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Primary closure versus delayed closure for non bite traumatic wounds within 24 hours post injury (Review)

Eliya-Masamba MC, Banda GW

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[Intervention Review]

Primary closure versus delayed closure for non bite traumatic wounds within 24 hours post injury

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ABSTRACT

Background

Acute traumatic wounds are one of the common reasons why people present to the emergency department. Primary closure has traditionally been reserved for traumatic wounds presenting within six hours of injury and considered 'clean' by the attending surgeon, with the rest undergoing delayed primary closure as a means of controlling wound infection. Primary closure has the potential benefit of rapid wound healing but poses the potential threat of increased wound infection. There is currently no evidence to guide clinical decision-making on the best timing for closure of traumatic wounds.

Objectives

To determine the effect on time to healing of primary closure versus delayed closure for non bite traumatic wounds presenting within 24 hours post injury. To explore the adverse effects of primary closure compared with delayed closure for non bite traumatic wounds presenting within 24 hours post injury.

Search methods

In May 2013, for this first update we searched the Cochrane Wounds Group Specialised Register; the Cochrane Central Register of Controlled Trials (CENTRAL) (*The Cochrane Library*); Ovid MEDLINE; Ovid MEDLINE (In-Process & Other Non-Indexed Citations); Ovid EMBASE; and EBSCO CINAHL. There were no restrictions with respect to language, date of publication or study setting.

Selection criteria

Randomised controlled trials comparing primary closure with delayed closure of non bite traumatic wounds.

Data collection and analysis

Two review authors independently evaluated the results of the searches against the inclusion criteria. No studies met the inclusion criteria for this review.

Main results

Since no studies met the inclusion criteria, neither a meta-analysis nor a narrative description of studies was possible.

Authors' conclusions

There is currently no systematic evidence to guide clinical decision-making regarding the timing for closure of traumatic wounds. There is a need for robust research to investigate the effect of primary closure compared with delayed closure for non bite traumatic wounds presenting within 24 hours of injury.

PLAIN LANGUAGE SUMMARY

Immediate closure or delayed closure for treating traumatic wounds in the first 24 hours following injury

Acute traumatic wounds, for example tears, cuts, and scrapes, are a common reason why people go to the emergency department. Primary closure (which is bringing the edges of the wound together with stitches, adhesive tape, staples or glue) is usually used on wounds which are treated quickly (within 6 hours of injury) and which are clean of debris. Wounds can be contaminated by dirt and debris and in these cases may not be closed until later, meaning a delayed closure. If this happens, the wound is cleaned, left for two to three days, checked to see if it is still clean and then closed. This is thought to reduce the chances of becoming infected. Primary closure has the potential benefit of rapid wound healing but may lead to an increased chance of infection.

We wanted to determine the effects on healing, and any adverse effects, of immediate closure compared with delayed closure. We searched the medical literature for randomised controlled trials but found no studies which answered the question. There is currently no evidence to suggest the best timing for closing acute traumatic wounds to promote the best healing.

BACKGROUND

Description of the condition

Acute traumatic wounds are said to be one of the most common reasons for people to present to the emergency department (Hollander 1999; Silbert 1981). More than 11 million patients with traumatic wounds are seen annually in emergency departments throughout the USA (Hollander 1995). Traumatic wounds (wounds resulting from injury), range from minor lacerations to those with extensive tissue damage. The extent of tissue damage is dependent upon the nature of the object causing the injury as well as the mechanism of injury. The Centers for Disease Control and Prevention has classified wounds of any origin into four categories: clean, clean/contaminated, contaminated and dirty/infected (Garner 1985). Traumatic wounds fall into the last two categories. Open, fresh traumatic wounds are categorised as contaminated, while old traumatic wounds that have retained devitalised (dead) tissue and those that involve existing clinical infection or perforated viscera (internal organs) are categorised as dirty/infected. In contrast, those categorised as clean are uninfected operative wounds in which no inflammation is encountered and the respiratory, alimentary, genital or urinary tract are not entered (Garner 1985). What this classification does not say, however, is what time span constitutes 'fresh' or 'old' traumatic wounds. Traumatic wounds do not always occur in isolation but can be associated with organ damage other than soft tissues (skin, subcutaneous tissue, muscle and their associated neurovascular supply). The management of traumatic wounds involving other structures (for instance bone, viscera) is governed by the nature of the organ involved and not merely by the timing of the injury in relation to presentation (Hohmann 2007; Rippon 1999; Robert 2006; Scherping 2007). Whilst traumatic wounds can result from many types of injury (for example, crush injury, blast injury, animal bites) we will not consider studies of traumatic wounds caused by animal bites as this is the subject of another Cochrane Review (in preparation).

Description of the intervention

In the management of traumatic wounds, basic principles of wound care need to be followed to obtain stable, long-term coverage, ultimately restoring form and function (Lee 2009). There has not been a significant change in these principles over the years (Hollander 1999). Wound care generally involves cleaning the wound and then re-approximating the wound edges until natural healing occurs (Hollander 1999). Judgement and surgical skill are necessary in deciding whether a wound is best allowed to heal by first intention (where the wound edges are approximated), secondary intention (where the wound is left open initially and closure is effected with granulation tissue which forms from the base and both sides of the wound towards the surface of the wound), or third intention (where the wound is initially left open and observed until there is no clinical evidence of inflammation or contamination then the wound edges are approximated). Healing by third intention is also referred to as delayed primary closure (Dorland 2003; Kumar 2005; Lorenz 2008). For traumatic wounds healing by first intention, primary closure has traditionally been reserved for traumatic wounds presenting within six hours of injury and considered 'clean' by the attending surgeon, with the rest undergoing delayed primary closure as a means of controlling wound infection. The wound undergoing delayed closure is first debrided (foreign material and devitalised or contaminated tissue is removed), then dressed and inspected on a daily basis for 48 to 72 hours after which it is closed, provided there is no further devitalised tissues or inflammation (McLatchie 2002).

How the intervention might work

The wound repair or healing process can be thought of as having overlapping phases that include inflammation, proliferation and remodelling. The wound is re-epithelialised during the proliferation phase before the scar matures in the remodelling phase (Lorenz 2008). Considering that remodelling can continue for two years, completion of re-epithelialisation has been one of the factors examined to assess the progress of wound healing in the short term. This progress has most recently been accurately measured by high resolution ultrasound scanning in B-mode (Dyson 2003; Lorenz 2008; Rippon 1999). Theoretically, epithelialisation of the wound when the edges are approximated occurs within 24 to 48 hours (Lorenz 2008). It would therefore be expected that the wound that is closed primarily would epithelialise and progress through the remaining processes of wound healing earlier than the wound whose closure is delayed by 48 hours or more.

Why it is important to do this review

In the clinical setting there are several factors that may not only slow down or stop the healing process, but may also result in unsightly and dysfunctional scars, and very rarely mortality. These include infection, poor nutrition, deficient oxygen and blood supply, chemotherapy, radiation and some disease states such as diabetes mellitus (Lorenz 2008). Infection is of particular importance in traumatic wounds. Traumatic wounds are classified by the Centers for Disease Control and Prevention as either contaminated or dirty/infected (Garner 1985), with a predicted relative probability that a wound will become infected of 10% to 17%, and over 27% respectively (Garner 1985). This suggests a potential harm if undertaking primary closure of traumatic wounds after surgical debridement, even in the absence of clinical infection (Garner 1985). The objective assessment of clinical infection is still a challenge, however, given the unavailability of a validated assessment tool (Garner 1985; Hollander 1995)

On the other hand, a threshold number of bacteria in the wound appears to be necessary to overcome host resistance and cause clinical infection. Bacterial contamination of wounds results in clinical infection and delayed wound healing if more than 10^5 organisms per gram of tissue are present in the wound (Lorenz 2008). Experience in microsurgical reconstruction suggests that microbial contamination of wounds could be held within tolerable limits for longer periods (up to 72 hours) through a correctly performed surgical debridement (Godina 1986). In addition, several reports (Faisham 2001; Harley 2002; Weitz-Marshall 2002) suggest infections of traumatic wounds are not caused by the initial contamination, but instead the organisms are acquired secondarily from the hospital.

Given that surgical debridement has the potential to reduce wound infection, primary closure following debridement of traumatic wounds without clinical infection therefore has the potential benefit of rapid wound healing. Currently, however, there is no systematic evidence to guide clinical decision-making on the best timing for closure of traumatic wounds.

OBJECTIVES

To determine the effect of primary closure compared with delayed closure for non bite traumatic wounds presenting within 24 hours post injury on time to healing.
To explore the adverse effects of primary closure compared with delayed closure for non bite traumatic wounds presenting within 24 hours post injury.

METHODS

Criteria for considering studies for this review

Types of studies

Randomised controlled trials (RCTs) comparing primary closure with delayed closure of non bite traumatic wounds. We excluded cluster-randomised trials and studies with cross-over designs because these are not appropriate for answering this particular question.

Types of participants

Inclusion criteria

People of any age presenting with non bite traumatic wounds in any setting within 24 hours of injury.

Exclusion criteria

We excluded wounds with clinical infection at presentation and wounds involving more than just the soft tissues (skin, subcutaneous tissue, muscle and their neurovascular bundles). We excluded studies of traumatic wounds caused by animal bites as this is the subject of another Cochrane Review (in preparation).

Types of interventions

Primary closure compared with delayed closure.

Primary closure is defined as the approximation of the wound edges immediately following debridement (the removal of foreign material and devitalised or contaminated tissue) or cleaning, whichever is appropriate. This is regardless of what is used to approximate the wound edges. Delayed closure is defined as the approximation of the wound edges at least 48 hours following debridement or cleaning, whichever is appropriate.

Types of outcome measures

Primary outcomes

1. Objective measures of wound healing, such as time to healing (where healing is defined as complete re-epithelialisation as measured by high resolution ultrasound scan, or as defined by trial authors); proportion of wounds healed within a specified time period.
2. Wound infection defined clinically as purulent discharge or erythema associated with pus (or as defined by trial authors).

Secondary outcomes

1. Number of surgical procedures.
2. Cosmetic outcome, using one or more validated cosmetic scores, such as the Cosmetic Visual Analogue Score (CVAS) or the Wound Evaluation Score (WES).
3. Death.

Search methods for identification of studies

For the search methods used in the original review see Appendix 1

Electronic searches

In May 2013, for this first update, we searched the following electronic databases to identify reports of relevant randomised clinical trials:

- Cochrane Wounds Group Specialised Register (searched 22 May 2013)
- The Cochrane Central Register of Controlled Trials (CENTRAL) (*The Cochrane Library* 2013, Issue 4)
- Ovid MEDLINE (2011 to May Week 2 2013)
- Ovid MEDLINE (In-Process & Other Non-Indexed Citations May 21, 2013)
- Ovid EMBASE (2011 to 2013 Week 20);
- EBSCO CINAHL (2011 to 17 May 2013).

We searched the Cochrane Central Register of Controlled Trials (CENTRAL) using the following MeSH headings and keywords:

#1 MeSH descriptor Wounds, Penetrating explode all trees
#2 MeSH descriptor Lacerations explode all trees
#3 MeSH descriptor Fractures, Open explode all trees
#4 (laceration* or gunshot or (gun NEXT shot) or "stab" or stabbing or stabbed):ti,ab,kw
#5 ((traumatic NEXT wound*) or (acute NEXT wound*)):ti,ab,kw
#6 ((mechanical NEXT trauma) or polytrauma):ti,ab,kw
#7 (blast or crush or avulsion) NEXT injur*:ti,ab,kw
#8 (#1 OR #2 OR #3 OR #4 OR #5 OR #6 OR #7)
#9 MeSH descriptor Wound Healing explode all trees
#10 wound NEXT closure*:ti,ab,kw
#11 wound NEXT repair*:ti,ab,kw
#12 (primary NEXT (closure* or repair*)):ti,ab,kw
#13 (secondary NEXT (closure* or repair*)):ti,ab,kw
#14 (delay* NEXT (closure* or repair*)):ti,ab,kw
#15 (#9 OR #10 OR #11 OR #12 OR #13 OR #14)
#16 (#8 AND #15)

The search strategies for Ovid MEDLINE, Ovid EMBASE and EBSCO CINAHL can be found in Appendix 2; Appendix 3 and Appendix 4 respectively. We combined the Ovid MEDLINE search with the Cochrane Highly Sensitive Search Strategy for identifying randomised trials in MEDLINE: sensitivity- and precision-maximising version (2008 revision) (Lefebvre 2011). We combined the EMBASE and CINAHL searches with the trial filters developed by the Scottish Intercollegiate Guidelines Network (SIGN 2010). There were no restrictions on the basis of date or language of publication.

Searching other resources

We searched the bibliographies of all retrieved and relevant publications identified by the above strategies for further studies.

Data collection and analysis

Selection of studies

Two review authors (ME, GB) independently assessed for inclusion all the citations identified as a result of the searches. Two review authors read the titles and abstracts to decide which should be

included in the review. There were no disagreements over which studies to include. We retrieved those citations which appeared to be eligible for inclusion in full and the two review authors further assessed them independently. The review authors themselves were not blinded as they were aware of the authors' names, publication details, institutions and results. We added those papers which did not meet the inclusion criteria to the Characteristics of excluded studies with the reason for their exclusion noted.

Data extraction and management

Data extraction was to be performed independently by two review authors, using an agreed form (see Appendix 5). We planned to resolve discrepancies through discussion or, if necessary, consulting a third person from the Wounds Group editorial base. Data were to be double-entered into Review Manager software (RevMan 5) (RevMan 2008) and checked for accuracy by one of the review authors (Dr Grace Banda). When information regarding any of the above were unclear, we planned to attempt to contact the authors of the original trial reports to provide further details.

Assessment of risk of bias in included studies

Two review authors were going to independently assess risk of bias for each study using the criteria outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2009). This tool identifies six distinct areas including sequence generation, allocation concealment, blinding of participants and outcome assessors, incomplete outcome data, selective outcome reporting and other potential threats to validity (see Appendix 6). Any disagreement would be resolved by discussion or by involving a third assessor.

We would make explicit judgements about whether studies are at high risk of bias, according to the criteria given in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2009). With reference to all six domains we would assess the likely magnitude and direction of the bias and whether we considered it likely to impact on the findings. We would explore the impact of the level of bias through undertaking sensitivity analyses (*see* Sensitivity analysis).

Measures of treatment effect

Dichotomous data

For dichotomous data, we would have presented results as summary risk ratio (RR) with 95% confidence intervals (CI).

Continuous data

For continuous data, we would have used the difference in means (MD) (with 95% CI) if outcomes were measured in the same way between trials. We we would have used the standardised mean difference (SMD) to combine trials that measured the same outcome, but used different methods.

Time-to-event data

Time to complete wound healing is time-to-event data and the most appropriate way of summarising time-to-event data would be to use methods of survival analysis and express the intervention effect as a hazard ratio. It is not appropriate to analyse time-to-event data using methods for continuous outcomes (e.g. using mean times-to-event) as the relevant times are only known for the subset of participants who have had the event. Censored participants must be excluded, which almost certainly will introduce bias.

Unit of analysis issues

We did not include cluster-randomised trials and studies with cross-over designs because these are not appropriate designs for this research question.

Dealing with missing data

We intended to note levels of attrition for included studies. We intended to explore the impact of including studies with high levels of missing data (15% or greater of the participants) in the overall assessment of treatment effect by using sensitivity analysis. We would consider studies to be at high risk of bias if they had inadequate randomisation and allocation concealment procedures and had unclear or inadequate blinding of outcome assessment.

We planned to carry out analyses for all dichotomous outcomes, as far as possible, on an intention-to-treat basis, i.e. we would attempt to include all participants randomised to each group in the analyses. The denominator for each outcome in each trial would be the number randomised minus any participants whose outcomes are known to be missing.

Assessment of heterogeneity

We planned to use the I^2 statistic to measure heterogeneity among the trials in each analysis. This examines the percentage of total variation across studies due to heterogeneity rather than to chance. If we identified substantial heterogeneity (I^2 statistic greater than 60%) and pooling was appropriate we would have used a random-effects model to pool data.

Data synthesis

We intended to carry out statistical analysis using the Review Manager software (RevMan). We would have used fixed-effect inverse variance meta-analysis for combining data where trials examined the same intervention, and the trials' populations and methods are judged sufficiently similar. Where there is clinical or methodological heterogeneity between studies sufficient to suggest that treatment effects may differ between trials we would have considered using a random-effects meta-analysis or not combining data.

Subgroup analysis and investigation of heterogeneity

We planned to carry out the following subgroup analyses if there were sufficient data:

1. acute traumatic wounds presenting within a specified time period (up to six hours, between six and up to 24 hours);
2. acute traumatic wounds with concurrent treatments such as debridement or antibiotics in comparison with wounds treated without additional intervention.

Sensitivity analysis

We had planned to use a fixed-effect meta-analysis for combining study data if the trials had been judged to be sufficiently similar. In the event of important heterogeneity (for example, I^2 greater than 60%), we would have considered using a random-effects model or not combining the data. We would have considered studies with inadequate randomisation and allocation concealment procedures

and those with unclear or inadequate blinding of outcome assessment to be at high risk of bias. For studies at high risk of bias, we had planned a sensitivity analysis omitting them.

RESULTS

Description of studies

See Characteristics of excluded studies.

Results of the search

The original search retrieved 380 citations. We excluded most of these articles on the basis of the primary topic focus being other than assessment of acute traumatic wounds presenting within 24 hours post injury. We excluded a further seven articles that had ambiguous titles after retrieving their abstracts. We obtained full-text papers for five studies for further assessment. None of these studies met the inclusion criteria of our review for varied reasons which are noted in the Characteristics of excluded studies.

For this update, the search retrieved 50 citations, none of which considered acute traumatic wounds within 24 hours of injury and none were retrieved in full.

Included studies

No studies met the inclusion criteria for the review.

Excluded studies

In the original search, we excluded studies from the review for the following reasons:

1. the outcome of interest was anal incontinence (Nordenstam 2008);
2. not a RCT, wounds involved bones (De Long 1999);
3. looked at colon repair (Kamwendo 2002);
4. looked at dog bite lacerations (Maimaris 1988); and
5. not a RCT (van de Baar 2010).

Risk of bias in included studies

Since no studies met the inclusion criteria, it was not possible to undertake a risk of bias assessment.

Effects of interventions

Since no studies met the inclusion criteria, neither a meta-analysis nor a narrative description of studies was possible.

DISCUSSION

The question of the best timing of closure of acute traumatic wounds is very important and the answer to it is long overdue. The need for evidence-based clinical guidance in this regard cannot be overemphasised considering how common the condition in question is (Hollander 1995; Hollander 1999; Silbert 1981). The finding of no studies for possible inclusion in this review is therefore very surprising.

The understanding of the mechanism of wound healing has given us an insight into the potential benefits of primary closure on the duration of wound healing (Lorenz 2008). It would therefore be expected that researchers would begin to look at the safety of extending the time post injury for allowing primary closure. So far this has only been done retrospectively in form of case studies, with conflicting results. In one study of 300 hand and forearm lacerations, Morgan et al found that lacerations closed within four hours had a lower infection rate than lacerations closed more than four hours after injury (7% versus 21%, respectively) (Morgan 1980). On the other hand, Baker and Lanuti (Baker 1990) did not find a difference in infection rate for lacerations closed less than or more than six hours from the time of injury in 2834 paediatric patients. In another study of 204 lacerations, Berk et al found that facial lacerations healed well regardless of the time to closure. In contrast, trunk and extremity lacerations had lower rates of healing if they were closed more than 19 hours after the time of injury (63% to 75%) than if they were closed earlier (75% to 91%) (Berk 1988). A more recent prospective cohort study done in the Netherlands showed that there was no significant relationship between wound age and the presence of infection after suturing. The only parameters that significantly predicted wound infection were location of the wound at the lower extremity compared to head as reference and age of the patient in the fourth quartile compared to the first quartile (van de Baar 2010).

Robust evidence of harm or benefit can only be obtained from high-quality randomised clinical trials. A randomised controlled trial has been done comparing primary closure versus delayed closure for traumatic wounds involving viscera (colon) whose management is more complex than that of traumatic wounds (Velmahos 2002). It should therefore be practically feasible to carry out a randomised clinical trial of adequate quality to answer this question. Researchers should now focus on designing good quality randomised clinical trials to provide the basis for the timing of wound closure in patients presenting with acute traumatic wounds within 24 hours of injury.

AUTHORS' CONCLUSIONS

Implications for practice

There is currently no systematic evidence to dictate a change in clinical practice regarding the timing of closure of non bite traumatic wounds within the first 24 hours of injury. Health care providers should therefore follow current practice guidelines developed based on experience. This should be done with the awareness that primary closure has the potential to increase the risk of wound infection. Delayed primary closure has the potential to increase time to healing while potentially reducing the risk of wound infection. The superiority of one intervention over the other is yet to be determined.

Implications for research

There is an urgent need for robust, randomised clinical trials to compare primary closure with delayed closure within 24 hours of injury. Given the incidence of acute traumatic wounds, it should be possible to achieve a large sample size in a single centre. Multicentre randomised clinical trials, on the other hand, would be more desirable with their added advantage of increasing external validity. We therefore recommend a multicentre randomised clinical trial with the following objectives:

- to determine the effect of primary closure compared with delayed closure for non bite traumatic wounds presenting within 24 hours post injury on time to healing; and

- to explore the adverse effects of primary closure compared with delayed closure for non bite traumatic wounds presenting within 24 hours post injury.

ACKNOWLEDGEMENTS

The South African Cochrane Centre, the UK Cochrane Centre and the Cochrane Wounds Group for their untiring support. Prof J Hofmeyr and the entire ECRU team for providing us with the skills to initiate this review. We are also grateful to Johns Hopkins Research project for providing access to the Internet and allowing Martha Eliya-Masambato take up the Aubrey Sheiham Scholarship in the UK. Wounds Group Editors: Andrew Jull, Andrea Nelson, Gill Worthy and peer referees: Ken Farion, Shirley Manknell, Humberto Saconato and Hester Vermeulen for their comments to improve the protocol. Jenny Bellorini who copy edited the review.

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CHARACTERISTICS OF STUDIES

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

Study	Reason for exclusion
De Long 1999	Not a RCT; fracture wounds
Kamwendo 2002	Looked at colon repair
Maimaris 1988	Looked at dog bite lacerations
Nordenstam 2008	The outcome of interest was anal incontinence
van de Baar 2010	Not a RCT

RCT: randomised controlled trial

APPENDICES

Appendix 1. Search methods reported in the original review (2011)

Electronic searches

We searched the following electronic databases to identify reports of relevant randomised clinical trials:

- The Cochrane Wounds Group Specialised Register (searched 14 July 2011);
- The Cochrane Central Register of Controlled Trials (CENTRAL) (*The Cochrane Library* 2011, Issue 3);
- Ovid MEDLINE (1950 to July Week 1 2011);
- Ovid MEDLINE (In-Process & Other Non-Indexed Citations, July 13, 2011);

- Ovid EMBASE (1980 to 2011 Week 27);
- EBSCO CINAHL (1982 to 14 July 2011).

We searched the Cochrane Central Register of Controlled Trials (CENTRAL) using the following MeSH headings and keywords:

#1 MeSH descriptor Wounds, Penetrating explode all trees
#2 MeSH descriptor Lacerations explode all trees
#3 MeSH descriptor Fractures, Open explode all trees
#4 (laceration* or gunshot or (gun NEXT shot) or "stab" or stabbing or stabbed):ti,ab,kw
#5 ((traumatic NEXT wound*) or (acute NEXT wound*)):ti,ab,kw
#6 ((mechanical NEXT trauma) or polytrauma):ti,ab,kw
#7 (blast or crush or avulsion) NEXT injur*:ti,ab,kw
#8 (#1 OR #2 OR #3 OR #4 OR #5 OR #6 OR #7)
#9 MeSH descriptor Wound Healing explode all trees
#10 wound NEXT closure*:ti,ab,kw
#11 wound NEXT repair*:ti,ab,kw
#12 (primary NEXT (closure* or repair*)):ti,ab,kw
#13 (secondary NEXT (closure* or repair*)):ti,ab,kw
#14 (delay* NEXT (closure* or repair*)):ti,ab,kw
#15 (#9 OR #10 OR #11 OR #12 OR #13 OR #14)
#16 (#8 AND #15)

The search strategies for Ovid MEDLINE, Ovid EMBASE and EBSCO CINAHL can be found in Appendix 1; Appendix 2 and Appendix 3 respectively. We combined the Ovid MEDLINE search with the Cochrane Highly Sensitive Search Strategy for identifying randomised trials in MEDLINE: sensitivity- and precision-maximising version (2008 revision). We combined the EMBASE and CINAHL searches with the trial filters developed by the Scottish Intercollegiate Guidelines Network. There were no restrictions on the basis of date or language of publication.

Searching other resources

We searched the bibliographies of all retrieved and relevant publications identified by the above strategies for further studies.

Appendix 2. Ovid MEDLINE search strategy

1 exp Wounds, Penetrating/
2 exp Lacerations/
3 exp Fractures, Open/
4 (laceration* or gunshot or gun shot or stab or stabbing or stabbed).tw.
5 (traumatic wound* or acute wound*).tw.
6 (mechanical trauma or polytrauma).tw.
7 ((blast or crush or avulsion) adj injur*).tw.
8 1 or 2 or 3 or 4 or 5 or 6 or 7
9 exp Wound Healing/
10 (wound* adj3 clos*).tw.
11 (wound* adj3 repair*).tw.
12 (primary adj3 (closure* or repair*)).tw.
13 (secondary adj3 (closure* or repair*)).tw.
14 (delay* adj3 (closure* or repair*)).tw.
15 9 or 10 or 11 or 12 or 13 or 14
16 8 and 15

Appendix 3. Ovid EMBASE search strategy

1 exp penetrating trauma/
2 exp laceration/
3 exp open fracture/
4 exp gunshot injury/
5 exp stab wound/
6 (laceration* or gunshot or gun shot or stab or stabbing or stabbed).tw.
7 (traumatic wound* or acute wound*).tw.
8 (mechanical trauma or polytrauma).tw.
9 ((blast or crush or avulsion) adj injur*).tw.
10 or/1-9
11 exp wound healing/
12 (wound* adj3 clos*).tw.

13 (wound* adj3 repair*).tw.
14 (primary adj3 (closure* or repair*)).tw.
15 (secondary adj3 (closure* or repair*)).tw.
16 (delay* adj3 (closure* or repair*)).tw.
17 or/11-16
18 10 and 17

Appendix 4. EBSCO CINAHL search strategy

S17 S9 and S16
S16 S10 or S11 or S12 or S13 or S14 or S15
S15 TI (delay* N3 closure* or delay* N3 repair*) or AB (delay* N3 closure* or delay* N3 repair*)
S14 TI (secondary N3 closure* or secondary N3 repair*) or AB (secondary N3 closure* or secondary N3 repair*)
S13 TI (primary N3 closure* or primary N3 repair*) or AB (primary N3 closure* or primary N3 repair*)
S12 TI wound* N3 repair* or AB wound* N3 repair*
S11 TI wound* N3 clos* or AB wound* N3 clos*
S10 (MH "Wound Healing+")
S9 S1 or S2 or S3 or S4 or S5 or S6 or S7 or S8
S8 TI (blast injur* or crush injur* or avulsion injur*) or AB (blast injur* or crush injur* or avulsion injur*)
S7 TI (mechanical trauma or polytrauma) or AB (mechanical trauma or polytrauma)
S6 TI (traumatic wound* or acute wound*) or AB (traumatic wound* or acute wound*)
S5 TI (laceration* or gunshot or gun shot or stab or stabbing or stabbed) or AB (laceration* or gunshot or gun shot or stab or stabbing or stabbed)
S4 (MH "Avulsion Fractures")
S3 (MH "Fractures, Open")
S2 (MH "Tears and Lacerations")
S1 (MH "Wounds, Penetrating+")

Appendix 5. Data extraction sheet

Part 1: Review, reviewer and study information	
Review title:	Primary closure versus delayed primary closure for non bite traumatic wounds presenting within 24 hours of injury
Reviewer:	Grace Banda / Martha Eliya / Other (specify)
Date of completion of this form:	Year / Month / Day
Study ID:	Author(s):
Title of report:	
Year of publication:	
Source:	
Language of publication:	English / French / German / Chinese / Other (specify)
Type of report:	Full paper / Abstract / Unpublished
Number of centres:	Single centre / multicentre (if multicentre, number of centres)
Date trial was conducted:	
Country where study performed:	
Funders of the trial:	
Part 2: Eligibility of study for review	
Study type:	Is the study a randomised controlled trial? Yes / No / Unclear
Participants:	Are the participants people with non bite traumatic wounds Yes / No / Unclear Are the wounds presenting within 24 hours of injury? Yes / No / Unclear Are the wounds involving only soft tissues (skin, muscle and their neurovascular structures)? Yes / No / Unclear Are the wounds clinically free of infection at presentation? Yes / No / Unclear
Intervention:	Does the study evaluate primary closure versus delayed primary closure? Yes / No / Unclear
Outcome:	Does the study report time from institution of intervention to wound healing? Yes / No / Unclear **AND/OR** Does the study report infection rate? Yes / No / Unclear

(Continued)

Part 3: Description of the study

Characteristics of the participants				
Inclusion criteria:				
Exclusion criteria:				
		Primary closure	**Delayed closure**	**TOTAL**
Number of participants:				
Mean age (SD) (range):				
Sex (F/M):				
Participants excluded from the study after randomisation:		**Number:**	**Number:**	**Number:**
		Reason:	**Reason:**	**Reason:**
Participants excluded from the study before randomisation:				
Withdrawals:	**Reason:**	**Number:**	**Number:**	**Number:**
		Reason:	**Reason:**	**Reason:**
Unit of randomisation:		Person / Wound		
Sample size calculation:		Yes / No / Unclear		
Types of interventions		**Primary closure**	**Delayed closure**	
What was done?				
Time taken from injury to intervention:		0 hours up to 6 hours 6 hours up to 12 hours 12 hours up to 18 hours 18 hours to 24 hours Mean time taken in hours	0 hours up to 6 hours 6 hours up to 12 hours 12 hours up to 18 hours 18 hours to 24 hours Mean time taken in hours	
Setting of intervention:		Emergency department	Emergency department	

(Continued)

	Operating theatre Other (specify)	Operating theatre Other (specify)	
Person carrying out the intervention:	Emergency physician Surgeon Junior doctor Specialist nurse Other (specify)	Emergency physician Surgeon Junior doctor Specialist nurse Other (specify)	
Antibiotics given:	Yes / No / Unclear	Yes / No / Unclear	
How long antibiotics given in hours:			
Non surgical debridement carried out:	Yes / No / Unclear	Yes / No / Unclear	
What form of non surgical debridement?			
How long non surgical debridement carried out in hours:			
Length of follow up:			
	Primary closure	**Delayed closure**	**TOTAL**
Outcome measures:			
Primary outcome description:		**Scale/measure (definition of how was it measured)**	
Time to healing:			
Number of wounds healed:			
Rates of infection:			
Other (specify):			
Secondary outcome description:		**Scale/measure (definition/description of how was it measured)**	
Number of surgical procedures :			
Mean Cosmetic Visual Analogue Score (CVAS):			

(Continued)

Wound Evaluation Score (WES):			
Death:			
Other (specify):			
Results (use dichotomous or continuous table):			
Dichotomous data table:			
Outcome	**Primary closure** Number/total number (%)	**Delayed closure** Number/total number (%)	Total Number/total number (%)
Number of wounds healed			
Infection			
Death			
Number of surgical procedures:			
Other (specify):			
Continuous data (Scale):			
Outcome	**Primary closure** (mean and SD)	**Delayed closure** (mean and SD)	Total (mean and SD)
Time to healing:			
CVAS:			
Risk of bias assessment:			
Method of randomisation - was the allocation sequence randomly generated?		Low risk - truly random (random numbers, coin toss, shuffle etc) High risk - quasi-random (patient number, date of birth) Not stated / unclear ***Describe***	
Allocation concealment - was the treatment allocation adequately concealed ?		Low risk (central allocation at trials office, sequentially numbered or coded envelopes, other methods where the trialist allocating treatment could not be aware of the treatment)	

(Continued)

	High risk (allocation was alternate – by patient, day of the week, admission ward etc) or based on information, such as date of birth, already known to the trialist Unclear (inadequate information given) ***Describe***
Blinding - was knowledge of the allocated interventions adequately prevented during the study?	Low risk / high risk / Unclear ***describe***
Participants:	Low risk / high risk / Unclear ***describe***
Personnel	Low risk / high risk / Unclear ***describe***
Outcome assessors (time to healing):	Low risk / high risk / Unclear ***describe***
Outcome assessors (infection):	Low risk / high risk / Unclear ***describe***
Outcome assessors (cosmetic outcome):	Low risk / high risk / Unclear ***describe***
Blinding of participants, personnel and outcome assessors *(Assessments should be made for each main outcome (or class of outcomes)):*	
Were incomplete outcome data adequately addressed?	Low risk / high risk / Unclear ***describe***
Was the drop-out rate described and acceptable? Extent of loss to follow up	
Was the drop-out rate described?	Yes / No ***describe***
Was the drop-out rate acceptable?	Yes / No ***describe***
What was the extent of loss to follow up?	
Use of intention-to-treat analysis	Low risk / high risk / Unclear ***describe***
Are reports of the study free of suggestion of selective outcome reporting? *(Explain)*	Low risk / high risk / Unclear ***describe***
Other sources of potential bias: ***For example: a*re groups balanced at baseline for variables such as wound size and duration?**	Low risk / high risk / ***describe***
Is further information required from the authors?	Yes / No

(Continued)

Give details of information required:

Date:___/___/2010	**Reviewer's signature**

Appendix 6. Risk of bias

1. Was the allocation sequence randomly generated?

Low risk of bias

The investigators describe a random component in the sequence generation process such as: referring to a random number table; using a computer random number generator; coin tossing; shuffling cards or envelopes; throwing dice; drawing of lots.

High risk of bias

The investigators describe a non-random component in the sequence generation process. Usually, the description would involve some systematic, non-random approach, for example: sequence generated by odd or even date of birth; sequence generated by some rule based on date (or day) of admission; sequence generated by some rule based on hospital or clinic record number.

Unclear

Insufficient information about the sequence generation process to permit judgement of low or high risk of bias.

2. Was the treatment allocation adequately concealed?

Low risk of bias

Participants and investigators enrolling participants could not foresee assignment because one of the following, or an equivalent method, was used to conceal allocation: central allocation (including telephone, web-based and pharmacy-controlled randomisation); sequentially-numbered drug containers of identical appearance; sequentially-numbered, opaque, sealed envelopes.

High risk of bias

Participants or investigators enrolling participants could possibly foresee assignments and thus introduce selection bias, such as allocation based on: using an open random allocation schedule (e.g. a list of random numbers); assignment envelopes were used without appropriate safeguards (e.g. if envelopes were unsealed or non opaque or not sequentially numbered); alternation or rotation; date of birth; case record number; any other explicitly unconcealed procedure.

Unclear

Insufficient information to permit judgement of low or high risk of bias. This is usually the case if the method of concealment is not described or not described in sufficient detail to allow a definite judgement, for example if the use of assignment envelopes is described, but it remains unclear whether envelopes were sequentially numbered, opaque and sealed.

3. Blinding - was knowledge of the allocated interventions adequately prevented during the study?

Low risk of bias

Any one of the following.

- No blinding, but the review authors judge that the outcome and the outcome measurement are not likely to be influenced by lack of blinding.
- Blinding of participants and key study personnel ensured, and unlikely that the blinding could have been broken.
- Either participants or some key study personnel were not blinded, but outcome assessment was blinded and the non-blinding of others unlikely to introduce bias.

High risk of bias

Any one of the following.

- No blinding or incomplete blinding, and the outcome or outcome measurement is likely to be influenced by lack of blinding.
- Blinding of key study participants and personnel attempted, but likely that the blinding could have been broken.
- Either participants or some key study personnel were not blinded, and the non-blinding of others likely to introduce bias.

Unclear

Any one of the following.

- Insufficient information to permit judgement of low or high risk of bias.
- The study did not address this outcome.

4. Were incomplete outcome data adequately addressed?

Low risk of bias

Any one of the following.

- No missing outcome data.
- Reasons for missing outcome data unlikely to be related to true outcome (for survival data, censoring unlikely to be introducing bias).
- Missing outcome data balanced in numbers across intervention groups, with similar reasons for missing data across groups.
- For dichotomous outcome data, the proportion of missing outcomes compared with observed event risk not enough to have a clinically relevant impact on the intervention effect estimate.
- For continuous outcome data, plausible effect size (difference in means or standardised difference in means) among missing outcomes not enough to have a clinically relevant impact on observed effect size.
- Missing data have been imputed using appropriate methods.

High risk of bias

Any one of the following.

- Reason for missing outcome data likely to be related to true outcome, with either imbalance in numbers or reasons for missing data across intervention groups.
- For dichotomous outcome data, the proportion of missing outcomes compared with observed event risk enough to induce clinically relevant bias in intervention effect estimate.
- For continuous outcome data, plausible effect size (difference in means or standardised difference in means) among missing outcomes enough to induce clinically relevant bias in observed effect size.
- 'As-treated' analysis done with substantial departure of the intervention received from that assigned at randomisation.
- Potentially inappropriate application of simple imputation.

Unclear

Any one of the following.

- Insufficient reporting of attrition/exclusions to permit judgement of low or high risk of bias (e.g. number randomised not stated, no reasons for missing data provided).
- The study did not address this outcome.

5. Are reports of the study free of suggestion of selective outcome reporting?

Low risk of bias

Any of the following.

- The study protocol is available and all of the study's pre-specified (primary and secondary) outcomes that are of interest in the review have been reported in the pre-specified way.
- The study protocol is not available but it is clear that the published reports include all expected outcomes, including those that were pre-specified (convincing text of this nature may be uncommon)

High risk of bias

Any one of the following.

- Not all of the study's pre-specified primary outcomes have been reported.
- One or more primary outcomes is reported using measurements, analysis methods or subsets of the data (e.g. subscales) that were not pre-specified.
- One or more reported primary outcomes were not pre-specified (unless clear justification for their reporting is provided, such as an unexpected adverse effect).
- One or more outcomes of interest in the review are reported incompletely so that they cannot be entered in a meta-analysis.
- The study report fails to include results for a key outcome that would be expected to have been reported for such a study.

Unclear

Insufficient information to permit judgement of low or high risk of bias. It is likely that the majority of studies will fall into this category.

6. Other sources of potential bias

Low risk of bias

The study appears to be free of other sources of bias.

High risk of bias

There is at least one important risk of bias. For example, the study:

- had a potential source of bias related to the specific study design used; or
- had extreme baseline imbalance; or
- has been claimed to have been fraudulent; or
- had some other problem.

Unclear

There may be a risk of bias, but there is either:

- insufficient information to assess whether an important risk of bias exists; or
- insufficient rationale or evidence that an identified problem will introduce bias.

WHAT'S NEW

Date	Event	Description
1 August 2013	New search has been performed	First update, new search,
1 August 2013	New citation required but conclusions have not changed	No new studies identified, conclusions unchanged

CONTRIBUTIONS OF AUTHORS

Martha Eliya-Masamba and Grace Banda both conceived the review question, developed the protocol, secured funding and completed the first draft of the protocol. They both edited the protocol, made an intellectual contribution to the protocol, advised on the protocol, and approved the final version of the protocol prior to submission. They both reviewed papers for inclusion and wrote the review. They are both guarantors of the work.

Contributions of editorial base:
Nicky Cullum: edited the protocol; advised on methodology, interpretation and protocol content. Approved the final protocol and review prior to submission.
Sally Bell-Syer: co-ordinated the editorial process. Advised on methodology, interpretation and content. Edited and copy edited the protocol, review and updated review.
Ruth Foxlee: designed the search strategy and edited the search methods section and ran the searches.

DECLARATIONS OF INTEREST

None known.

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Internal sources

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- NIHR / Department of Health (England), (Cochrane Wounds Group), UK.

INDEX TERMS

Medical Subject Headings (MeSH)

*Wound Closure Techniques; Acute Disease; Emergency Treatment [*methods]; Time Factors; Wound Infection [etiology]; Wounds and Injuries [*surgery]

MeSH check words

Humans