Whilst studies appear to support the idea that talking with a supportive listener enables women to gain a fuller understanding of labour and birth events, and to develop a sense of resolution about their birth experiences (Berg 1998; Reynolds 1997), the effectiveness of debriefing in the prevention of psychological morbidity following birth is not at all clear (Gamble 2002).

The National Institute for Health and Care Excellence (NICE) guideline on antenatal and postnatal mental health (NICE 2007), which provides recommendations for routine national health service provision in England and Wales, does not recommend routine formal debriefing for women who have experienced a physically traumatic birth, for example an emergency caesarean birth. However, it encourages maternity staff and other healthcare professionals to support women who wish to talk about their birth experiences and to make use of natural support systems available from family and friends, taking into account the effect of the birth on the partner. This supports a postnatal debriefing approach, or a non-directive counselling approach, rather than a psychological debriefing one.

Cognitive behavioural therapy (CBT) interventions may be offered to women (Lapp 2010) who have had difficult birth experiences. While these tend to be highly structured and offer women the opportunity to explore their thoughts and feelings related to the birth, they differ from psychological debriefing in that they do not include the formal stages detailed above, and they do not

have a focus on emotional processing. Instead, they concentrate on identifying unhelpful thoughts and feelings which may be open to challenge and reprocessing.

Non-directive counselling, which can include supportive listening interventions, aims to provide a safe space for women to narrate their experience to a supportive other. It does not provide direct input aimed at reprocessing, challenging, or active recall of the delivery. While non-directive counselling approaches may be utilized within a postnatal debriefing approach, they are more limited, with no planned focus on recalling and discussing specific aspects of the delivery (Chew-Graham 2008; Gamble 2002; Rowan 2007).

How the intervention might work

A positive birth experience has been associated with a sense of mastery and competency (Nichols 1996) and positive expectations of future childbirth experiences (Waldenstrom 1996). However, some women who experience a birth as traumatic are at risk of developing emotional distress and psychological trauma. The offer of a structured opportunity for a woman to recount her experience of labour and birth to someone who listens, acknowledges and normalises her emotional responses to the birth is hypothesised as enabling the emotional processing of that experience soon afterwards, in order to prevent subsequent development of psychological problems that may have occurred due to inadequate processing of a traumatic experience of labour and birth (Ayers 2006; Deahl 2000). This is much as debriefing following other traumatic events has been hypothesised to do (Roberts 2009).

Clinically it appears that those who respond to trauma by discussing their experience with professionals, friends and family members are less likely to develop trauma symptoms than those who feel unable to talk about the event (Rowan 2007), although there is very little published research. It is thought that the experience of discussing the birth enables the woman to develop a coherent narrative by fusing her own memories together with the answers to questions she may ask the health professional discussing the birth with her (NICE 2007). Women in labour are more likely to find it difficult to form a coherent narrative as they are likely to be tired, to have experienced high levels of pain, and to have been given systemic drugs which make it more difficult to encode the experience into their memory (Furuta 2014).

Andersen 2012 reported that subjective distress and obstetric emergencies were predictive of trauma responses. Subjective distress may be addressed by debriefing as each element of the birth experience is discussed and explored, providing women with time to consider what was happening in the labour room and to form an alternative understanding of their experience through normalisation, externalisation and articulation of their responses (utilizing the language of psychological debriefing). In terms of a CBT approach, an opportunity to challenge the woman's thoughts and beliefs is provided. Where an objective obstetric emergency has occurred, debriefing may provide time to consider the facts and confer understanding as to the reason for decisions, particularly where these were rapid.

The NICE guideline for routine postnatal care (NICE 2015) recommends that women are asked about their emotional health at every postnatal contact, with some evidence to suggest that listening to women may reduce emotional distress (Gamble 2002), although further research into this area was recommended. Various individual Royal College of Obstetrics and Gynaecology (RCOG) guidelines recommend debriefing in particular circumstances, such as late intrauterine fetal death and stillbirth (RCOG 2010); maternal collapse in pregnancy and the puerperium (RCOG 2011); placenta praevia, placenta praevia accreta and vasa praevia (RCOG 2011b); in the prevention and management of postpartum haemorrhage (RCOG 2009); and umbilical cord prolapse (RCOG 2008).

Debriefing has the potential to prevent the development of PTSD in the general population. However, the conflicting evidence on debriefing for women following traumatic childbirth causes a number of issues that require clarification, the definition of debriefing by whom, what does it entail, and the timing or targeting of it. Ayers 2006 suggest that it is possible that postnatal debriefing is different from psychological debriefing and may be more effective, but that the evidence is inconclusive. Others state that it might be important to differentiate between those women who have experienced a traumatic birth and those with trauma symptoms of PTSD; and that it might be appropriate to offer women an opportunity to discuss their childbirth experience while differentiating this discussion from formal debriefing (Rowan 2007).

Why it is important to do this review

If a woman's experience of childbirth is particularly traumatic, that experience may have a negative impact on her emotional well-being and lead to serious psychological morbidity with profound, short- and long-term implications not only for her mental health but also for her relationship with her child and other family members (Beck 2004; Dennett 2003). Maternal psychological morbidity can have adverse consequences for maternal-infant interaction and the child's longer-term cognitive and emotional development (Deave 2008; DiPietro 2006). Research on postnatal psychological morbidity has focused mainly on the effects of depressive disorders on the woman and her family. Although research on psychological trauma following childbirth has been scant, there is now increasing focus on the relatively rare and debilitating phenomenon of PTSD (Andersen 2012; Ayers 2001a; Olde 2006).

Previous Cochrane reviews, which assessed the effects of single session psychological debriefing (Rose 2002) and multiple session psychological interventions (Roberts 2009) for prevention of PTSD after traumatic incidents in a range of populations (including individuals admitted to hospital following physical trauma, attending trauma clinics or casualty), concluded that there is no evidence that either single session or multiple session interven-

tions was a useful treatment. While these authors concluded that compulsory debriefing of victims of trauma should cease, the review focused on the non-obstetric population, excluding trials of women post-birth due to differences in the participants and interventions involved. This systematic review therefore addresses this evidence gap by reviewing relevant trials which used debriefing interventions to prevent psychological trauma in women following childbirth.

OBJECTIVES

To assess the effects of debriefing compared with standard postnatal care for the prevention of psychological trauma in women following childbirth.

METHODS

Criteria for considering studies for this review

Types of studies

We included all published and unpublished randomised controlled trials (RCTs), quasi-randomised controlled trials (such as those where allocation was by date of birth or hospital number) and cluster RCTs.

We excluded cross-over trials, because this type of trial is only suitable for interventions with a temporary effect in the treatment of stable, chronic conditions (Higgins 2008); it was therefore a trial design unlikely to be used to address the current review question.

Types of participants

Participant characteristics

Women of any age who had given birth within one month of the intervention being offered. We included women regardless of the type of birth they had. We excluded papers with patients with pre-existing mental health disorders, psychological symptoms associated with miscarriage and abortion; and PTSD or PTSD symptoms in pregnant women not associated with pregnancy related events but with other events such as conflict, accidents or natural disasters.

Setting

There was no restriction on setting.

Co-morbidities

We excluded participants with pre-existing mental health disorders (such as anxiety or depression) and puerperal psychosis.

Types of interventions

Experimental intervention

Definition of debriefing

Any psychological intervention delivered to postnatal women with the intent of preventing psychological trauma that involved some recollection, reliving or reworking of the birth experience and subsequent emotional reactions (Gamble 2004; NICE 2005).

Inclusion was not dependent on authors' labelling of the intervention as debriefing. Interventions could have been described as psychological debriefing, stress debriefing, critical incident stress debriefing, crisis intervention, psychiatric stress debriefing, multiple stressor debriefing, traumatic event debriefing; or as non-directive (including supportive listening interventions), counselling or cognitive behavioural therapy. Rather, what was considered important was that the intervention should provide an opportunity, more or less structured, for women to recount their birth experience and to have their emotional reactions to that experience acknowledged and normalised.

For the purposes of this review (and in case it is necessary to stratify the analyses by type of debriefing in future updates of the review), we classified the types of intervention as follows.

Postnatal debriefing: this has been defined in different ways and generally lacks clarification of what postnatal debriefing constitutes (Rowan 2007; Steele 2003). Postnatal debriefing typically involves a midwife going through a woman's birth events with her, usually with the medical notes available (Meades 2011).

Psychological debriefing: usually defined as a set of procedures administered to individuals or groups who have been exposed to traumatic events that are aimed at preventing psychological morbidity, PTSD and other related anxiety disorders, and to aid recovery.

Number and timing of sessions

We included both single session and multiple session debriefing. There was no upper limit on the number of sessions that we included. We included both individual and group interventions.

Target group for the intervention

We included interventions that were either universal (all women following birth), selected (women at risk of psychological trauma), or indicated (women identified with existing psychological trauma or distress symptom) (Lumley 2004). Universal prevention strategies are designed to reach the entire population, without regard to

individual risk factors. Selected prevention strategies target subgroups of the general population that are determined to be at risk. Indicated prevention interventions identify individuals who are experiencing early signs of psychological trauma and other related psychological problems associated with childbirth and target them with special programmes.

Comparator intervention

Standard postnatal care, which denotes the usual postnatal care provided within the first six weeks post-birth in each setting, and which did not include any routine psychological intervention aimed at preventing psychological trauma.

Types of outcome measures

Primary outcomes

1. Prevalence of psychological trauma. The Impact of Event Scale (IES) (Horowitz 1979; Sundin 2002) is the most widely used validated instrument to measure the presence of symptoms of psychological trauma. When IES data were unavailable, data on any comparable scales, such as the Clinician Administered PTSD Scale (Blake 1995), were used.

Secondary outcomes

2. Severity of psychological trauma.
3. Depression as measured using a variety of scales, including the Hospital Anxiety and Depression Scale - Depression Subscale (HAD-D), the Beck Depression Inventory (BDI) (Beck 1961) or the Edinburgh Postnatal Depression Scale (EPDS) (Cox 1987).
4. Anxiety as measured using a variety of scales, including the Hospital Anxiety and Depression Scale- Anxiety Subscale (HAD-A), Spielberger State-Trait Anxiety Inventory (STAI) (Spielberger 1983) or Viney and Westbrook's cognitive anxiety scale (Viney 1976).
5. Fear of childbirth as measured, for example, using the Wijma Delivery Expectancy/Experience Scale (W-DEQ) (Wijma 1998), a measure to assess a woman's fears about childbirth, by asking her about her expectancies before childbirth (version A: W-DEQ (A)) and experiences after childbirth (version B: W-DEC (B)).
6. General psychological morbidity as measured using a variety of scales, including the Hospital Anxiety and Depression Scale (HADS) (Bjelland 2002; Zigmond 1983), the Brief Symptom Inventory (BSI) (Derogatis 1983), Short Form-36 (SF-36) (Ware 1992) or the Langer 22 Item Scale of psychiatric symptoms (Langner 1962).
7. Health service utilisation including outpatient and inpatient use of psychiatric unit, other health services.
8. Attrition from treatment.
9. Use of healthcare resources.

Timing of outcome assessment

The timing of outcome assessments in this review were specified as follows.

- Short term: up to three months postpartum.
- Medium term: three to six months postpartum.
- Long term: more than six months postpartum.

These timings were selected to reflect the onset and duration of clinical features of PTSD symptoms (one of the main outcomes of interest in this review), as defined in the DSM-IV diagnostic criteria for PTSD (APA 1994), which describes symptoms as acute if the duration is less than three months, chronic if three months or more, and of delayed onset if at least six months after the stressor event.
These timings also correspond with those used in the Cochrane systematic review on psychological debriefing for preventing post-traumatic stress in the general population (Rose 2002).

Search methods for identification of studies

The Cochrane Depression, Anxiety and Neurosis Review Group's Specialised Register (CCDANCTR)

The Cochrane Depression, Anxiety and Neurosis Group (CCDAN) maintains two clinical trials registers at their editorial base in Bristol, UK: a references register and a studies based register. The CCDANCTR-References Register contains over 37,000 reports of RCTs in depression, anxiety and neurosis. Approximately 60% of these references have been tagged to individual, coded trials. The coded trials are held in the CCDANCTR-Studies Register and records are linked between the two registers through the use of unique Study ID tags. Coding of trials is based on the EU-Psi coding manual, using a controlled vocabulary. Reports of trials for inclusion in the Group's registers are collated from routine (weekly), generic searches of MEDLINE (1950 on), EMBASE (1974 on) and PsycINFO (1967 on); quarterly searches of the Cochrane Central Register of Controlled Trials (CENTRAL) and review specific searches of additional databases. Reports of trials are also sourced from international trials registers using the World Health Organization International Clinical Trials Registry Platform portal (ICTRP), pharmaceutical companies, the handsearching of key journals, conference proceedings and other (non-Cochrane) systematic reviews and meta-analyses.
Details of CCDAN's generic search strategies (used to identify RCTs) can be found on the Group's website.

Electronic searches

1. The CCDANCTR-Studies Register was searched using the following controlled search terms (to 4 March 2015):
Intervention = (debriefing or "crisis intervention" or counsel*) and Concomitant Health Condition = childbirth

2. The CCDANCTR-References Register was searched using a more sensitive set of free-text terms to identify additional untagged/uncoded reports of RCTs (to 4 March 2015):
Free-text =((postpartum or "post partum" or post-partum or postnatal or "post natal" or perinatal or "peri natal" or puerper* or parturition or *birth* or childbirth or caesarean or caesarean or labour or labor) and ((debrief* or "crisis intervention*" or counsel*) or (*trauma* and prevent*)))
Earlier searches included the following terms.
CCDANCTR-Studies: Intervention = Debriefing or "Crisis Intervention" or Intervention = Counselling and Duration of treatment = "1 session"
CCDANCTR-References: Free-text = (debrief* or "crisis intervention*" or "trauma* stress" or "trauma* event" or catastroph* or emergenc*)
3. Additional searches were carried out on the following bibliographic databases (Appendix 1): CENTRAL (all years to 25 March 2013); MEDLINE (1946 to week 4 October 2014); EMBASE (1980 to 2013 week 12); PsycINFO (1806 to week 5 July 2013); Maternity and Infant Care (previously MIDIRS) (1971 to July 2013); CINAHL (1985 to 2013).
No restrictions on date, language or publication status were applied to the searches. The results of searches were screened for those dealing with childbirth.
To evaluate use of healthcare resources, the results of searches were examined using the following key words: "Economic evaluation", "Economic analysis", "Cost-benefit", "Cost-effectiveness", "Cost-consequences", "Cost", "Price", "Service use/ utilisation".

Searching other resources

Grey literature

Journals and conference proceedings specifically relating to mental health and the prevention of psychological trauma in women following childbirth were searched using the following bibliographic databases:

- Web of Science Conference Proceedings Citation Index (all years);
- Open Grey (all years).

Reference lists - handsearching

Reference lists of all included studies, previous systematic reviews and major textbooks of stress disorders were checked for published reports and citations of unpublished research.

Correspondence

The authors of significant papers and other experts in the field were contacted to identify additional studies, published or unpublished.

Data collection and analysis

Selection of studies

Two review authors (MHB and DB) independently screened titles and abstracts for inclusion of all the potential studies we identified as a result of the search and coded them as 'retrieve' (eligible or potentially eligible or unclear) or 'do not retrieve'. We retrieved the full-text study reports or publications and three review authors (MHB, DB and MF) independently screened the full text and identified studies for inclusion and identified and recorded reasons for exclusion of the ineligible studies. We resolved any disagreement through discussion or, if required, we consulted another author (RS). We identified and excluded duplicate records and we collated multiple reports that related to the same study so that each study rather than each report was the unit of interest in the review. We recorded the selection process in sufficient detail to complete a PRISMA flow diagram and a Characteristics of excluded studies table.

Data extraction and management

We utilised standardised data extraction forms to extract all available data. Data extraction was independently completed by three review authors (MHB, DB and MF) using a data extraction form which included verification of study eligibility; sample size; diagnostic criteria used; nature, timing and duration of debriefing intervention; number and frequency of sessions; type of professional delivering the intervention; intervention components; control components; outcomes (primary and secondary measures); and reported statistics, length of follow-up, number of participants lost or excluded at each stage of the trial. Data were entered into Review Manager 5.2 (RevMan 2012) by one review author (MHB) and checked by another author (MF).

Main planned comparisons

- Standard postnatal care with debriefing versus standard postnatal care without debriefing.

Assessment of risk of bias in included studies

Assessment of risk of bias in the included studies was conducted by two independent review authors (DB and MF) using the following domains from the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2008).
(a) Sequence generation
We assessed the method used to produce comparable intervention and control groups. We also investigated possible selection bias that might have been introduced due to an inadequate allocation sequence.
(b) Allocation concealment

We assessed possible selection bias by checking the method used to conceal the allocation sequence and whether intervention allocation could have been foreseen in advance of, or during, recruitment.

(c) Blinding of participants and personnel

We assessed possible performance bias by describing all the methods used, if any, to blind study participants and personnel (for example intervention providers) from knowledge of which intervention women received.

(d) Blinding of outcome assessors

We assessed possible detection bias by describing all the methods used, if any, to blind the outcome assessor from knowledge of which intervention women received.

(e) Incomplete outcome data

Possible attrition bias caused by withdrawals, dropouts, or protocol deviations was assessed by checking whether the level of missing data and reasons for missing data were balanced across groups.

(f) Selective reporting

Within-study selective outcome reporting was assessed by checking whether all pre-specified study outcomes were adequately reported, particularly in cases when non-significant results were mentioned but not reported (which may result in overestimation of the effect of the intervention in a meta-analysis). We also checked whether there were cases in which outcomes not specified prior to the study were reported. We assessed these potential sources of bias by comparing the outcomes listed in the methods section of an article with the reported results, if protocols were not available.

(g) Other sources of bias

We assessed whether there were any other possible sources of bias which were not addressed in the other domains mentioned above, for example issues such as adherence to study protocol or imbalances in the intervention and control groups at baseline.

For each item, one of the following three judgements were made: 'low risk of bias' (plausible bias, unlikely to seriously alter the results); 'high risk of bias' (plausible bias that seriously weakens confidence in the results); or 'unclear risk of bias' (plausible bias that raises some doubt about the results). We resolved any disagreement by discussion with all review authors until consensus was reached. The support for the judgements made is included in the risk of bias tables.

Measures of treatment effect

Dichotomous data

For dichotomous outcomes, such as the presence of psychological trauma, depression, anxiety or fear of childbirth caseness, the Mantel-Haenszel method for computing the pooled risk ratio (RR) with 95% confidence interval (CI) was used.

Continuous data

For continuous outcomes, the mean difference (MD) and 95% CI were calculated where all outcomes were measured using the same scale or there was only one trial. Where different scales had been used, the standardised mean difference (SMD) and 95% CI were calculated.

Both dichotomous data (prevalence) and continuous data (severity) were presented for each outcome, where data were available. We calculated numbers needed to treat to benefit (NNTB), to prevent one woman developing psychological stress, for high-risk and low-risk groups. We used Review Manager 5.1 for statistical analysis.

Unit of analysis issues

Trials with multiple treatment groups

If trials with multiple intervention or control groups were identified, recommendations from the *Cochrane Handbook for Systematic Reviews of Interventions* would have been followed (Higgins 2008). The possible solutions would have included combining groups to create a single pair-wise comparison. We would have described the nature of multiple intervention comparisons in the Characteristics of included studies table.

Cluster-randomised trials

If cluster randomised trials were identified, we would have consulted a statistician and analysed data following the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2008). The process would have included checking for correct analysis by cluster, for example using an estimate of the intra-cluster correlation co-efficient (ICC) from the trial or from a study of a similar population.

Dealing with missing data

We would contact investigators or study sponsors in order to verify key study characteristics and obtain missing numerical outcome data where possible (for example when a study was identified as abstract only). We would document all correspondence with trialists and report which trialists responded.

Where data could not be obtained for dichotomous outcomes, we conducted intention-to-treat (ITT) analysis by imputing outcome for the missing participants with the most optimistic scenario (that is all missing participants had negative outcomes) and with the most pessimistic scenario (that is all missing participants had positive outcomes). Sensitivity analyses were conducted to examine potential bias caused by missing data by comparing results from the ITT analysis with imputation and 'available case' analysis (that is analysing data with participants whose outcomes were known and excluding any participants whose outcomes were missing from

the denominator for each outcome in each trial) (Higgins 2008). If these analyses yielded similar results in terms of the effects of the treatment, the results of available case analyses were presented. Where data were missing for continuous outcomes, available case analyses were conducted. Where data were missing for standard deviations, we planned to calculate them from standard errors (SEs), CIs and t-test values.

Assessment of heterogeneity

We initially examined the forest plots to assess the possibility of statistical heterogeneity. We also used the I^2 test to investigate heterogeneity between studies. Following the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2008), we assessed the heterogeneity as:

- 0% to 40%, might not be important;
- 30% to 60%, may represent moderate heterogeneity;
- 50% to 90%, may represent substantial heterogeneity;
- 75% to 100%, considerable heterogeneity.

In addition, we regarded heterogeneity as substantial if the P value was less than 0.10 in the Chi² test for heterogeneity.
If a substantial level of heterogeneity was identified among trials, we planned to explore the reasons for this by pre-specified subgroup analysis.

Assessment of reporting biases

We planned to create funnel plots to investigate the possibility of publication bias if there were more than 10 included studies. However, it should be noted that asymmetry in funnel plots can be caused by other issues as well as reporting bias.

Data synthesis

Trials were categorised according to key differences; for example, different intervention types, methods used to diagnose psychological trauma (rating scales, self-report questionnaires, interviews), the timing of the intervention and of follow-up in relation to childbirth.
Random-effects model meta-analyses were used for data synthesis because it was expected that the population and setting were likely to be slightly different across studies, so it would make more sense to assume that the treatment effects were slightly different from study to study.
The*Cochrane Handbook for Systematic Reviews of Interventions* states that with the more common positive skewness, presentation of a geometric mean with its 95% CI is equivalent to an analysis of a log transformation of the data (Higgins 2008). However, log-transformed and untransformed data cannot be combined in a meta-analysis. The *Cochrane Handbook for Systematic Reviews of Interventions* also states that skewness is not necessarily a problem for meta-analyses in RevMan if the sample sizes in the individual studies are large. Where we had a small sample size for the specific meta-analysis and skewed data we have stated that interpretation of the outcomes should be treated with caution, with an explanation of the rationale for this. When data could not be statistically combined for a meta-analysis, extracted data were synthesised into a narrative summary.

Subgroup analysis and investigation of heterogeneity

We intended to perform subgroup analyses, where possible, for selected key demographic, psychosocial, obstetric and health data. Five a priori subgroup analyses were originally planned.

1. The effectiveness of the nature of specific types of psychological interventions e.g. stress debriefing (as detailed above, a formal critical incident debriefing operationalised in eight steps), non-directive counselling (the opportunity for the woman to talk through her story with a skilled and qualified facilitator), supportive listening (an opportunity for the woman to talk through her story with a health professional, and possibly to ask questions about the birth where she is unclear).
2. The effectiveness of the intervention mode e.g. individual versus group-based interventions.
3. The effectiveness of intervention frequency e.g. single session versus multiple session interventions.
4. The effectiveness of selection of the trial population e.g. universal, selective or indicated.
5. The effectiveness of the timing of intervention onset in relation to childbirth.

Sensitivity analysis

We planned to perform sensitivity analyses to evaluate the effects of exclusion of trials that were judged to have a high risk of bias for one or more of the domains of random sequence generation, allocation concealment, blinding and outcome reporting from the meta-analysis of the primary outcome.

Summary of findings table

A 'summary of finding' (SOF) table was produced using GRADEprofiler (GRADEpro) (Higgins 2008). In the SOF table, quality ratings for a body of evidence were made for each of the seven important outcomes up to three months (that is prevalence of psychological trauma, depression or depressive symptoms, anxiety, fear of childbirth etc.) regardless of whether the data were available or not. The quality rating was downgraded from the highest quality for RCT evidence to moderate, low, or very low quality evidence depending on the presence of five factors of risk of bias. In general, the quality rating fell by one level for each factor up to a maximum of three levels for all factors. When there were less than 100 people in a meta-analysis or study, or with fewer than 100 events, we downgraded the evidence two levels to 'low quality evidence' due to that factor alone.

RESULTS

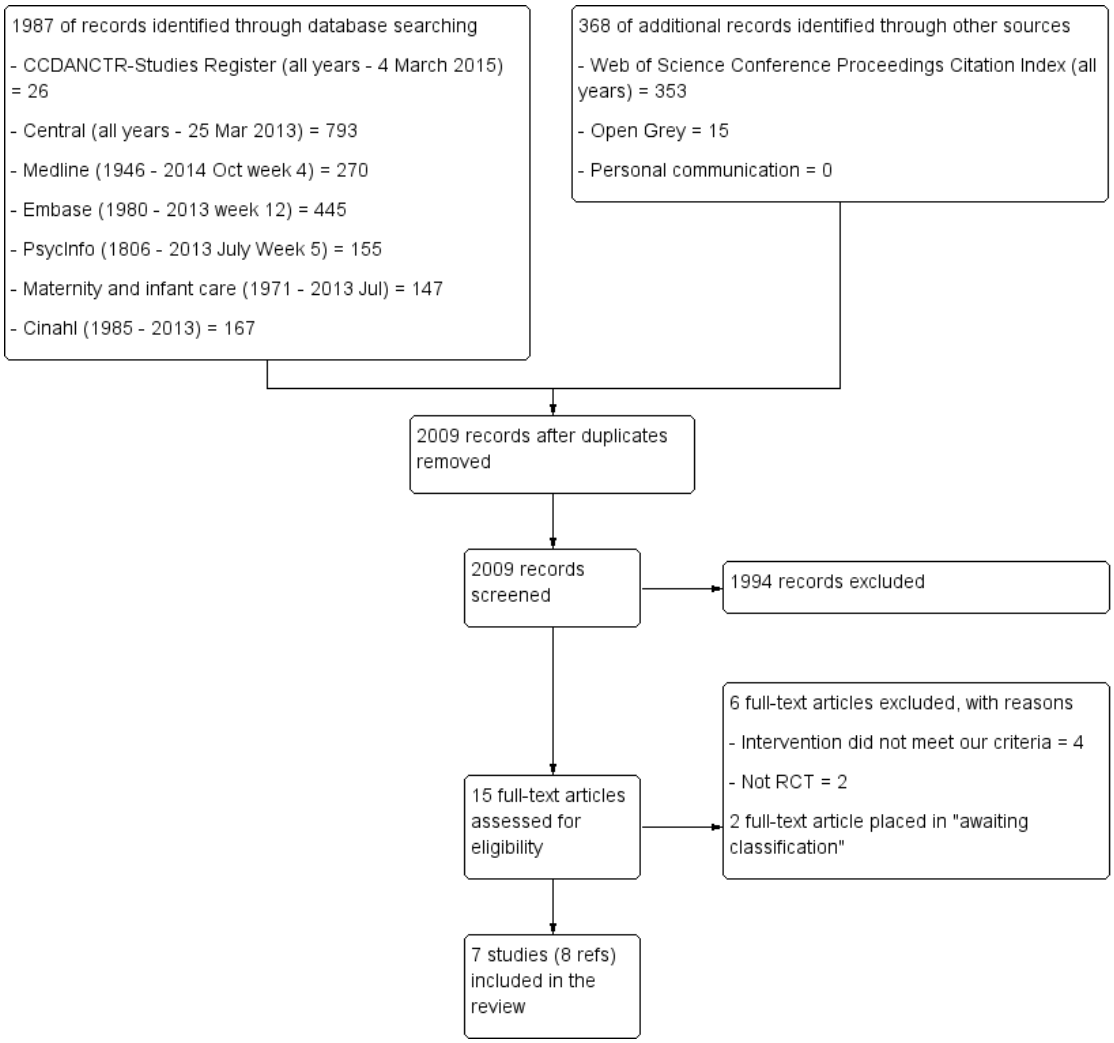
Description of studies

See: Characteristics of included studies; Characteristics of excluded studies; Characteristics of ongoing studies.

Results of the search

We identified 2009 papers after excluding duplicate articles using the bibliographic software programme EndNote (version X6). Initial screening based on a review of the titles, abstracts and keywords revealed 1994 studies not relevant on the basis of the inclusion and exclusion criteria. Full-text versions were obtained for the remaining 15 studies (21 papers). From these, we included seven trials (representing eight papers) and excluded six studies.
We identified two studies awaiting classification.
See Figure 1 for a PRISMA flow diagram depicting the study selection process.

Figure 1. Study flow diagram Figure 1.



Included studies

Seven trials were included in this review (Gamble 2005; Kershaw 2005; Lavender 1998; Priest 2003; Ryding 1998; Ryding 2004; Small 2000). Information from a study which followed up women in the original trial by Small 2000 was also included. There were no disagreements between review authors about trials to be included. Full details of the studies are presented in the Characteristics of included studies table. When further information was required we wrote to the study authors, but we received no response.

Design

All seven trials were described as 'randomised'. Ryding 2004 randomised women on 18 predetermined days of the month to the intervention and the remainder to the control. Ryding 1998 used an alternate assignment approach, in which every second emergency caesarean section patient was selected for the intervention group and the rest were selected for the comparison group. For the purposes of this review, these studies were regarded as quasi-randomised.

Sample sizes

The number of women included in the studies ranged from 102 to 1745.

Setting

All studies were conducted in high-income countries. Two studies were undertaken in the UK (Kershaw 2005; Lavender 1998), two in Sweden (Ryding 1998; Ryding 2004) and three in Australia (Gamble 2005; Priest 2003; Small 2000). All trials recruited participants in hospital settings.

Participants

No trials included all women following birth; with some pre-specified selection of participants in all trials. One trial (Priest 2003) did recruit women who gave birth to healthy infants at or near term (≥ 35 weeks gestation), which were the majority of the cases, and included women with any mode of birth; for the purposes of this review they were classified as 'universal' participants. Five trials (Kershaw 2005; Lavender 1998; Ryding 1998; Ryding 2004; Small 2000) selected participants by level of obstetric intervention during labour and birth and the mode of birth. For example, Lavender only recruited women with singleton pregnancies in cephalic presentations in spontaneous labour at term who proceeded to normal vaginal birth of a healthy baby (classified as 'selected - low level of obstetric intervention'). Of the other four trials, the intervention was offered to women who had a high level of obstetric intervention, that is women who had operative birth (forceps or vacuum assisted vaginal birth or emergency caesarean section) (Kershaw 2005; Small 2000) or women who gave birth to a live infant by emergency caesarean section (Ryding 1998; Ryding 2004). These trials were classified as 'selected - high level of obstetric intervention'. One trial (Gamble 2005) was classified as 'indicated', as selection was based on women who had trauma symptoms following birth.

It is important to note that all included trials excluded women who had a stillbirth or neonatal death in the index pregnancy. The majority of the trials also excluded women whose babies were admitted to neonatal intensive care (Kershaw 2005; Lavender 1998; Priest 2003; Ryding 2004; Small 2000) or who were critically ill themselves (Kershaw 2005; Lavender 1998; Ryding 1998; Small 2000). Two trials (Kershaw 2005; Lavender 1998) based selection on parity, including women who had given birth to their first child. In addition, some trials had exclusion criteria based on the woman's age (Gamble 2005; Small 2000). All included trials excluded women who had insufficient ability to communicate in the native language of the study setting (that is English in the United Kingdom(UK) and Australia and Swedish in Sweden); however, one trial (Lavender 1998) did not clearly report whether women were excluded based on language ability.

Interventions

Types of population

As described above, interventions in included trials were offered to women in subgroups: 'universal' (Priest 2003), 'selected - low level of obstetric intervention' (Lavender 1998), 'selected - high level of obstetric intervention' (Kershaw 2005; Ryding 1998; Ryding 2004; Small 2000) and 'indicated' (Gamble 2005). All women recruited were receiving usual postnatal care. No trials recruited women seeking treatment for psychological trauma.

Approaches to the debriefing intervention

In three trials (Gamble 2005; Kershaw 2005; Priest 2003) the debriefing intervention adopted the seven key stages from the critical incident stress debriefing model of Mitchell 1983, or incorporated an element of Mitchell's model. The remaining four trials used a less structured approach (Lavender 1998; Ryding 1998; Ryding 2004; Small 2000). For example, the debriefing intervention in Small 2000 provided women with an opportunity to discuss their experiences and concerns related to their labour, birth and post-delivery, but the contents of the debriefing were determined by the women. Similarly, Lavender 1998 used an interactive approach in which women were encouraged to speak freely and openly about their experience of their labour, ask questions, and explore their feelings. In four trials (Gamble 2005; Lavender 1998; Priest 2003; Small 2000), debriefing interventions were delivered by research midwives, while community midwives were used in Gamble 2005. In Ryding 1998, a consultation session was provided by an obstetrician with a primary psychotherapy qualification. The group

sessions in Ryding 2004 were facilitated by a maternity and child welfare psychologist and an experienced delivery ward midwife.
Six out of the seven included trials implemented interventions targeted at individuals, with only one trial including a group-based intervention in which four to five women were invited to group counselling sessions (Ryding 2004). All trials included a face-to-face intervention for the first session, and in trials with multiple sessions the debriefing intervention included face-to-face (Kershaw 2005; Ryding 1998; Ryding 2004) or telephone contact (Gamble 2005) for the subsequent sessions. The duration of the debriefing intervention varied between and within trials. For example, the debriefing in Priest 2003 took from 15 to 60 minutes, while Lavender 1998 spent 30 to 120 minutes with each woman. In the group sessions in the study by Ryding 2004, the consultations lasted for two hours each.

Frequency of intervention

Three trials included a single session intervention (Lavender 1998; Priest 2003; Small 2000) and the remaining four trials included more than one intervention session; there were two sessions in Gamble 2005, Kershaw 2005 and Ryding 2004, and three to four sessions in Ryding 1998. The length of time to the timing of the second session varied. Kershaw 2005 included a second debriefing session in the woman's home at 10 weeks, and in Gamble 2005 women were contacted by telephone at four to six weeks. Ryding 2004 invited women to a second session at approximately two months post-birth (p. 21). In Ryding 1998 the second session took place before women were discharged from hospital (no exact time was stated), following caesarean section; the third session was about two weeks postpartum; and the fourth, if necessary, occurred at about three weeks postpartum. However, the number of psychological debriefing sessions offered to women prior to each assessment time point was unclear for the primary outcome in some trials.

Onset timing of intervention

Some interventions were administered within 48 hours of birth (Small 2000), and others took place within 72 hours (Gamble 2005; Priest 2003). Kershaw 2005 commenced their intervention at 10 days and Ryding 2004 at one month post-birth. Two trials (Lavender 1998; Ryding 1998) did not mention the exact timing of the intervention, but based on available information it appeared to happen within a few days following childbirth. For example, Lavender 1998 reported "all eligible women received the intervention before being transferred into the community" (p. 216), while Ryding 1998 reported that "the counsellor booked the woman for a consultation at the maternity ward as soon as practicable following operative birth" (p. 233).

Comparisons

All included studies compared outcomes between debriefing and standard postnatal care groups. In one trial (Kershaw 2005) the comparison was standard postpartum care plus 'normal' debriefing versus standard postpartum care and debriefing. While the term 'normal' debriefing (control) was used as opposed to 'debriefing' (intervention) for the comparison, it was considered appropriate to consider 'normal' debriefing as a part of the standard postnatal care as it was described by the authors as "the doctor at delivery giving information and answering questions and the community midwife asking about the birth on her first visit" (p.1505). From the study description, it appeared that the procedures did not involve a formal debriefing such as the normalisation of a woman's emotional reaction to that experience, which is an important element of debriefing. Therefore, it was considered appropriate to include Kershaw's study as one of the trials which compared standard postnatal care without debriefing versus standard care with debriefing. None of the other trials reported if women in the control group accessed psychological support.

Outcomes

Five trials (Gamble 2005; Kershaw 2005; Priest 2003; Ryding 1998; Ryding 2004) compared the prevalence of psychological trauma, that is PTSD or PTSD symptoms and the severity of the symptoms (primary outcome) between a group who received debriefing and a group who received standard care. Five trials (Gamble 2005; Lavender 1998; Priest 2003; Ryding 2004; Small 2000) examined the prevalence or severity of depression symptoms. Two trials (Gamble 2005; Lavender 1998) assessed the prevalence of anxiety, but neither of these trials assessed the severity of the symptoms. Only one included study (Ryding 2004) examined the presence of fear of childbirth, comparing the debriefing and standard care groups. One trial (Small 2000) compared the severity of general mental morbidity between the debriefing and non-debriefing groups. None of the included studies examined health service utilisation, attrition from treatment, or use of healthcare resources; the cost-effectiveness of debriefing could therefore not be assessed.

Excluded studies

Of the 1985 records screened, we excluded 1972 records based on the titles, abstracts and keywords (mainly due to ineligible population, intervention or outcomes on the basis of the inclusion and exclusion criteria).
The Characteristics of excluded studies table lists four additional trials which were identified as potentially relevant but did not meet the review inclusion criteria when the full-text paper was examined. We excluded two studies because they had a non-randomised design (Jotzo 2005; Meades 2011), and two because they did not satisfy the criteria for debriefing (Borghini 2014; Tam 2003). We also excluded one study because the intervention included 'infant redefinition' as part of the CBT (Shaw 2013). The remaining trial (Selkirk 2006) was excluded because the intervention involved the

comparison of low and high levels of medical interventions.

Studies awaiting classification

Two studies are awaiting classification (Gamble 2010; Taghizadeh 2008). In Gamble 2010, the trialists have only reported qualitative data to date. One published paper (Taghizadeh 2008) was translated from Farsi or Persian, but due to a lack of information on how the outcome of interest (PTSD) was defined, the review authors were unable to make an informed decision on whether the study should be included or excluded. The authors used 'category' of PTSD, such as lack of PTSD, mild, medium or severe, but did not provide an explanation as to how scores for each category were derived. Attempts were made to obtain information from the contact author for this study, however to date no response has been received. Based on the current assessment of this paper, inclusion or exclusion of data from the study is unlikely to impact on the review's conclusions.

Risk of bias in included studies

The overall quality of the studies in relation to other methodological and reporting issues was variable. However, of the seven studies included, no studies were assessed as low risk of bias across all seven domains of the assessment. Detailed information about the risk of bias in the individual studies is presented in Characteristics of included studies. The risk of bias graph and summary are presented in Figure 2 and Figure 3, respectively.

Figure 2. Risk of bias graph: review authors' judgements about each risk of bias item presented as percentages across all included studies.

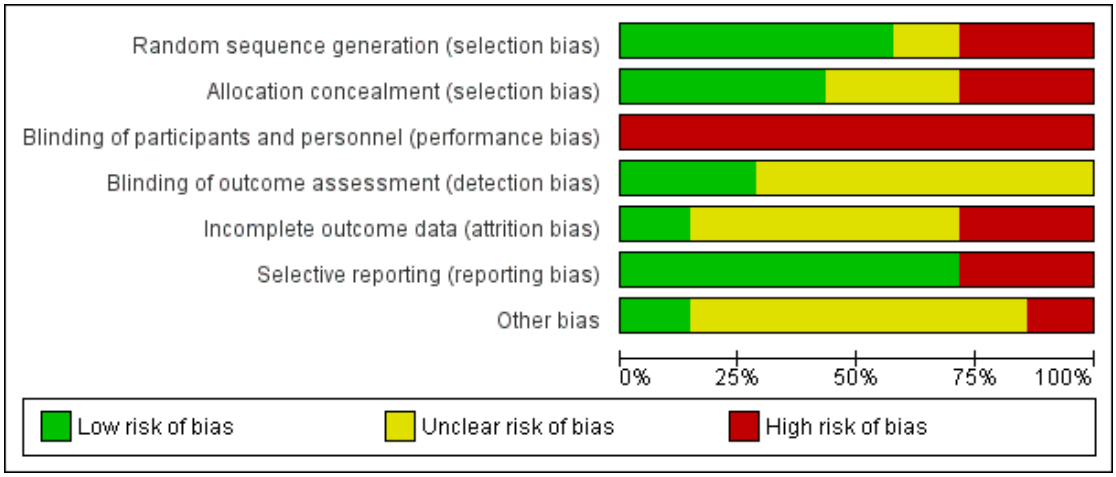
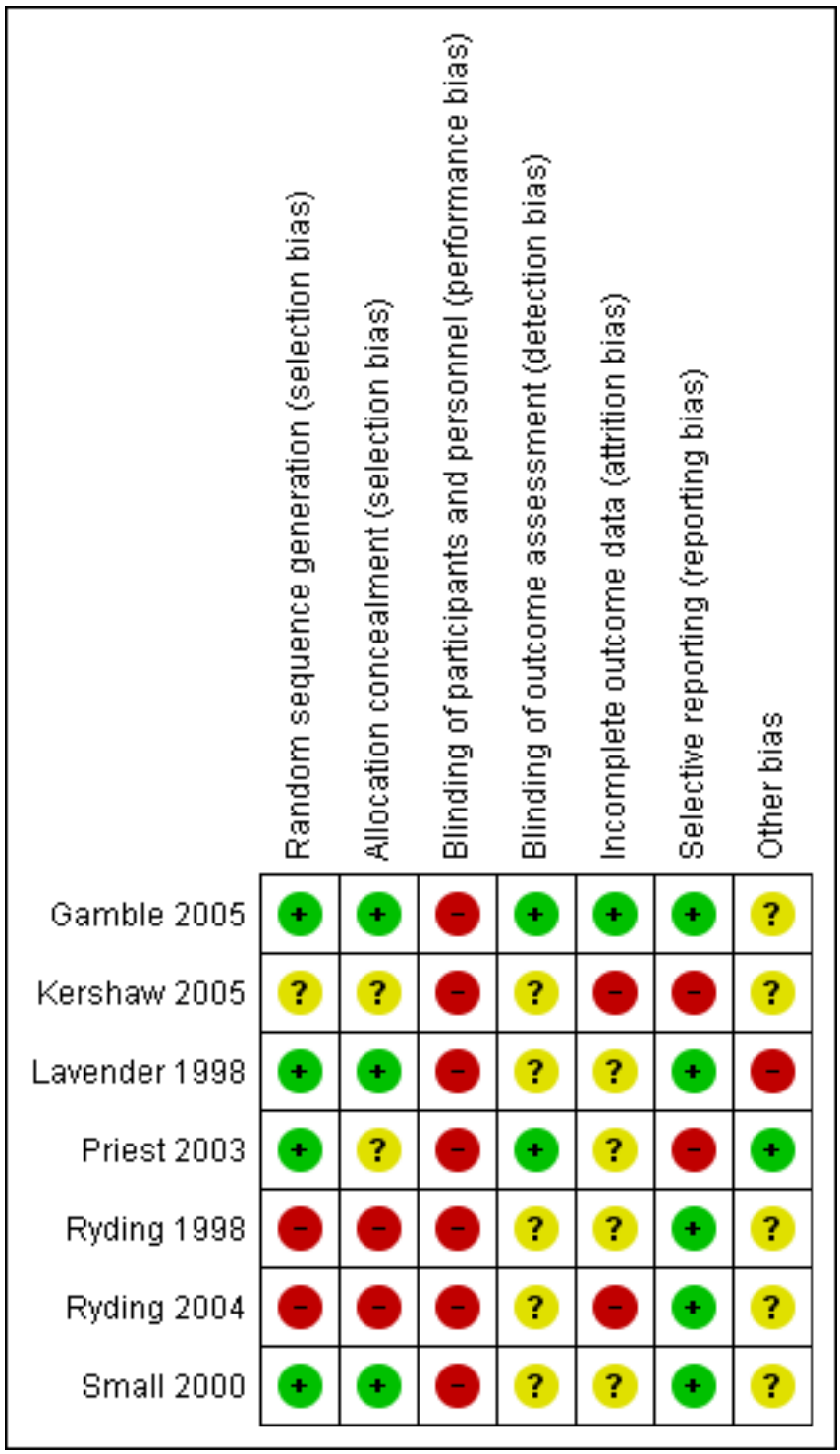


Figure 3. Risk of bias summary: review authors' judgements about each risk of bias item for each included study.



Allocation

Overall, there was a moderate risk of bias. Four trials (Gamble 2005; Lavender 1998; Priest 2003; Small 2000) used methods that appeared to result in sufficient or adequate sequence generation (computer-generated random numbers, centralised telephone randomisation). Two studies used systematic methods: an alternate assignment approach (Ryding 1998) and assignment based on dates (Ryding 2004), which have the risk of producing selection bias. In one study (Kershaw 2005) assessment of risk of bias was not possible as information was not clear about how the random sequence allocation was generated. As a method to reduce the chance of imbalance across important baseline characteristics between the intervention and control groups, one study (Priest 2003) used stratified (block) randomisation (parity, mode of birth). In the remaining studies information was not given about the method used for minimising baseline imbalance (for example simple, restricted, balanced random allocation), and five of the seven trials were small (n < 200).

In one study (Small 2000) allocation was determined using separate computer-generated randomisation, accessed by telephone. Three studies (Gamble 2005; Lavender 1998; Priest 2003) described using opaque sealed envelopes to conceal allocation. However, in one of these studies (Priest 2003) the participating woman selected an envelope from a pack of at least six envelopes containing random allocations, which might have introduced a certain bias (for example the preferences of the women and the clinician could have influenced the allocation). Another study used sealed envelopes but whether the envelopes were opaque or not was not adequately described (Kershaw 2005). In the studies by Ryding (Ryding 1998; Ryding 2004) the methods of concealment were considered inadequate because of the use of an alternate allocation or the use of predetermined days of the month for randomisation. In such systematic methods foreknowledge of group assignments among those recruiting women was unavoidable.

Blinding

Blinding of participants and personnel (performance bias)

There was a high risk of performance and information bias because blinding would not be possible for debriefing providers or recipients due to the nature of the intervention. Therefore, none of the included studies were free from performance bias. Two studies (Lavender 1998; Ryding 1998) clearly discussed unblinding issues stating, for example, "Women in the control group may have been disappointed not to receive an intervention that they perceived as being beneficial" (Lavender 1998) (pp. 217 to 218). These study authors also pointed out that women in the intervention groups may have offered more favourable answers to the debriefing providers who had spent time talking to them.

Blinding of outcome assessment (detection bias)

It was difficult to assess the risk of detection bias because the majority of the included studies did not provide full details on whether outcome assessors were blind to group allocation. In one trial (Gamble 2005) outcomes were assessed via a telephone interview with a researcher who was blinded to group allocation. One trial (Priest 2003) used postal questionnaires (entire sample of participants) and interviews (partial sample of participants). In the remaining studies postal self-administered questionnaires were used. Considering the nature of the outcomes (subjective), self-report questionnaires might not have been vulnerable to risk of bias (although these could not have been used as diagnostic criteria) if participants were assured that those who received the completed questionnaires were not involved in provision of the intervention or their care. However, except for one study (Priest 2003), it was not clear to whom the questionnaires were returned and whether recipients were blinded to group allocation. In addition, in the majority of included studies information was not provided on whether the individuals who would enter or analyse the data were blinded to the group allocation.

Incomplete outcome data

Overall, the level of attrition from included studies was unclear. In some trials loss to follow-up was greater than 20% (Kershaw 2005), and greater than 10% in the trial by Small 2000. In most studies the rate of loss to follow-up was similar in the debriefing and control groups. There was, however, one trial in which assessing the levels of attrition was difficult due to the lack of clear reporting of the number of women randomised to each arm (Lavender 1998). Some trials reported the potential systematic differences between women who stayed in the trial and those who withdrew. For example, in the Kershaw study (Kershaw 2005) women who did not return the questionnaires tended to be different from those who returned their questionnaires in terms of their socio-economic status (for example younger, single, ethnic minority) and with higher stress (for example baby in special care baby unit (SCBU), concealed pregnancy, mother ill). Ryding (Ryding 2004) reported that while 92% (82 out of 89) of women in the debriefing group returned follow-up questionnaires, 28% (23 out of 82) of these women did not actually attend the group counselling as intended. The reasons included women having no perceived need to attend the group meetings or women feeling unwell and unable to attend. Even where response rates were high, there were issues with missing data, particularly when a study used a postal self-administered

questionnaire. However, none of the studies provided sufficient information on missing data for each outcome and how they dealt with this in the analysis.

Selective reporting

Since study protocols were not accessible or available, formal assessment of reporting bias was difficult. However, there were unreported outcome data in two trials (Kershaw 2005; Priest 2003) that might result in reporting bias. In Priest 2003 the authors referred to no significant differences between the debriefing and control groups in prevalence and severity of PTSD symptoms or depressive symptoms as measured with self-report scales (the IES and EPDS respectively). As these data were not presented, they could not be included in the meta-analysis. The same authors also measured the proportion of women who met diagnostic criteria for PTSD or for major or minor depression in the year after giving birth, using DSM-IV criteria (APA 1994). These were measured in structured interviews conducted after screening for possible psychological disorders using self-reported measures (IES, EPDS, etc.) at a specific point in time (two months, six months or one year postpartum). Outcomes were not presented for each follow-up time but were presented 'in the year after giving birth', which appeared to be a total (or accumulated) number of new or existing PTSD or depression cases identified at any of the three assessment time points within the first year of the birth. These were classified as long-term outcome effects in this review, but interpretation requires caution as the onset and duration of disease might vary. In addition, Priest 2003 conducted subgroup analyses for women who underwent operative delivery, along with the total sample of women who gave birth to a healthy baby at or near term. However, no denominators for study comparison groups (debriefing versus without debriefing) were provided within subgroups.

Missing information on outcomes was also identified in Kershaw's study (Kershaw 2005). This study assessed the prevalence of PTSD symptoms at 10 days, 10 weeks and 20 weeks postpartum. However, data were only presented for 10 days postpartum (the risk of having PTSD symptoms is unlikely to be detected in such a short follow-up time, but it is probable that acute stress symptoms can be detected). The study also examined the severity of the fear of future childbirth using the Wijma Delivery Expectancy Scale (WDEQ), but there were unreported data on the SD or SE. We attempted to contact these authors (Kershaw 2005; Priest 2003) for more information but without success.

From the remaining studies, we did not find major problems related to reporting bias.

Other potential sources of bias

The majority of studies reported no significant baseline imbalances between the intervention and control groups after randomisation, although there were potential cases of underpowering due to small sample sizes. In some studies, substantial numbers of eligible women were not invited to participate or of the women who were invited a substantial number declined to participate. If the women who did not participate were systematically different from those who did, this may result in study bias and limit the generalisability of the study results. For example, in Lavender 1998 a high proportion of participants were single mothers, 68 were single and 43 were married. This study also reported high levels of psychological morbidity in the control group at three weeks postpartum, with half displaying high anxiety and over half reporting high depression scores (> 11) on the HADS. This might indicate potential bias or be related to the generalisability of the study findings.

In the Kershaw 2005 study, assessment of potential sources of bias due to imbalances in the intervention and control groups at baseline was difficult. While the study showed a lower risk of traumatic stress symptoms in the debriefing group at 10 days postpartum compared with the control group, it was unclear if the lower incidence of symptoms in the debriefing group was attributable to the intervention or indicated imbalances in the intervention and control groups at baseline. It appeared that the authors planned to measure traumatic stress symptoms at 10 days before the first debriefing session was provided to the intervention group. However, the authors noted that debriefing might have been initiated by community midwives on the first postnatal contact at home, before 10 days postpartum. Although traumatic stress symptoms at 10 days were treated in the review as the outcome rather than the baseline, the interpretation of the results requires caution as it could indicate a baseline imbalance between the intervention and control groups.

We were not able to assess the issues of adherence to the study protocol because of the lack of information (that is protocols, manuals or audit reports of studies were not accessible or available).

Effects of interventions

See: **Summary of findings for the main comparison**

In this section, we have reported the primary and secondary outcomes separately based on the seven trials that have contributed to a comparison of debriefing versus non-debriefing. The proportion and severity of outcomes, as well as outcomes according to time periods (short, medium and long-term postpartum), have been reported where data were available. Although subgroup analyses by trial population were planned as part of the review (by type of psychological intervention; and by mode, frequency and timing of psychological intervention), the results for the trial population only were stratified as there were insufficient data for subgroup analyses.

It was not appropriate in this review to separate single from multiple session debriefing sessions for the primary outcome as the trials which included multiple sessions were unclear about the number of sessions offered prior to assessment of the primary outcome. Future updates of this review should consider separating single

from multiple sessions, if the data support this.
Sensitivity analyses to deal with missing outcomes were conducted comparing the result of ITT with imputation analysis from available case analysis. The results were very similar, indicating that exclusion of participants with missing outcomes did not change the results. We have, therefore, presented the results of the available case analyses. Planned sensitivity analysis to evaluate the effects of the exclusion of trials rated as having a high risk bias could not be performed because only one or two studies contributed data.

Comparison 1: Standard postnatal care with debriefing versus standard postnatal care without debriefing

Primary outcomes

1.1 Presence of psychological trauma

Short term: up to three months postpartum

Differences in the prevalence of women's self-reported symptoms of psychological trauma were not statistically significant between the debriefing group (individual multiple debriefing sessions) and the non-debriefing group (RR 0.60; 95% CI 0.34 to 1.06) among those who had a high level of obstetric intervention during labour and birth (selected sample). Results, however, were based on two small trials (338 participants) that assessed symptoms at 10 days (Kershaw 2005) and four weeks postpartum (Ryding 1998). With such a small number of trials, statistical heterogeneity was not found. However, the point estimate of the intervention effect of these two trials was in the opposite direction (Analysis 1.1.). The baseline risk of prevalence of traumatic stress symptoms (control) was approximately 2% in Ryding 1998 and almost 40% in Kershaw 2005, indicating these trials were clinically or methodologically different from each other. It is important to note that Kershaw also assessed PTSD symptoms at 10 weeks postpartum, reporting no statistically significant difference between the groups, but these data were not available. One study (Gamble 2005) which included 102 women who experienced a distressing or traumatic birth (indicated sample) showed no statistically significant difference in diagnosis of PTSD (measured by the MINI-PTSD) in the intervention group with multiple individual debriefing sessions and those in the control group without debriefing, at four to six weeks postpartum (RR 1.15; 95% CI 0.66 to 2.01) (Analysis 1.1). No data were available for a universal sample. Priest 2003 (which included 1745 women who gave birth at or near term) appeared to measure diagnostic PTSD and post-traumatic stress symptoms using the Impact of Event Scale (IES) at two months postpartum (cut-off not known), and mentioned no significant differences between the intervention group (with an individual, single debriefing session) and the control group (with standard postnatal care without debriefing). However, data were not presented and it was not possible to include the results in our analysis.

Medium term: three to six months postpartum

Two trials (Ryding 1998; Ryding 2004) involving 252 women with high levels of obstetric intervention (selected sample) showed no evidence of a difference between the debriefing group and non-debriefing group at six months postpartum (RR 0.62; 95% CI 0.27 to 1.42) (Analysis 1.2.1). The interventions assessed in these two trials had multiple components, including two to four counselling sessions, but one trial included group (Ryding 2004) and the other included individual sessions (Ryding 1998). The point estimate of the effect in each trial was in the opposite direction, but statistical heterogeneity was not observed. One study (Kershaw 2005) compared the prevalence of PTSD symptoms at 20 weeks postpartum between women who had given birth to their first child by operative birth who received individual multiple debriefing sessions and those who received standard postnatal care. The study results could not be included in the meta-analysis as the data were not reported, possibly due to lack of statistical significance.
Only one study (Gamble 2005) examined the effect of debriefing among 103 women who had experienced a distressing or traumatic birth (indicated). Results showed no difference in the risk of diagnosis of PTSD measured by the MINI-PTSD between women allocated to the intervention group with individual, multiple debriefing sessions and those in the non-debriefing group at six months postpartum (RR 0.35; 95% CI 0.10 to 1.23) (Analysis 1.2.2).
One study (Priest 2003), involving 1574 women who gave birth at or near term (universal sample), appeared to measure psychological trauma symptoms at six months postpartum using the IES-Revised (IES-R) (cut-off point not known) and reported no significant differences between the intervention group (with an individual, single debriefing session) and the control group (with standard postnatal care without debriefing). However, no data were available and it was not possible to include the trial in the analysis.

Long term: more than six months postpartum

One trial (Priest 2003) involving 1745 women who delivered healthy infants at or near term (universal) reported no significant differences between the intervention group with an individual, single debriefing session and the control group with no debriefing in the proportion of women who met diagnostic criteria for psychological trauma in the year after giving birth (RR 0.71; 95% CI 0.23 to 2.23) (Analysis 1.3). As described earlier, the outcome appeared to be a total number of PTSD cases (it was unclear if these were new or existing cases) identified at any of the three follow-up

time points during a period of one year (two months, six months or one year postpartum).

Secondary outcomes

Where data were available on the prevalence (proportion) and severity of secondary outcomes, these have been reported separately.

1.2 Severity of psychological trauma

Short term: up to three months postpartum

There were no significant differences in self-reported stress symptoms at four to six weeks postpartum between debriefing (individual, multiple sessions) and control groups (MD -0.64; 95% CI -1.94 to 0.66) (Analysis 1.4). This result was based on one trial (Gamble 2005) involving 102 women who had experienced a distressing or traumatic birth (indicated).

The study by Priest 2003 involving women who gave birth at or near term (universal sample) mentioned no significant difference in the IES-R scores at two months postpartum between the intervention group (with an individual, single debriefing session) and the control group (with standard postnatal care without debriefing). However, as data were not presented it was not possible to include this study in the analysis in the current review.

No data were available for selected samples.

Medium term: three to six months postpartum

PTSD symptoms at three months postpartum were less severe in the intervention group with individual, multiple debriefing sessions compared to control; the difference was statistically significant (MD -1.29; 95% CI -2.47 to -0.11) (Analysis 1.5). This result was based on one study involving 103 women who had experienced a distressing or traumatic birth (indicated).

One study (Priest 2003) involving women who gave birth at or near term (universal sample) reported that there was no significant difference in the IES-R scores at six months postpartum between the intervention group (with an individual, single debriefing session) and the control group (with standard postnatal care without debriefing). However, as data were not available, it was not possible to include this study in the analysis in the current review.

No data were available for selected samples.

Long term: more than six months postpartum

There was one study (Priest 2003) involving women who gave birth at or near term (universal sample), which mentioned no significant difference in the IES-R scores at 12 months postpartum between the intervention group (an individual, single debriefing session) and the control group (standard postnatal care without debriefing). However, as data were not presented, inclusion in the analysis in the current review was not possible.

No data were available for selected and indicated samples.

1.3a Prevalence of depression or depressive symptoms

Short term: up to three months postpartum

There was a lower proportion of probable depression (as measured with the HAD scale > 11) at three weeks postpartum in women allocated to the debriefing group (individual, single session) than those receiving standard postnatal care without debriefing. The difference was statistically significant (RR 0.16; 95% CI 0.07 to 0.37) (Analysis 1.6). This result, however, was based on one trial with a sample of primiparous women (n = 114) with a low level of obstetric intervention during labour and birth (Lavender 1998).

One trial involving 102 women who had a distressing birth (Gamble 2005) showed no difference in the proportion of probable depression (EPDS $\geq$ 13) at four to six weeks postpartum in the women allocated to the debriefing group (individual, multiple sessions) compared with those receiving standard postnatal care without debriefing (RR 0.96; 95% CI 0.55 to 1.67) (Analysis 1.6).

One trial (Priest 2003) involving women who gave birth at or near term (universal sample) mentioned that there was no significant difference in depressive symptoms measured with the EPDS ($\geq$ 13) at two months postpartum between the intervention group (an individual, single debriefing session) and the control group (standard postnatal care without debriefing). As data were not presented, inclusion of this study in the analysis in the current review was not possible.

Medium term: three to six months postpartum

Two trials involving 1064 women who had a high level of obstetric intervention during labour and birth (selected samples) showed no difference between the intervention and standard postnatal care groups in the proportion of women with probable depression at six months postpartum (RR 1.00; 95% CI 0.56 to 1.79) (Analysis 1.7). This result was based on trials with potential clinical heterogeneity in terms of the type of intervention: one trial included a two session group debriefing intervention (Ryding 2004) and the other included a single individual debriefing intervention (Small 2000); there was moderate statistical heterogeneity (I^2 = 44%). Looking at the individual trials, both showed no evidence of either a positive or negative effect of debriefing on psychological trauma.

One trial with an indicated sample (women who experienced a distressing birth) reported no difference in the proportion of women with probable depression as measured with the EPDS (based on a score of $\geq$ 13) at three months postpartum between the debriefing group and non-debriefing group (RR 0.25; 95% CI 0.09 to 0.69) (Analysis 1.7).

One trial (Priest 2003) involving women who gave birth at or near term (universal sample) reported no significant difference in depressive symptoms as measured with the EPDS (based on a score of ≥ 13) at six months postpartum between the intervention group (an individual, single debriefing session) and the control group (standard postnatal care without debriefing). As data were not presented, inclusion of this study in the analysis in the current review was not possible.

Long term: more than six months postpartum

There was no statistically significant difference between women in the intervention group who received a single standardised debriefing session and those who received standard postnatal care in the risk of having depression (as diagnosed using the DSM-IV) within the first year of giving birth (RR 0.98; 95% CI 0.80 to 1.20) (Analysis 1.8.1). The result was based on one study (Priest 2003) involving 1745 women who gave birth to a healthy baby at or near term (universal sample). One study (Small 2000) involving 534 women who had an operative birth (selected) showed no difference between the debriefing and control groups in terms of the proportion of women with depression (EPSD ≥ 13) at four to six years after childbirth (RR 0.95; 95% CI 0.65 to 1.40) (Analysis 1.8.2). There were no data for the indicated sample.

1.3b Severity of depressive symptoms

Short term: up to three months postpartum

One study (Priest 2003) involving women who gave birth at or near term (universal sample) reported no significant difference in the EPDS scores at two months postpartum between the intervention group (an individual, single debriefing session) and the control group (standard postnatal care without debriefing). However, as data were not presented, this study could not be included in the analysis in the current review.
No data were available for selected and indicated samples.

Medium term: three to six months postpartum

No significant difference was observed in the severity of depressive symptoms based on EPDS scores between the single individual debriefing group and the non-debriefing group at six months postpartum (MD 0.44; 95% CI -0.28 to 1.16) based on one study involving 917 women who had an operative birth (Small 2000) (Analysis 1.9).
One study (Priest 2003) reported no significant difference in the EPDS scores at six months postpartum between the intervention group (an individual, single debriefing session) and the control group (standard postnatal care without debriefing) among women who gave birth at or near term (universal sample). As data were not presented, this study could not be included in the analysis in the current review.
No data were available for an indicated sample.

Long term: more than six months

No significant difference was observed in the severity of depressive symptoms based on EPDS scores between the debriefing (single individual session) and the control (non-debriefing) groups at four to six years after operative birth (MD -0.44; 95% CI -1.36 to 0.48) (Analysis 1.10). The result was based on one study only (Small 2000).
One study (Priest 2003) involving women who gave birth at or near term (universal sample) reported no significant difference in the EPDS scores at 12 months postpartum between the intervention group (an individual, single debriefing session) and the control group (standard postnatal care without debriefing). As data were not presented, this study could not be included in the analysis in the current review.
No data were available for universal and indicated samples.

1.4a Prevalence of anxiety

Short term: up to three months postpartum

A significant difference was observed between the debriefing and control groups at up to one month postpartum (RR 0.14; 95% CI 0.05 to 0.37) (Analysis 1.11) in the prevalence of anxiety measured with the HAD scale (≥ 11). The result was based on a single small study (Lavender 1998) involving 114 women who had a low level of obstetric intervention and vaginal birth of a healthy baby.
No data were available for universal and indicated samples.

Medium term: three to six months postpartum

There was no significant difference in the risk of having anxiety (as measured with DASS measurement of anxiety > 9) between debriefing (multiple individual sessions) and control groups at three months postpartum, although the point estimate of the effect was in favour of debriefing (RR 0.18; 95% CI 0.02 to 1.42) (Analysis 1.12). The result was based on one small sample (n = 103) study that involved women who had a distressing and traumatic birth experience (Gamble 2005).

Long term: more than six months

No data were available.

1.4b Severity of anxiety

No data were available.

1.5a Presence of fear of childbirth

There was no evidence of significant differences in the presence of fear of childbirth as measured with the W-DEQ(B) > 60 between the debriefing (multiple group counselling sessions) and standard care groups at six months postpartum; the result was based on only one study (Ryding 2004) that involved 147 women giving birth to a live infant by emergency caesarean section (RR 0.93; 95% CI 0.53 to 1.63) (Analysis 1.13).

1.5b Severity of the fear of childbirth

In one study (Kershaw 2005) no significant differences were reported between the debriefing and standard postnatal care groups in severity of fear of future childbirth at 10 days, 10 weeks and 20 weeks postpartum using the W-DEQ(A) scores. As data were missing on CIs, SD and SEs, inclusion in the analysis in the current review was not possible.

1.6a Prevalence of general psychological morbidity

No data were available for either the prevalence or severity of general psychological morbidity.

Short term: up to three months postpartum

No data were available.

Medium term: three to six months postpartum

One study (Small 2000) involving 917 women who had an operative birth (selected sample) reported no significant difference in general mental health scores (as measured with the SF-36 subscales) at six months postpartum between the debriefing (single individual session) and the control (non-debriefing) groups (MD -1.51; 95% CI -3.90 to 0.88) (Analysis 1.14).
No data were available for universal and indicated samples.

Long term: more than six months

No data were available.

1.6b Severity of general psychological morbidity (long term: more than six months postpartum)

Of 534 women who had an operative birth (selected sample), no significant difference was observed in the mental health outcome (as measured with the SF-36 mental health component summary scores) between the debriefing (single individual session) and the control (non-debriefing) groups at four to six years after operative birth (MD 1.80; 95%CI -0.02 to 3.62) (Analysis 1.15). The result was based on one study only (Small 2000). No data were available for universal and indicated samples.

1.7 Health service utilization

No data were available.

1.8 Attrition from treatment

No data were available.

1.9 Use of healthcare resources

No data were available.

DISCUSSION

Summary of main results

This review examined whether psychological debriefing prevents psychological trauma and other forms of psychological morbidity (for example depression, anxiety and fear) following childbirth, compared to standard postnatal care without debriefing. The review included seven trials conducted in three countries, the UK, Australia and Sweden, each involving 102 to 1745 women. In general, maternity care in these countries is similar with respect to safety and quality of care and organisation of services including routine provision of midwifery care for women whose pregnancies and labour are classed as low risk. We therefore concluded that combining the results from the included studies using meta-analyses was appropriate and clinically meaningful in terms of the context of maternity care. However, to date only a very small number of studies contributed to each outcome, sometimes just a single study; therefore, conducting meta-analysis was not always possible.

Psychological trauma

We found no robust evidence that debriefing reduced or increased the risk of developing psychological trauma during the postpartum period. Only one trial contributed to the outcome of severity of psychological trauma (Gamble 2005). This trial involved women who reported a distressing birth experience and compared a group who received a psychological intervention (that is midwife-led counselling within 72 hours and again at four to six weeks postpartum) with a control group. Results suggested that psychological debriefing did not have any significant effect on the severity of psychological trauma as measured at four to six weeks postpartum, but psychological trauma severity was statistically significantly lower in the debriefing group at three months postpartum. This might indicate that debriefing does not have an immediate effect on severity of symptoms but facilitates recovery from a distressing birth experience and subsequent psychological trauma by

three months postpartum. Alternatively, the difference between the two groups could be due in part to the number of counselling sessions women had received before each assessment. In either case, this finding was based on just one small trial conducted in a particular type of trial population (indicated), and data were not available for other types of trial populations.

Depression

Evidence of reduction in depression was found in the smallest two of the five trials, one a selected sample (Lavender 1998) with the outcome measured up to three months postpartum, and the other an indicated sample with the measurement at three to six months postpartum (Gamble 2005).

Only one study (Small 2000) contributed to data on the severity of depressive symptoms in the medium term (three to six months postpartum), and it showed no significant difference between the debriefing and standard postnatal care groups for women experiencing an operative birth. The same trial also showed no difference in the severity of depressive symptoms at four to six years postpartum between the debriefing and control groups.

Anxiety

From two albeit small trials, the overall estimate of the effect of debriefing on the risk of developing anxiety following birth was in favour of debriefing over standard care (Gamble 2005; Lavender 1998). One trial (Lavender 1998) showed that women's self-reported anxiety in the debriefing group was almost halved compared to the control group in the short term for women who were obstetrically at low risk (that is experiencing spontaneous labour and vaginal birth of healthy term babies). The other trial (Gamble 2005) among women who had distressing and traumatic births showed no evidence of a lower risk of anxiety in the debriefing group compared to the no debriefing group at three months follow-up.

Fear of childbirth

Based on a single trial (Ryding 2004), no evidence was found that debriefing reduced the prevalence of fear after childbirth, as measured by the WDEQ-B. One trial also showed no significant difference in severity of fear of future childbirth using the WDEQ-A.

General psychological morbidity

Only one trial (Small 2000) contributed to the outcome of general psychological morbidity, showing no evidence of a favourable effect of debriefing on women's self-reported mental health as measured using the SF-36 mental health component scores.

Finally, no evidence was found in the current review that psychological debriefing had any statistically significant adverse effects on selected outcomes.

Overall completeness and applicability of evidence

We were unable to determine the effectiveness and safety of psychological debriefing because of the lack of high quality evidence and substantial heterogeneity between studies. We originally proposed we would conduct subgroup analyses to investigate heterogeneity. However, this was not possible because of the small number of studies as well as the lack of a description of the interventions. Studies included in this review were undertaken during the last two decades (1996 to 2004) in high-income countries, but information on study timing and duration was not always given (Lavender 1998; Ryding 1998). Study samples in all trials included in this review were recruited in hospitals, with the findings only applicable to similar contexts in high-income countries. There is some uncertainty about whether the tools measuring trauma are relevant for a postnatal population and the optimal time to implement an intervention, with the included studies widely differing in the timing and frequency of the intervention of interest. The IES, which was the most widely used tool for self-assessment of psychological trauma, was developed and validated for the general population and there might be limitations to applying this tool to the childbearing population.

No study evaluated general psychological morbidity, health service utilisation, attrition from treatment and use of healthcare resources.

Quality of the evidence

Although evidence generated in this review was based on five RCTs and two quasi-randomised studies, the quality of the body of evidence was generally low when assessed using the GRADE criteria (study limitations, consistency of effect (heterogeneity), imprecision, indirectness, and publication bias). The quality of the evidence for the prevalence of psychological trauma (primary outcome) and the prevalence of depression symptoms was rated low or very low, based on few studies (one to three studies) with high risk of bias in the main domains such as performance bias, random sequence generation, allocation concealment and incomplete outcome data. The quality of evidence for the remaining outcomes (that is the prevalence of anxiety, prevalence of fear of childbirth, prevalence of general psychological morbidity, health service utilization and attrition from treatment) could not be assessed as data were not available. Details of our judgements are shown in the 'Summary of findings' tables and described below.

Study limitations

As described earlier, trials included in this review to assess the effects of debriefing for each outcome had a number of methodological limitations. These included inadequate random sequence generation, lack of allocation concealment, lack of blinding and incomplete outcome data. Therefore, when each outcome was assessed, most of the information was based on studies with unclear, or sometimes high, risk of bias that weakened our confidence in the estimate of the effect of debriefing.

Consistency of effect - clinical and statistical heterogeneity

We have presented outcomes separately according to type of trial population, where possible. The assessment of consistency of effect was thus not always applicable due to the frequent contribution of just a single trial for many outcomes. Statistical heterogeneity (measured with $I^2 \geq 50$) was not observed, but there might be clinical heterogeneity for the primary outcome of prevalence of postpartum psychological trauma in the short term, which was evaluated based on two trials (Gamble 2005; Kershaw 2005). In Kershaw's study, post-traumatic symptoms were assessed at 10 days postpartum (by definition, a diagnosis of "PTSD requires more than one month of symptoms" according to DSM-IV-TR, p. 471) using a self-report scale (IES > 19), while Gamble 2005 measured it at four to six weeks postpartum using diagnostic criteria for PTSD. Different measurements and follow-up times might tell us different stories as psychological trauma could resolve naturally within one month without any psychological intervention.

Imprecision of results

As described earlier, the estimate of effect for most outcomes was based on a single, and often small, study that gave wide CIs. Even when effect sizes were based on more than one trial, sample sizes were still frequently small, which again resulted in wide CIs. The quality of the evidence for most outcomes was downgraded due to the imprecision of results.

Indirectness of evidence

All studies included in this review compared the effectiveness of debriefing with standard postnatal care. Thus, indirectness of evidence was not an issue.

Publication bias

Due to the small number of trials included in this review, we were unable to assess possible publication bias using funnel plots.

Potential biases in the review process

To minimise bias and issues related to subjectivity of judgement, any disagreements that occurred in the reviewing process were discussed among all review authors until a consensus was reached. Three review authors independently carried out data extraction. The accuracy of the data was further checked by the third review author. Potential risk of bias in each study and the overall quality of evidence of each outcome were assessed by two independent review authors. We adopted a highly sensitive search strategy. However, the literature identified was predominantly written in English and most studies were from high-income countries.

Agreements and disagreements with other studies or reviews

Two Cochrane reviews, one on psychological debriefing for preventing PTSD in the general population (Rose 2002) and one on multiple session early psychological interventions for the prevention of PTSD (Roberts 2009), have been published. The current systematic review sought to address the evidence gap by reviewing trials that used debriefing interventions to prevent psychological trauma in women following childbirth. The findings of this review that there is a lack of robust evidence for the effectiveness of individual debriefing on PTSD or PTSD symptoms were consistent with earlier reviews by Rose 2002 and Roberts 2009. In contrast with the findings of Rose 2002 and Roberts 2009, which indicated potential harmful effects of debriefing (for example increased self-reporting of PTSD symptoms), we did not find any significant evidence that debriefing has adverse effects on psychological trauma and other psychological morbidity following childbirth. This may indicate that the potential for debriefing to do harm may be different according to the nature of the trauma or study population (obstetric and postnatal population, general population etc.).

AUTHORS' CONCLUSIONS

Implications for practice

We did not find any high quality evidence to inform practice, with research conducted to date being too varied to provide consistent findings to support either a positive or adverse effect of formal psychological debriefing for women following birth. In line with clinical practice guidelines in the UK, including NICE guidance on routine postnatal care (NICE 2015) and antenatal and postnatal mental health (NICE 2007), and other Cochrane reviews on psychological interventions after trauma in the general population (Roberts 2009; Rose 2002), routine psychological debriefing for women after childbirth cannot be supported. This recommendation does not preclude other forms of postnatal discussion with women following birth, as currently recommended by NICE

(NICE 2015; NICE 2007), which are not intended to prevent PTSD and are not provided as a debriefing intervention. The impact of discussion between care providers and women post-birth was not included as an objective of this review.

Implications for research

1. The majority of trials (five out of seven) included in the current review had small sample sizes, and power calculations were not performed or were inappropriately performed in all but two trials. A large, pragmatic randomised controlled trial (RCT) is required to assess the effectiveness of psychological debriefing for the prevention of psychological trauma and subsequent psychological problems in women following childbirth, with short, medium and long-term follow-up.

2. The effectiveness of debriefing on these outcomes may differ according to the nature of the debriefing, such as single or multiple session, individual or group sessions, obstetrician or midwife-led debriefing, and face-to-face or telephone debriefing. More high quality RCTs with clarity as to the number of sessions offered within the intervention are needed to determine this.

3. The effects of debriefing may also differ according to population (universal, selected and indicated) and may be further influenced by differences in individual characteristics and wider social and environmental factors before, during and after birth. More trials of adequate size are needed in order to investigate whether specific types of debriefing interventions work or do not work for particular groups of women.

4. Future trials need to provide greater detail on the definition of outcome measures (e.g. self-report or clinical diagnosis) and the process of measuring them. When clinical diagnosis is not used, sophisticated measurement of psychological trauma in a postnatal population should be performed by means of a carefully selected and appropriately used self-report measurement tool. This is particularly important for PTSD or PTSD symptoms, as widely used and validated self-report measurement tools in the general population have not been validated for the postpartum population. There are currently few scales designed for measuring psychological trauma following childbirth, such as the Traumatic Event Scale (Wijma 1997) and the Perinatal Post-Traumatic Stress Disorder Questionnaire (DeMier 1996), but these scales have not been validated with clinical diagnostic interviews (Ayers 2008; Stramrood 2010).

5. High rates of obstetric intervention in labour and birth in some birth settings may mean that women require improved emotional care from health professionals to reduce the risk of childbirth being experienced as a traumatic experience, given a higher prevalence of psychological trauma among these women.

6. As all included trials excluded women who had insufficient ability to communicate in the native language of the study setting, there is no information on the response of these women to psychological debriefing.

7. No included studies were conducted in low or middle-income countries.

8. The review authors are aware of ongoing RCTs, the results of which will be incorporated into this review as soon as they are available.

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Disclaimer

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* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

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

Gamble 2005

Methods	RCT
Participants	A total of 103 women with trauma symptoms following birth (indicated) Setting: Three maternity teaching hospitals in Brisbane Inclusion: Women over 18 years of age, in the last trimester of pregnancy, expected to give birth to a live infant, and able to complete questionnaires and interviews in English Exclusion: Women experiencing stillbirth or neonatal death
Interventions	Comparison: Individual counselling with elements of critical stress debriefing (Mitchell 1983) versus postnatal care as usual. Multiple interventions: 1 session of face to face counselling (within 72 hours of birth on the postnatal ward) and 1 session of telephone counselling (at 4 to 6 weeks postpartum) lasting from 40 to 60 mins
Outcomes	MINI-PTSD, EPDS, DASS-21
Notes	The intervention model described in this study (counselling) incorporated elements of critical stress debriefing, pertinent to the context of childbearing. Postpartum EPDS scores of more than 12 (range 13 to 29) was much higher than in the general birthing population at 4 to 6 weeks, and higher in the control group than in the intervention at 3 months, indicating probable depression

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	"Computer-generated, random allocations"
Allocation concealment (selection bias)	Low risk	"Women...were randomized using sealed, opaque envelopes"
Blinding of participants and personnel (performance bias) All outcomes	High risk	Due to the nature of the intervention, the blinding of recipients and providers of debriefing is not possible or difficult
Blinding of outcome assessment (detection bias) All outcomes	Low risk	"A second research midwife, blinded to group allocation, conducted the 3-month follow-up telephone interview."
Incomplete outcome data (attrition bias) All outcomes	Low risk	No. of questionnaires returned/No. women randomised = 4 to 6 weeks postpartum Total: 99% (102/103); debriefing: 98% (49/50); control: 100% (53/53) 3 months postpartum Total: 100% (103/103); debriefing: 100% (50/50); control:

Gamble 2005 (*Continued*)

		100% (53/53)
Selective reporting (reporting bias)	Low risk	None detected
Other bias	Unclear risk	No significant baseline imbalances were detected between intervention and controlled groups after randomisation. "At 4 to 6 weeks postpartum, 34 women (33%) had a total EPDS score of more than 12 (range 13-29). This prevalence is much higher than the postnatal depression rates of the general birthing population (between 10-16%) reported in other studies."

Kershaw 2005

Methods	RCT
Participants	A total of 319 postnatal women (selected) Setting: one hospital in UK Inclusion: Women who delivered a first child by operative birth (i.e. forceps, vacuum assisted or emergency caesarean section) Exclusion: Women who were not able to speak and read English, was too ill on intensive care, had experienced a stillbirth, had a neonatal death or the baby was in critical condition
Interventions	Comparison: standard postnatal care versus face-to-face individual debriefing (Mitchell 1983) by community midwives. Multiple interventions at 10 days and 10 weeks postbirth. Counselling duration lasted up to 90 minutes
Outcomes	WDEQ(A); IES
Notes	Intervention was carried out in the women's homes by community midwives specifically trained in postpartum debriefing. Control group received standard postpartum care plus 'normal' debriefing - the doctor at delivery giving information and answering questions and the community midwife asking about the birth on her first visit

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	"Women were randomly allocated…using sealed envelopes containing the treatment group". However, the method used to generate a random (unpredictable) sequence was not clearly described
Allocation concealment (selection bias)	Unclear risk	Although sealed envelopes were used, it was not clear whether these were opaque without foreknowledge of treatment assignments
Blinding of participants and personnel (performance bias)	High risk	Due to the nature of the intervention, the blinding of recipients and providers of debriefing is not possible or difficult

Kershaw 2005 (*Continued*)

All outcomes		
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Postal questionnaire
Incomplete outcome data (attrition bias) All outcomes	High risk	No. of questionnaires returned/No. women randomised = 10 days postpartum Total: 75% (240/319); debriefing: 75% (120/161); control: 76% (120/158) 10 weeks postpartum Total: 62% (199/319); debriefing: 64% (103/161); control: 61% (96/158) 20 weeks postpartum Total: 61% (195/319); debriefing: 63% (102/161); control: 59% (93/158) Women who did not return questionnaires tended to be younger and those who had additional stress Insufficient information how missing data were dealt with
Selective reporting (reporting bias)	High risk	Non-significant results were mentioned, but data was not reported for some outcomes. Bias in a meta-analysis was likely to occur for primary outcomes
Other bias	Unclear risk	Possibility of imbalances in the intervention and control groups at baseline

Lavender 1998

Methods	RCT
Participants	A total of 120 postnatal women (selected) Setting: regional teaching hospital in North West England Inclusion: Primigravidas with singleton pregnancies and cephalic presentations who were in spontaneous labour at term and proceeded to normal vaginal delivery of a healthy baby Exclusion: Those with thirrd degree perineal tear, manual removal of the placenta, baby admitted to special care unit and women requiring high dependency care
Interventions	Comparison: standard care versus interactive individual interview when women were encouraged to spend as much time as necessary discussing their labour, asking questions and exploring their feelings with a research midwife (duration: from 30 to 120 minutes) . Single session
Outcomes	HADS
Notes	
Risk of bias	

Lavender 1998 (*Continued*)

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	"Randomisation was performed by simple random sampling using computer-generated numbers"
Allocation concealment (selection bias)	Low risk	Sealed opaque envelopes were used
Blinding of participants and personnel (performance bias) All outcomes	High risk	Due to the nature of the intervention, the blinding of recipients and providers of debriefing is not possible or difficult
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Postal questionnaire
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	No. of questionnaires returned/No. women randomised = total: 95% (114/120); debriefing: --% (58/unclear); control: --% (56/unclear) Insufficient information how missing data were dealt with
Selective reporting (reporting bias)	Low risk	None detected
Other bias	High risk	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 Outcomes were measured at 3 weeks postpartum

Priest 2003

Methods	RCT
Participants	A total of 1745 women who delivered healthy infants at term (universal) Setting: Two large maternity hospitals in Perth, Australia Inclusion: Women delivered healthy infant at or near term Exclusion: Insufficient English, already under psychological care, less than 18 years or with infant in neonatal care
Interventions	Comparison: Standardised individual debriefing (Mitchell 1983) by research midwives versus standard postnatal care. Face-to face. Single intervention: within 72 hours of delivery. Duration of intervention: 15 to 60 min
Outcomes	EPDS; IES; SADS; Clinician-administered PTSD scale
Notes	

Priest 2003 (*Continued*)

Risk of bias		
Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	"Randomisation was conducted within the strata of parity...and mode of delivery". Envelopes were used for the random allocation." Stratified randomisation
Allocation concealment (selection bias)	Unclear risk	"Each woman selected an envelope from a group of at least six sealed, opaque envelopes containing random allocations to either the intervention or control group."
Blinding of participants and personnel (performance bias) All outcomes	High risk	Due to the nature of the intervention, the blinding of recipients and providers of debriefing is not possible or difficult
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Postal questionnaire for all participants and interview with selected participants "All researchers except the research midwife were blinded to the women's groups allocation"
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	No. of questionnaires returned/No. women randomised = 2 months postpartum Total: 94% (1642/1745); debriefing: 92% (809/875); control: 96% (833/870) 6 months postpartum Total: 90% (1574/1745); debriefing: 89% (777/875); control: 92% (797/870) 12 months postpartum Total: 80% (1401/1745); debriefing: 80% (696/875); control: 81% (705/870) Insufficient information how missing data were dealt with
Selective reporting (reporting bias)	High risk	There were unreported outcome data
Other bias	Low risk	Substantial numbers (74%) of eligible women were not invited to participate because of time constraints. Of women who were invited to participate, 28% refused No significant baseline imbalances were detected between intervention and controlled groups after randomisation

Ryding 1998

Methods	Quasi-randomised
Participants	A total of 106 women with emergency caesarean section were allocated to either intervention or control groups (selected) Setting: One hospital in Sweden Inclusion: Swedish-speaking women giving birth to a live infant by emergency caesarean section
Interventions	Comparison: Individual consultations by an obstetrician with a primary psychotherapy qualification versus standard postnatal care after an emergency caesarean section. Multiple interventions: three to four consultations during the first 2 to 3 weeks after delivery. The first consultation took at least 1 h. The second to fourth meetings were limited to about 45 min
Outcomes	W-DEQ (A,B); IES; SCL
Notes	IES > 30

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	"Every second EmCS patient, according to the delivery ward register, was selected for counselling, the remainder being selected for the comparison group."
Allocation concealment (selection bias)	High risk	Due to a systematic method of the group allocation, foreknowledge of the forthcoming allocations was unavoidable
Blinding of participants and personnel (performance bias) All outcomes	High risk	Due to the nature of the intervention, the blinding of recipients and providers of debriefing is not possible or difficult
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	"The women in the counselling group might have been biased by gratitude to the research leader, who had also performed the counselling, therefore reporting that they were more healthy afterwards than they really were"
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	No. of questionnaires returned/No. women randomised = 1 month postpartum Total: 93% (99/106); debriefing: 94% (50/53); control: 92% (49/53) 6 months postpartum Total: 93% (99/106); debriefing: 94% (50/53); control: 92% (49/53) Insufficient information how missing data were dealt with
Selective reporting (reporting bias)	Low risk	None detected

Ryding 1998 (*Continued*)

Other bias	Unclear risk	Informed consent was obtained after randomisation

Ryding 2004

Methods	Quasi-randomised
Participants	A total of 162 women with emergency caesarean section (selected) Setting: One hospital Sweden Inclusion: Swedish-speaking women giving birth to a live infant by emergency caesarean section
Interventions	Comparison: Group counselling vs. standard care after an emergency caesarean section. Multiple interventions: 2 sessions (with a 2 to 3 week interval, lasted for 2 hours each) , conducted at about 2 months postpartum "to share their experiences of birth and the initial period with the baby". The group leaders were a psychologist and a midwife
Outcomes	W-DEQ (B); IES; EPDS
Notes	W-DEQ(B) > 60; IES > 30; EPDS > 12

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	"The women who gave birth on approximately 18 predetermined days of the month were randomised to the counselling group, and the remainder to the control group."
Allocation concealment (selection bias)	High risk	Due to a systematic method of the group allocation, foreknowledge of the forthcoming allocations was unavoidable
Blinding of participants and personnel (performance bias) All outcomes	High risk	Due to the nature of the intervention, the blinding of recipients and providers of debriefing is not possible or difficult
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Postal questionnaire
Incomplete outcome data (attrition bias) All outcomes	High risk	No. of questionnaires returned/No. women randomised = Total: 91% (147/162); debriefing: 92% (82/89); control: 89% (65/73) Among women in debriefing and returned questionnaire, 28% (23/82) did not attend the group counselling as intended. The reasons included feeling very well and having no need for the group meetings, feeling unwell and not being up to the group meetings Insufficient information how missing data were dealt with

Ryding 2004 (*Continued*)

Selective reporting (reporting bias)	Low risk	None detected
Other bias	Unclear risk	Of 217 women who met the inclusion criteria, 13 women never received information about the study and 42 (21%) of the remainder declined to participate

Small 2000

Methods	RCT
Participants	A total of 1041 women who had had operative deliveries (selected) Setting: large maternity hospital, Australia. Women were approached in the postnatal ward Inclusion: women who had given birth by caesarean section, forceps or vacuum extraction assisted Exclusion: women who had not had operative births, stillbirths or those who had babies weighing < 1500 g, those with insufficient English, those ill themselves, very ill babies and those whose private obstetrician refused access
Interventions	Standard care without debriefing versus standard care with a face to face individual debriefing (i.e. women were given an opportunity to discuss their labour, birth, post delivery events and experiences with a midwife). Single session before the women were discharged from hospital. Up to 60 min
Outcomes	EPDS; SF-36
Notes	EPDS ≥ 13

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	"We used telephone randomisation to allocate women to debriefing or standard care, with allocation determined by separate computer generated, adaptive biased coin randomisation schedules for each research midwife" who carried out recruitment and debriefing
Allocation concealment (selection bias)	Low risk	Telephone randomisation to allocate women
Blinding of participants and personnel (performance bias) All outcomes	High risk	Due to the nature of the intervention, the blinding of recipients and providers of debriefing is not possible or difficult
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Postal questionnaires

Small 2000 (*Continued*)

Incomplete outcome data (attrition bias) All outcomes	Unclear risk	6 months postpartum No. of questionnaires returned/No. women randomised = total: 88% (917/1041); debriefing: 90% (467/520); control: 86% (450/521) 4 to 6 years after childbirth No. of questionnaires returned/No. women randomised = total: 51% (534/1041); debriefing: 51% (264/520); control: 52% (270/521) Insufficient information how missing data were dealt with
Selective reporting (reporting bias)	Low risk	None detected
Other bias	Unclear risk	Substantial numbers (40%) of potential eligible women were not approached. Of women who met the inclusion criteria, 21% declined to participate No important baseline imbalances were detected between intervention and control groups after randomisation

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

Study	Reason for exclusion
Borghini 2014	The intervention was not debriefing, but "designed to improve parents' observation, attention and understanding of their preterm infant's characteristics and interactional competencies, as well as to promote parentalsensitivity and responsiveness towards the infant's needs"
Jotzo 2005	This study used an intervention for parents of premature infants during hospitalization in a level III NICU. Study population not relevant to aims of the current review
Meades 2011	Not RCT
Selkirk 2006	The intervention involved the comparison of low and high levels of medical interventions
Shaw 2013	Not RCT. The intervention includes 'infant redefinition' as part of the CBT (psychoeducation behavioural intervention sessions modelled to address parenting and maternal sensitivity, and targeted at enhancing maternal-infant interactions)
Tam 2003	This study used 'educational counselling' not debriefing

Characteristics of studies awaiting assessment *[ordered by study ID]*

Gamble 2010

Methods	Women experiencing a distressing birth were randomised to counselling or parenting support. The counselling or parenting support was delivered face to face within the first week after the birth and over the telephone at 4 to 6 weeks. Women in the study will also be offered a qualitative interview to explore their experiences upon completion of the study intervention, and this is a separate component of this particular trial
Participants	Pregnant women expecting a live baby and not in psychological or psychiatric treatment. Adequate language skills (English). Target sample size = 1200
Interventions	Midwife-led counselling for distressed mothers following childbirth compared to distress controls and non-distressed mothers - parenting support (active control) and usual care (matched control). Usual care involved completion of a questionnaire at the point that the midwife visited after the birth and at 6 weeks postpartum. All groups received the usual follow-up as provided by the hospital
Outcomes	Primary outcomes Trauma and depression will be measured through completion of questionnaires: Depression Anxiety Stress Scale (DASS-21) Edinburgh Postnatal Depression Scale (EPDS) Post-traumatic symptom scale (PSS) Secondary outcomes Anxiety and stress will be measured through completion of a questionnaire Depression Anxiety Stress Scale (DASS -21)
Notes	Lead researcher (Professor Jenny Gamble) was contacted in February 2014. However they discovered an error with data entry, and have not finalized the revised analysis

Taghizadeh 2008

Methods	RCT
Participants	Women who experienced a traumatic childbirth
Interventions	Counselling
Outcomes	PTSD symptoms measured by the IES
Notes	Article in Farsi or Persian. The paper was translated, but due to a lack of information on how the outcome of interest (PTSD) was defined, the review authors were unable to make an informed decision on whether the study should be included or excluded

DATA AND ANALYSES

Comparison 1. Psychological debriefing versus usual postnatal care

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Prevalence of PTSD symptoms (short-term: Up to 3 months postpartum)	3		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
1.1 Selected - high level of obstetric intervention	2	338	Risk Ratio (M-H, Random, 95% CI)	0.60 [0.34, 1.06]
1.2 Indicated	1	102	Risk Ratio (M-H, Random, 95% CI)	1.15 [0.66, 2.01]
2 Prevalence of PTSD symptoms (medium-term: 3-6 months postpartum)	3		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
2.1 Selected - high level of obstetric intervention	2	246	Risk Ratio (M-H, Random, 95% CI)	0.62 [0.27, 1.42]
2.2 Indicated	1	103	Risk Ratio (M-H, Random, 95% CI)	0.35 [0.10, 1.23]
3 Prevalence of PTSD symptoms (long-term: > 6 months postpartum)	1		Risk Ratio (M-H, Fixed, 95% CI)	Totals not selected
3.1 Universal	1		Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
4 Severity of PTSD symptoms (short-term: Up to 3 months postpartum)	1		Mean Difference (IV, Fixed, 95% CI)	Totals not selected
4.1 Indicated	1		Mean Difference (IV, Fixed, 95% CI)	0.0 [0.0, 0.0]
5 Severity of PTSD symptoms (medium-term: 3-6 months postpartum)	1		Mean Difference (IV, Fixed, 95% CI)	Totals not selected
5.1 Indicated	1		Mean Difference (IV, Fixed, 95% CI)	0.0 [0.0, 0.0]
6 Prevalence of depression/depressive symptoms (short-term: Up to 3 months postpartum)	2		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
6.1 Selected - low level of obstetric intervention	1	114	Risk Ratio (M-H, Fixed, 95% CI)	0.16 [0.07, 0.37]
6.2 Indicated	1	102	Risk Ratio (M-H, Fixed, 95% CI)	0.96 [0.55, 1.67]
7 Prevalence of depression/depressive symptoms (medium-term: 3-6 months postpartum)	3		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
7.1 Selected - high level of obstetric intervention	2	1064	Risk Ratio (M-H, Random, 95% CI)	1.00 [0.56, 1.79]
7.2 Indicated	1	103	Risk Ratio (M-H, Random, 95% CI)	0.25 [0.09, 0.69]
8 Prevalence of depression/depressive symptoms (long-term: > 6 months postpartum)	2		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
8.1 Universal	1	1401	Risk Ratio (M-H, Fixed, 95% CI)	1.00 [0.82, 1.22]

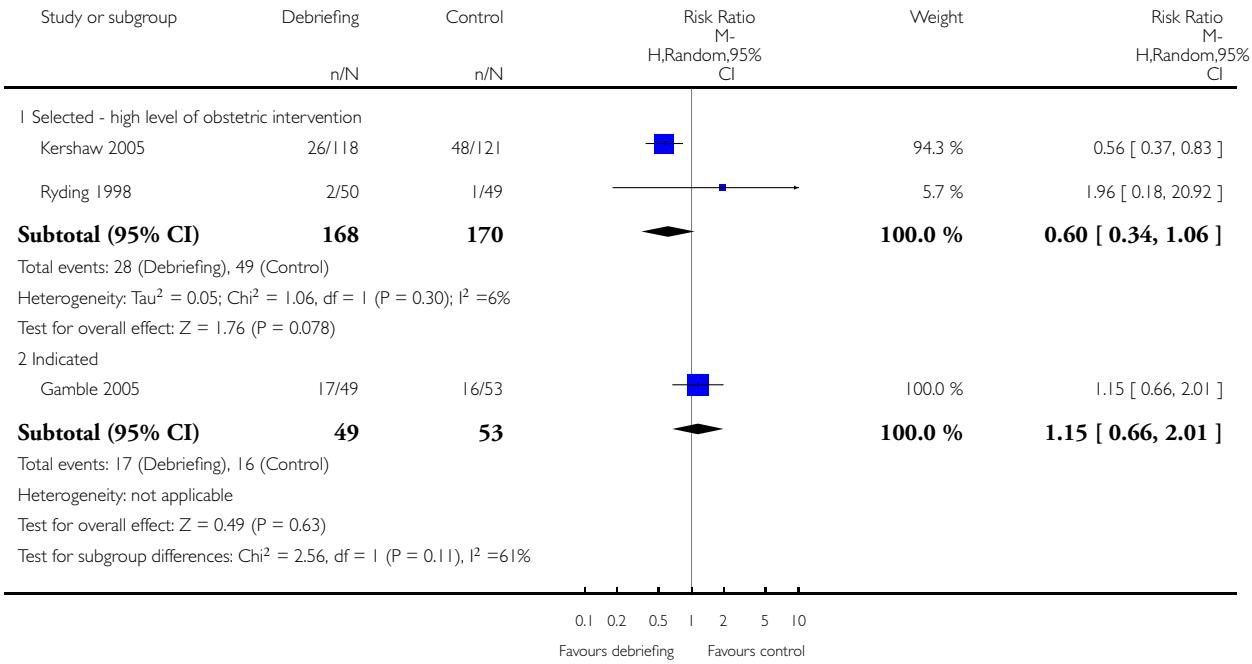
8.2 Selected - high level of obstetric intervention	1	534	Risk Ratio (M-H, Fixed, 95% CI)	0.95 [0.65, 1.40]
9 Severity of depressive symptoms (medium-term: 3-6 months postpartum)	1		Mean Difference (IV, Fixed, 95% CI)	Totals not selected
9.1 Selected - high level of obstetric intervention	1		Mean Difference (IV, Fixed, 95% CI)	0.0 [0.0, 0.0]
10 Severity of depressive symptoms (long-term: > 6 months postpartum)	1		Mean Difference (IV, Fixed, 95% CI)	Totals not selected
10.1 Selected - high level of obstetric intervention	1		Mean Difference (IV, Fixed, 95% CI)	0.0 [0.0, 0.0]
11 Prevalence of anxiety (short-term: Up to 3 months postpartum)	1		Risk Ratio (M-H, Fixed, 95% CI)	Totals not selected
11.1 Selected - low level of obstetric intervention	1		Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
12 Prevalence of anxiety (medium-term: 3-6 months postpartum)	1		Risk Ratio (M-H, Fixed, 95% CI)	Totals not selected
12.1 Indicated	1		Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
13 Fear of childbirth (medium-term: 3-6 months postpartum)	1		Risk Ratio (M-H, Fixed, 95% CI)	Totals not selected
13.1 Selected - high level of obstetric intervention	1		Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
14 Severityof general psychological morbidity (medium-term: 3-6 months postpartum)	1		Mean Difference (IV, Fixed, 95% CI)	Totals not selected
14.1 Selected - high level of obstetric intervention	1		Mean Difference (IV, Fixed, 95% CI)	0.0 [0.0, 0.0]
15 Severity of general psychological morbidity (long-term: > 6 months postpartum)	1		Mean Difference (IV, Fixed, 95% CI)	Totals not selected
15.1 Selected - high level of obstetric intervention	1		Mean Difference (IV, Fixed, 95% CI)	0.0 [0.0, 0.0]

Analysis 1.1. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 1 Prevalence of PTSD symptoms (short-term: Up to 3 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 1 Prevalence of PTSD symptoms (short-term: Up to 3 months postpartum)

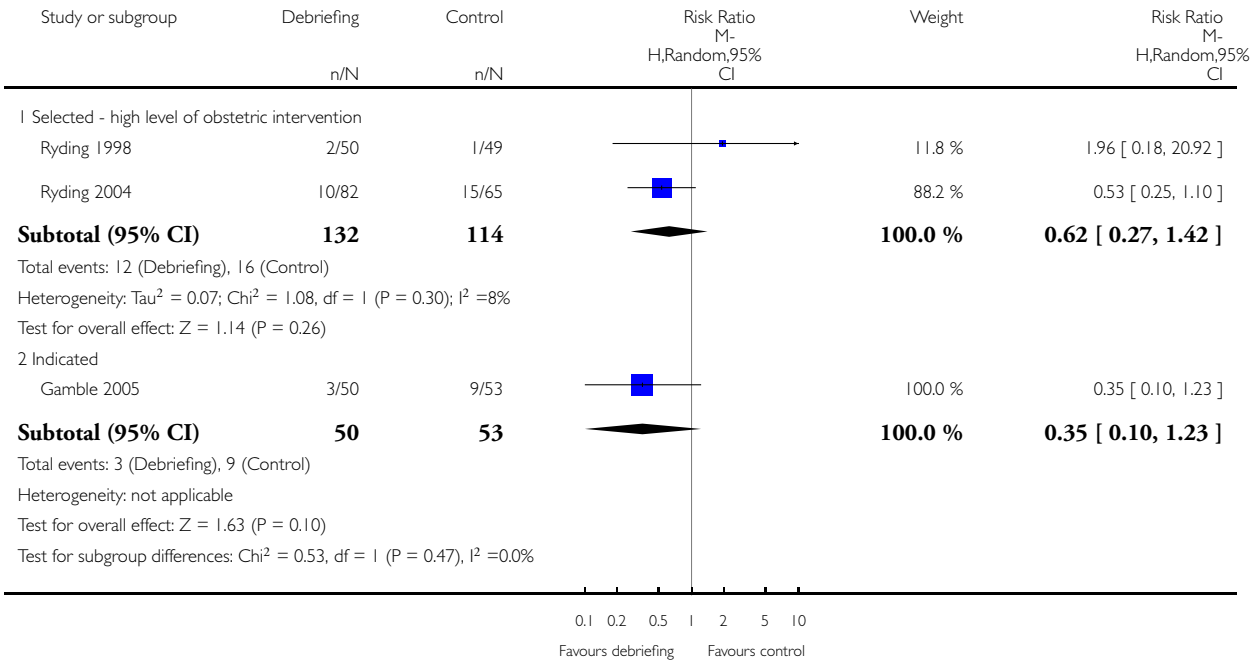


Analysis 1.2. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 2 Prevalence of PTSD symptoms (medium-term: 3-6 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 2 Prevalence of PTSD symptoms (medium-term: 3-6 months postpartum)



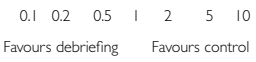
Analysis 1.3. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 3 Prevalence of PTSD symptoms (long-term: > 6 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 3 Prevalence of PTSD symptoms (long-term: > 6 months postpartum)

Study or subgroup	Debriefing n/N	Control n/N	Risk Ratio M-H,Fixed,95% CI	Risk Ratio M-H,Fixed,95% CI
1 Universal				
Priest 2003	184/875	172/870		1.06 [0.88, 1.28]



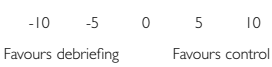
Analysis 1.4. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 4 Severity of PTSD symptoms (short-term: Up to 3 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 4 Severity of PTSD symptoms (short-term: Up to 3 months postpartum)

Study or subgroup	Debriefing N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Mean Difference IV,Fixed,95% CI
1 Indicated						
Gamble 2005	49	4.81 (3.65)	53	5.45 (3.01)		-0.64 [-1.94, 0.66]



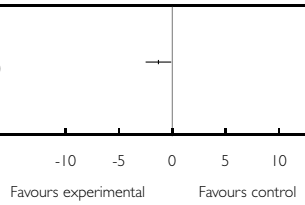
Analysis 1.5. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 5 Severity of PTSD symptoms (medium-term: 3-6 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 5 Severity of PTSD symptoms (medium-term: 3-6 months postpartum)

Study or subgroup	Debriefing N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Mean Difference IV,Fixed,95% CI
1 Indicated						
Gamble 2005	50	2.54 (2.44)	53	3.83 (3.59)		-1.29 [-2.47, -0.11]



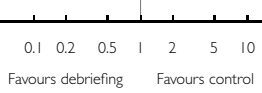
Analysis 1.6. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 6 Prevalence of depression/depressive symptoms (short-term: Up to 3 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 6 Prevalence of depression/depressive symptoms (short-term: Up to 3 months postpartum)

Study or subgroup	Debriefing n/N	Control n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
1 Selected - low level of obstetric intervention					
Lavender 1998	5/58	31/56		100.0 %	0.16 [0.07, 0.37]
Subtotal (95% CI)	**58**	**56**		**100.0 %**	**0.16 [0.07, 0.37]**
Total events: 5 (Debriefing), 31 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 4.19 (P = 0.000028)					
2 Indicated					
Gamble 2005	16/49	18/53		100.0 %	0.96 [0.55, 1.67]
Subtotal (95% CI)	**49**	**53**		**100.0 %**	**0.96 [0.55, 1.67]**
Total events: 16 (Debriefing), 18 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.14 (P = 0.89)					
Test for subgroup differences: Chi^2 = 12.01, df = 1 (P = 0.00), I^2 =92%					

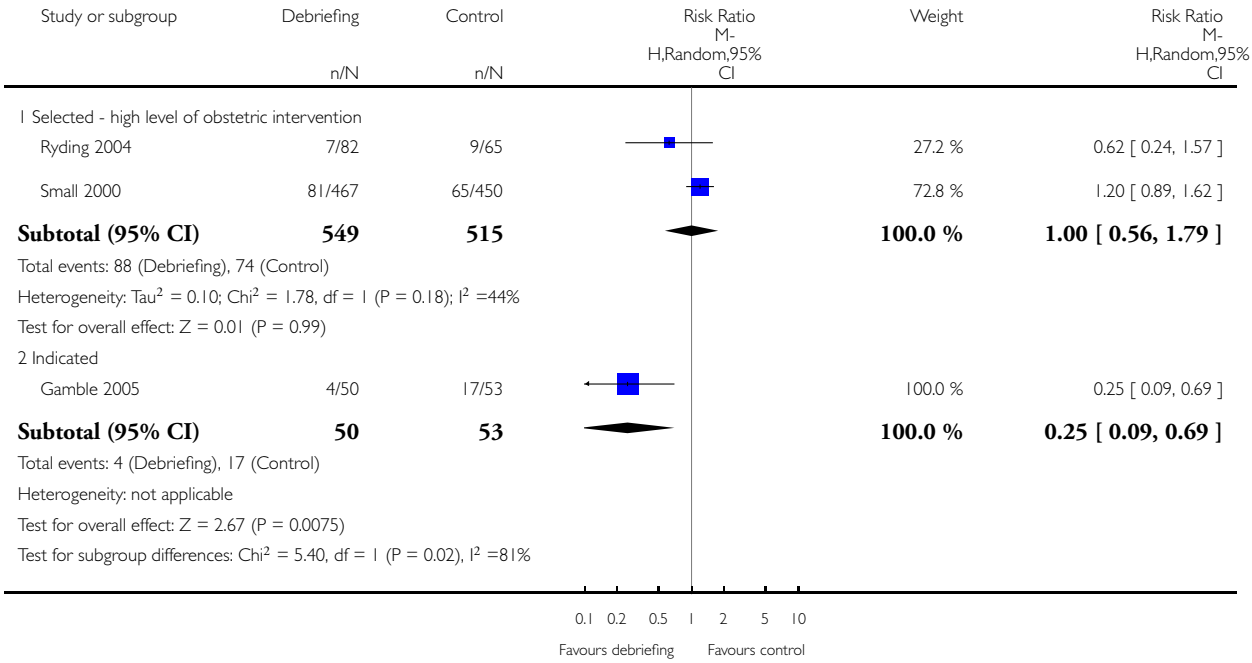


Analysis 1.7. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 7 Prevalence of depression/depressive symptoms (medium-term: 3-6 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 7 Prevalence of depression/depressive symptoms (medium-term: 3-6 months postpartum)



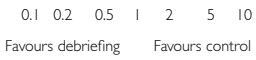
Analysis 1.8. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 8 Prevalence of depression/depressive symptoms (long-term: > 6 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 8 Prevalence of depression/depressive symptoms (long-term: > 6 months postpartum)

Study or subgroup	Debriefing n/N	Control n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
1 Universal					
Priest 2003	156/696	158/705		100.0 %	1.00 [0.82, 1.22]
Subtotal (95% CI)	**696**	**705**		**100.0 %**	**1.00 [0.82, 1.22]**
Total events: 156 (Debriefing), 158 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.00 (P = 1.0)					
2 Selected - high level of obstetric intervention					
Small 2000	42/264	45/270		100.0 %	0.95 [0.65, 1.40]
Subtotal (95% CI)	**264**	**270**		**100.0 %**	**0.95 [0.65, 1.40]**
Total events: 42 (Debriefing), 45 (Control)					
Heterogeneity: not applicable					
Test for overall effect: Z = 0.24 (P = 0.81)					
Test for subgroup differences: Chi^2 = 0.04, df = 1 (P = 0.83), I^2 =0.0%					



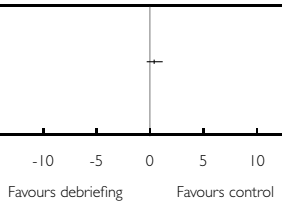
Analysis 1.9. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 9 Severity of depressive symptoms (medium-term: 3-6 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 9 Severity of depressive symptoms (medium-term: 3-6 months postpartum)

Study or subgroup	Debriefing N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Mean Difference IV,Fixed,95% CI
1 Selected - high level of obstetric intervention						
Small 2000	467	7.16 (5.68)	450	6.72 (5.5)		0.44 [-0.28, 1.16]



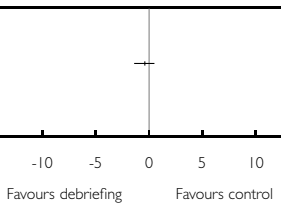
Analysis 1.10. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 10 Severity of depressive symptoms (long-term: > 6 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 10 Severity of depressive symptoms (long-term: > 6 months postpartum)

Study or subgroup	Debriefing N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Mean Difference IV,Fixed,95% CI
1 Selected - high level of obstetric intervention						
Small 2000	264	6.33 (5.16)	270	6.77 (5.69)		-0.44 [-1.36, 0.48]



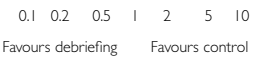
Analysis 1.11. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 11 Prevalence of anxiety (short-term: Up to 3 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 11 Prevalence of anxiety (short-term: Up to 3 months postpartum)

Study or subgroup	Debriefing n/N	Control n/N	Risk Ratio M-H,Fixed,95% CI	Risk Ratio M-H,Fixed,95% CI
1 Selected - low level of obstetric intervention				
Lavender 1998	4/58	28/56		0.14 [0.05, 0.37]



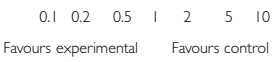
Analysis 1.12. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 12 Prevalence of anxiety (medium-term: 3-6 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 12 Prevalence of anxiety (medium-term: 3-6 months postpartum)

Study or subgroup	Experimental n/N	Control n/N	Risk Ratio M-H,Fixed,95% CI	Risk Ratio M-H,Fixed,95% CI
1 Indicated				
Gamble 2005	1/50	6/53		0.18 [0.02, 1.42]

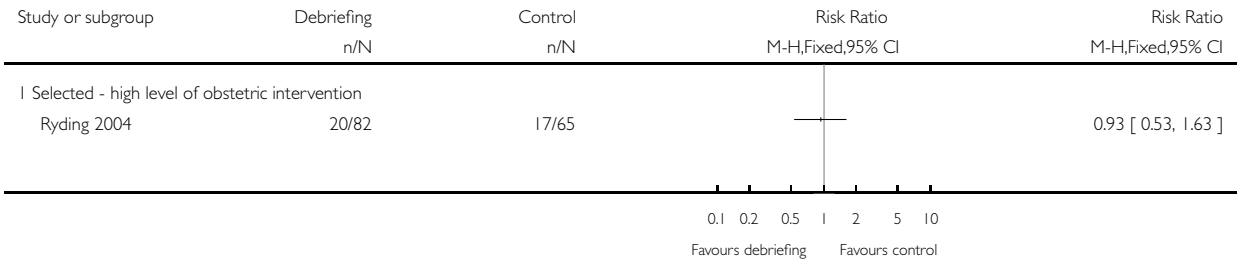


Analysis 1.13. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 13 Fear of childbirth (medium-term: 3-6 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 13 Fear of childbirth (medium-term: 3-6 months postpartum)

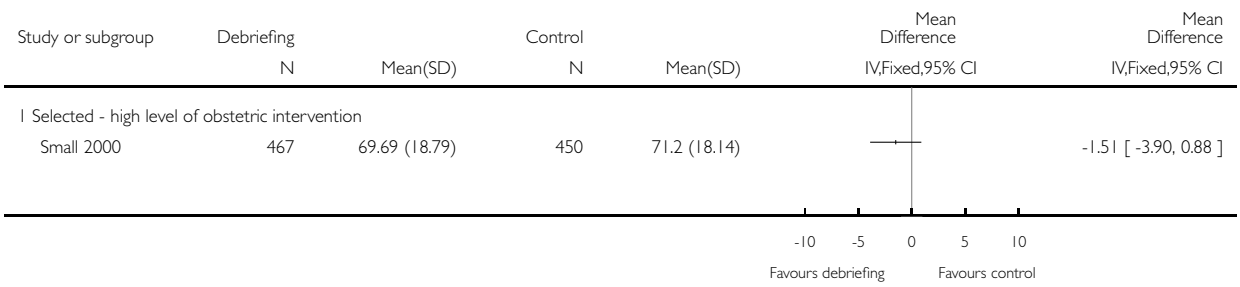


Analysis 1.14. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 14 Severityof general psychological morbidity (medium-term: 3-6 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 14 Severityof general psychological morbidity (medium-term: 3-6 months postpartum)

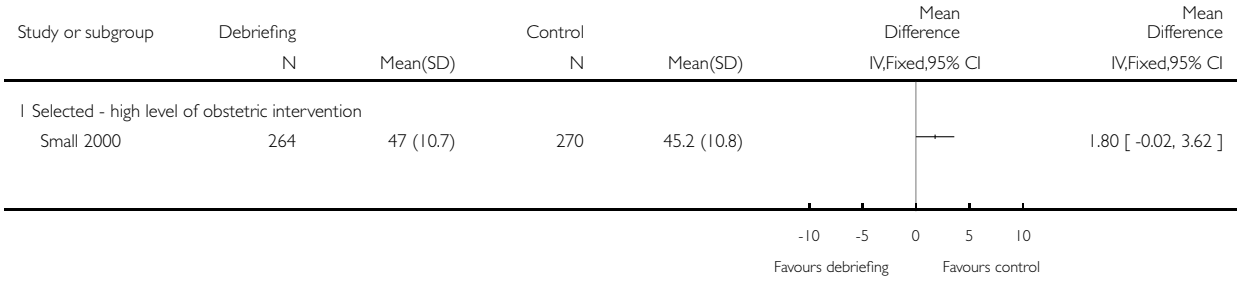


Analysis 1.15. Comparison 1 Psychological debriefing versus usual postnatal care, Outcome 15 Severity of general psychological morbidity (long-term: > 6 months postpartum).

Review: Debriefing interventions for the prevention of psychological trauma in women following childbirth

Comparison: 1 Psychological debriefing versus usual postnatal care

Outcome: 15 Severity of general psychological morbidity (long-term: > 6 months postpartum)



APPENDICES

Appendix 1. Additional database searches

1 CENTRAL search strategy

#1 MeSH descriptor POSTPARTUM PERIOD, this term only
#2 MeSH descriptor POSTNATAL CARE, this term only
#3 ((post partum) or postpartum)
#4 ((post natal) or postnatal)
#5 ((peri natal) or perinatal)
#6 puerper*
#7 MeSH descriptor PARTURITION explode all trees
#8 ((child NEXT birth) or childbirth)
#9 birth
#10 MeSH descriptor PREMATURE BIRTH, this term only
#11 MeSH descriptor CESAREAN SECTION, this term only
#12 (caesarean or cesarean)
#13 (#1 OR #2 OR #3 OR #4 OR #5 OR #6 OR #7 OR #8 OR #9 OR #10 OR #11 OR #12)
#14 debrief*
#15 MeSH descriptor CRISIS INTERVENTION, this term only
#16 crisis intervention*
#17 MeSH descriptor COUNSELING, this term only
#18 counsel*
#19 MeSH descriptor STRESS DISORDERS, TRAUMATIC explode all trees with qualifier: PC
#20 (#14 OR #15 OR #16 OR #17 OR #18 OR #19)

#21 MeSH descriptor STRESS DISORDERS, TRAUMATIC explode all trees
#22 trauma*
#23 posttrauma* or (post trauma*)
#24 stress*
#25 (psycholog* or mental or maternal) and (distress* or disorder* or health* or morbid*)
#26 (#21 OR #22 OR #23 OR #24 OR #25)
#27 (#13 AND #20 AND #26)

2 MEDLINE search strategy (OvidSP)

1. Postnatal Care/
2. (postnatal$ or puerperium).ti,ab.
3. pregnancy/ or exp labor, obstetric/ or exp parturition/
4. (childbirth or labo?r or parturi$).ti,ab.
5. ((trauma$ or cris?s) adj3 (birth$ or labo?r$ or deliver$)).ti,ab.
6. Perinatal Care/
7. Postpartum Period/
8. (peri?natal$ or post?partum or post?natal$).ti,ab.
9. Labor pain/
10. ((obstetric or labo?r) adj pain$).ti,ab.
11. Obstetric labor complications/
12. or/1-11
13. debrief$.ti,ab.
14. Crisis Intervention/
15. counseling/ or directive counseling/
16. counsel$.ti.
17. behavior therapy/ or cognitive therapy/
18. ((cognitive or behavio?r$) adj therap$).ti,ab.
19. or/13-18
20. PTSD.ti,ab.
21. Stress Disorders, Post-Traumatic/
22. post?traumatic$.ti,ab.
23. puerperal disorders/
24. depression, postpartum/
25. or/20-24
26. and/12,19,25

3 EMBASE search strategy (OvidSP)

1. exp obstetric care/
2. (postnatal$ or puerperium or postpartum).ti,ab.
3. pregnancy/
4. exp childbirth/
5. (childbirth or labo?r or parturi$).ti,ab.
6. ((trauma$ or cris?s) adj3 (birth$ or labo?r$ or deliver$)).ti,ab.
7. puerperium/
8. (peri?natal$ or post?partum or post?natal$).ti,ab.
9. labor pain/
10. ((obstetric or labo?r) adj pain$).ti,ab.
11. exp labor complication/
12. or/1-11
13. debrief$.ti,ab.
14. crisis intervention/

15. COUNSELING/ or DIRECTIVE COUNSELING/
16. counsel$.ti.
17. behavior therapy/
18. cognitive therapy/
19. ((cognitive or behavio?r$) adj therap$).ti,ab.
20. or/13-19
21. PTSD.ti,ab.
22. posttraumatic stress disorder/
23. post?traumatic$.ti,ab.
24. puerperal disorder/
25. puerperal depression/
26. or/21-25
27. and/12,20,26

4 PsycINFO search strategy (OvidSP)

1. postnatal period/ or perinatal period/ or pregnancy/
2. (postnatal$ or puerperium or postpartum).ti,ab.
3. "labor (childbirth)"/ or birth/ or exp obstetrical complications/
4. (childbirth or labo?r or parturi$).ti,ab.
5. ((trauma$ or cris?s) adj3 (birth$ or labo?r$ or deliver$)).ti,ab.
6. postnatal period/
7. (peri?natal$ or post?partum or post?natal$).ti,ab.
8. ((obstetric or labo?r) adj pain$).ti,ab.
9. or/1-8
10. "debriefing (psychological)"/
11. debrief$.ti,ab.
12. crisis intervention/
13. (cris?s adj intervention$).ti,ab.
14. counseling/
15. counsel$.ti,ab.
16. cognitive behavior therapy/ or behavior therapy/ or cognitive therapy/
17. ((cognitive or behavio?r$) adj therap$).ti,ab.
18. or/10-17
19. PTSD.ti,ab.
20. posttraumatic stress disorder/
21. post?traumatic$.ti,ab.
22. postpartum depression/
23. (puerperal adj (illness$ or disorder$ or psychos?s)).ti,ab.
24. or/19-23
25. and/9,18,24

5 Maternity and Infant Care search strategy (OvidSP)

1. Postnatal care.de.
2. Puerperium.de. or postnatal$.mp.
3. obstetric.mp. or Labour.de. or Pregnancy.de.
4. parturition.mp.
5. childbirth.mp.
6. labo?r.mp. [mp=abstract, heading word, title]
7. parturi$.mp. [mp=abstract, heading word, title]
8. ((trauma$ or cris?s) adj3 (birth$ or labo?r$ or deliver$)).mp. [mp=abstract, heading word, title]
9. Perinatal care.de.

10. Postnatal period.de. or Postpartum Period.mp.
11. (peri?natal$ or post?partum or post?natal$).mp. [mp=abstract, heading word, title]
12. Labor pain.mp.
13. ((obstetric or labo?r) adj pain$).mp. [mp=abstract, heading word, title]
14. Labour complications.de. or labo?r complications.mp.
15. 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 10 or 11 or 12 or 13 or 14
16. Debriefing.de. or debrief$.mp.
17. Crisis Intervention.mp.
18. directive counseling.mp. or Counselling.de.
19. counsel$.mp. [mp=abstract, heading word, title]
20. Cognitive therapy.de.
21. behavio?r therapy.mp. [mp=abstract, heading word, title]
22. ((cognitive or behavio?r$) adj therap$).mp. [mp=abstract, heading word, title]
23. 16 or 17 or 18 or 19 or 20 or 21 or 22
24. PTSD.mp. or Stress disorders - post-traumatic.de.
25. post?traumatic$.mp. [mp=abstract, heading word, title]
26. Puerperal disorders.de.
27. (Postnatal depression or Depression).de.
28. 24 or 25 or 26 or 27
29. 15 and 23 and 28

6 CINAHL search strategy (EBSCOhost)

S27 S13 and S20 and S26
S26 S21 or S22 or S23 or S24 or S25
S25 (MH "Depression, Postpartum") OR (MH "Edinburgh Postnatal Depression Scale")
S24 (MH "Puerperal Disorders")
S23 TI (posttraumatic* or post-traumatic*) AND AB (posttraumatic* or post-traumatic*)
S22 TI PTSD OR AB PTSD
S21 (MH "Stress Disorders, Post-Traumatic")
S20 S14 or S15 or S16 or S17 or S18 or S19
S19 TI ((cognitive or behaviour* or behavior*) N2 therap*) OR AB ((cognitive or behaviour* or behavior*) N2 therap*)
S18 (MH "Behavior Therapy") OR (MH "Cognitive Therapy")
S17 TI counsel* OR AB counsel*
S16 (MH "Counseling")
S15 (MH "Crisis Intervention")
S14 TI debrief* OR AB debrief*
S13 S1 or S2 or S3 or S4 or S5 or S6 or S7 or S8 or S9 or S10 or S11 or S12
S12 (MH "Obstetrics") OR (MH "Pregnancy Outcomes") OR (MH "Obstetric Emergencies")
S11 TI ((obstetric or labor or labour) N3 pain*) OR AB ((obstetric or labor or labour) N3 pain*)
S10 (MH "Labor Pain")
S9 TI (perinatal* or peri-natal* or postpartum or post-partum or postnatal or post-natal*) OR AB (perinatal* or peri-natal* or postpartum or post-partum or postnatal or post-natal*)
S8 (MH "Postnatal Period")
S7 (MH "Perinatal Care")
S6 TI ((trauma* or crisis* or crises*) N3 (birth* or labour* or labor* or deliver*)) OR AB ((trauma* or crisis* or crises*) N3 (birth* or labour* or labor* or deliver*))
S5 TI (childbirth or labour or labor or parturi*) OR AB (childbirth or labour or labor or parturi*)
S4 (MH "Labor") OR (MH "Labor Complications")
S3 TI (pregnancy or parturition) OR AB (pregnancy or parturition)d
S2 TI (postnatal* or puerperium) OR AB (postnatal* or puerperium)
S1 (MH "Postnatal Care")

7 Web of Science Conference Proceedings Citation Index (all years)

Topic=((debriefing or "crisis intervention" or counsel*)) AND Topic=((postpartum or "post partum" or post-partum or postnatal or "post natal" or perinatal or "peri natal" or puerper* or parturition or birth or childbirth or caesarean or cesarean or labour or labor))

8 Open Grey (all years)

childbirth discipline:(05Q - Psychology)

WHAT'S NEW

Last assessed as up-to-date: 4 March 2015.

Date	Event	Description
13 April 2015	Amended	Contact details updated.

CONTRIBUTIONS OF AUTHORS

This review was originally designed by MH Bastos and further developed in collaboration with D Bick, M Furuta, R Small and K McKenzie-McHarg.

DECLARATIONS OF INTEREST

None known

SOURCES OF SUPPORT

Internal sources

- No sources of support provided, Other.

External sources

- No sources of support supplied

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

We adopted updated methods for the assessment of the risk of bias in individual trials and the overall quality of evidence for each outcome following the recommendation of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2008).

In the protocol, we described the classification of participants under the section of 'Participants' (under 'Criteria for considering studies for this review' in 'Methods'). However, we moved these descriptions from 'Participants' to 'intervention' to make clear that the classification was planned to be used for subgroup analysis rather than for criteria for selection of study participants for this review.

We stated in the protocol that "the main outcome measure…is psychological trauma in women following childbirth, as variously defined and measured by study trialists (e.g. Rates of PTSD and traumatic stress symptoms)" (p.2). In the included studies, the prevalence and severity of symptoms were measures used to assess the main outcome. We originally did not distinguish between 'main' and 'primary' outcomes. However, following the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2008), we further divided the main outcomes into primary and secondary outcomes. We presented the prevalence of PTSD symptoms as a primary outcome (conclusions about the effects of debriefing in this review were based largely on this outcome) and severity of PTSD symptoms as a secondary outcome. We also presented both the proportion and severity of other secondary outcomes, where data were available.

Regarding data synthesis, we stated in the protocol that "Where substantial heterogeneity was found (I^2 > 50%), a random-effects model was used. Where there was no significant degree of heterogeneity, we used a fixed-effect model for meta-analyses". However, the latest *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2008) suggests that the decision to use the fixed-effect model versus random-effects model should be informed by: (i) assumptions about whether heterogeneity in the treatment effect is likely to exist, and (ii) the clinical and methodological heterogeneity detected, not statistical heterogeneity, as measures of statistical heterogeneity are often poorly estimated when only a few studies are included in a meta-analysis. We therefore used random-effects model meta-analyses, which is a conservative option and more appropriate for this study than a fixed-effect model (which assumes that there is one true effect) because the population and setting of trials were slightly different, therefore the effects were likely to differ slightly.

The co-author Cathy Rowan was unable to contribute to this review, however she participated in the development of the protocol.

INDEX TERMS

Medical Subject Headings (MeSH)

Delivery, Obstetric [*psychology]; Depression [prevention & control]; Narrative Therapy [*methods]; Parturition [*psychology]; Postpartum Period [*psychology]; Randomized Controlled Trials as Topic; Stress Disorders, Post-Traumatic [*prevention & control]; Stress, Psychological [*prevention & control]

MeSH check words

Adult; Female; Humans; Pregnancy